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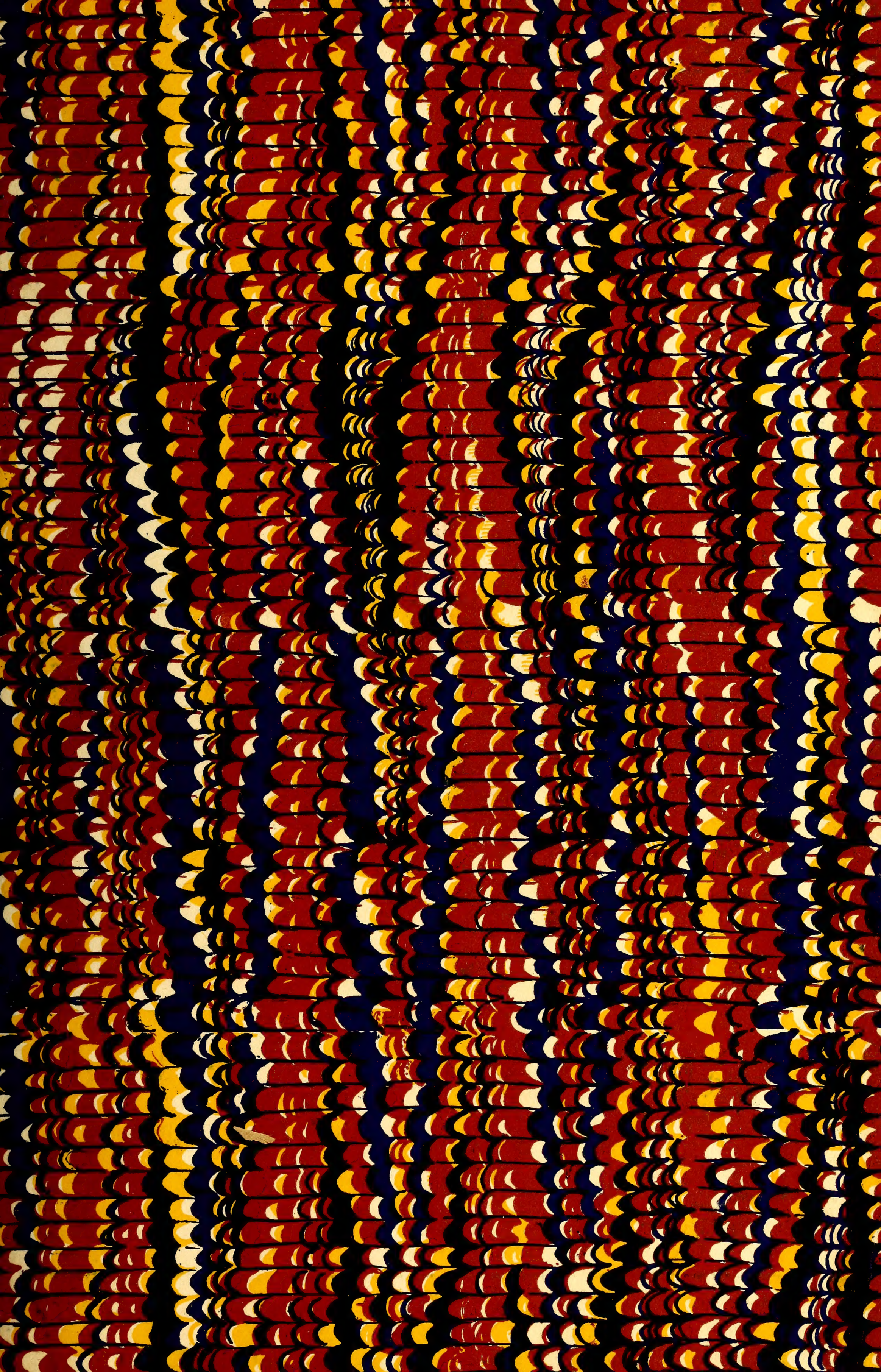

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VOLUME 6, 1920

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CONTENTS

PAGE

Officers and Members of the Academy, November 16, 1920 ix

Report of the Autumn Meeting, 1919, of the National Academy of Sciences 43

Report of the Annual Meeting, 1920, of the National Academy of Sciences 442

The Kilauea Volcano Observatory *Committee Report* 706

Errata 716

Index 717

NATIONAL RESEARCH COUNCIL

The Larger Opportunities for Research on the Relations of Solar and Terrestrial Radiation *By C. G. Abbot* 82

Notes from the Minutes of the Meetings in January and February, 1920, of the Executive Board and Interim Committee of the National Research Council 335

Report of the Meetings of the International Research Council and of the Affiliated Unions Held at Brussells, July 18–28, 1919 340

Report on the Organization of the International Astronomical Union 349

A Survey of Research Problems in Geophysics 545

- Present Status of Geodesy and Some Problems of This Branch of Geophysics *By William Bowie* 545
- The Problems of Seismology *By Harry Fielding Reid* 555
- The Status and Problems of Meteorology *By C. F. Marvin* 561
- Some of the Chief Problems in Terrestrial Magnetism and Electricity *By Louis A. Bauer* 572
- The Problems and Functions of the Section of Physical Oceanography of the American Geophysical Union *By G. W. Littlehales* 580
- The Problems of Volcanology *By Henry W. Washington* 583
- An Outline of Geophysical Chemical Problems *By Robert S. Sosman* 592

MATHEMATICS

The Commutativity of One-Parameter Transformations in Real Variables *By A. C. Lunn* 24

Groups Generated by Two Operators, s_1, s_2, Which Satisfy the Conditions $s_1^m = s_2^n$, $(s_1s_2)^k = 1$, $s_1s_2 = s_2s_1$ *By G. A. Miller.* 70

Note on Geometrical Products *By C. L. E. Moore and H. B. Phillips* 155

The Starting of a Ship *By James K. Whittemore* 182

A Thermodynamic Study of Electrolytic Solutions *By F. L. Hitchcock* 186

Functionals Invariant under One-Parameter Continuous Groups of Transformations in the Space of Continuous Functions *By I. A. Barnett* 200

On Kummer's Memoir of 1857 Concerning Fermat's Last Theorem *By H. S. Vandiver* 266

On the Distortion in Conformal Mapping when the Second Coefficient in the Mapping Function has an Assigned Value *By T. H. Gronwall* 300

Conformal Mapping of a Family of Real Conics on Another *By T. H. Gronwall* 312

On the Class Number of the Field $\Omega(e^{2i\pi/p^n})$ and the Second Case of Fermat's Last Theorem *By H. S. Vandiver* 416

PAGE
A NEW PROOF OF A THEOREM DUE TO SCHOENFLIES...........*By J. R. Kline* 529
ON THE STRUCTURE OF FINITE CONTINUOUS GROUPS WITH EXCEPTIONAL TRANSFORMATIONS...*By S. D. Zeldin* 541
MOTION ON A SURFACE FOR ANY POSITIONAL FIELD OF FORCE..*By Joseph Lipka*. 621
SEMINVARIANTS OF A GENERAL SYSTEM OF LINEAR HOMOGENEOUS DIFFERENTIAL EQUATIONS...*By E. B. Stouffer* 645
THE PERMANENT GRAVITATIONAL FIELD IN THE EINSTEIN THEORY .... *By L. P. Eisenhart* 678
AN APPLICATION OF THE PORISM OF FOUR TANGENTS OF A TWISTED CUBIC........ *By J. H. McDonald* 702

STATISTICS

ON THE RATE OF GROWTH OF THE POPULATION OF THE UNITED STATES SINCE 1790 AND ITS MATHEMATICAL REPRESENTATION................................ *By Raymond Pearl and Lowell J. Reed* 275
ON A SINGLE NUMERICAL INDEX OF THE AGE DISTRIBUTION OF A POPULATION.... *By Raymond Pearl* 427
A SIMPLIFIED METHOD FOR THE STATISTICAL INTERPRETATION OF EXPERIMENTAL DATA..*By George A. Linhart* 682
A CRITICAL STUDY OF FERTILIZER EXPERIMENTS............................ *By C. B. Lipman and G. A. Linhart* 684

ASTRONOMY

A NEW METHOD OF DETERMINING THE SOLAR CONSTANT OF RADIATION *By C. G. Abbot* 4
THE LARGER OPPORTUNITIES FOR RESEARCH ON THE RELATIONS OF SOLAR AND TERRESTRIAL RELATION...................................*By C. G. Abbot* 82
A NOTE ON A SIMPLE DEVICE FOR INCREASING THE PHOTOGRAPHIC POWER OF LARGE TELESCOPES...................................*By Harlow Shapley* 127
STUDIES OF MAGNITUDES IN STAR CLUSTERS, XI. FREQUENCY CURVES OF THE ABSOLUTE MAGNITUDE AND COLOR INDEX FOR 1152 GIANT STARS.......... *By Harlow Shapley* 293
REPORT ON THE ORGANIZATION OF THE INTERNATIONAL ASTRONOMICAL UNION.... *By W. W. Campbell and Joel Stebbins* 349
STUDIES OF MAGNITUDES IN STAR CLUSTERS, XII. SUMMARY OF A PHOTOMETRIC INVESTIGATION OF THE GLOBULAR SYSTEM MESSIER 3.................. *By Harlow Shapley and Helen N. Davis* 486
NEW OBSERVATIONS ON THE VARIABILITY OF THE SUN............*By C. G. Abbot* 674

PHYSICS

AN EXAMPLE OF TORSIONAL VISCOUS RETROGRESSION...........*By Carl Barus* 2
THE INTENSITIES OF X-RAYS OF THE L-SERIES, II. THE CRITICAL POTENTIALS OF THE PLATINUM LINES...........................*By D. L. Webster* 26
SPECTRA OF EXPLOSIONS.................................*By J. A. Anderson* 42
THE TORSIONAL MAGNETIC ENERGY ABSORPTION OF AN IRON CONDUCTOR........ *By Carl Barus* 59
A STUDY OF ABSORPTION SPECTRA WITH THE ELECTRIC FURNACE................ *By Arthur S. King* 63
A STUDY OF THE EFFECT OF A MAGNETIC FIELD ON ELECTRIC FURNACE SPECTRA.. *By Arthur S. King* 65
A STATISTICAL METHOD FOR STUDYING THE RADIATIONS FROM RADIOACTIVE SUBSTANCES AND THE X-RAYS AND ITS APPLICATION TO SOME γ-RAY PROBLEMS.. *By Alois F. Kovarik* 105

PAGE
ON THE NATURE OF THE HEAT PRODUCTION IN A SYSTEM OF PLATINUM BLACK, ALCOHOL AND AIR..*By L. B. Loeb* 107
A KINEMATICAL INTERPRETATION OF ELECTROMAGNETISM.........*By Leigh Page* 115
THE LAWS OF ELASTICO-VISCOUS FLOW, II................*By A. A. Michelson* 122
INFERENCES FROM THE HYPOTHESIS OF DUAL ELECTRIC CONDUCTION; THE THOMSON EFFECT..*By Edwin H. Hall* 139
THE FUNCTIONS OF INTENSITY AND PHASE IN THE BINAURAL LOCATION OF PURE TONES..*By G. W. Stewart* 166
THE MEASUREMENT OF SMALL TIME INTERVALS AND SOME APPLICATIONS, PRINCIPALLY BALLISTIC.....*By L. T. E. Thompson, C. N. Hickman, and N. Riffolt* 169
CHARCOAL ACTIVATION...................................*By H. H. Sheldon* 178
THE EXISTENCE OF HOMOGENEOUS GROUPS OF LARGE IONS.....*By O. Blackwood* 253
AN IMPROVED FORM OF HIGH TENSION D. C. APPARATUS.....*By D. L. Webster* 269
ON THE SPRINGFIELD RIFLE AND THE LEDUC FORMULA.......................
By Arthur Gordon Webster 289
ON THE CONNECTION OF THE SPECIFIC HEATS WITH THE EQUATION OF A STATE OF A GAS......................................*By Arthur Gordon Webster* 302
A PHOTOGRAPHIC METHOD OF FINDING THE INSTANTANEOUS VELOCITY OF SOUND WAVES AT POINTS NEAR THE SOURCE..................*By Arthur L. Foley* 310
EXPERIMENTS ON THE VIBRATION OF AIR IN CONICAL HORNS.................
By A. G. Webster 316
ON THE NATURE OF THE NEGATIVE CARRIERS PRODUCED IN PURE HYDROGEN AND NITROGEN BY PHOTO-ELECTRONS.......................*By Leonard B. Loeb* 435
SHOCK OR WATER RAM IN PIPE LINES WITH IMPERFECT REFLECTION AT THE DISCHARGE END AND INCLUDING THE EFFECTS OF FRICTION AND NON-UNIFORM CHANGE OF VALVE OPENING........................*By W. F. Durand* 441
THE VERTICAL INTERFEROMETER..........................*By A. A. Michelson* 473
ON THE APPLICATION OF INTERFERENCE METHODS TO ASTRONOMICAL MEASUREMENTS...*By A. A. Michelson* 474
A MODIFICATION OF THE REVOLVING MIRROR METHOD FOR MEASURING THE VELOCITY OF LIGHT..............................*By A. A. Michelson* 476
ON THE K. SERIES OF THE X-RAYS..
By William Duane and Wilhelm Stenström 477
THE POSSIBILITIES OF THE ROCKET IN WEATHER FORECASTING...............
By Robert H. Goddard 493
NOTE ON A PNEUMATIC METHOD OF MEASURING VARIATIONS OF THE ACCELERATION OF GRAVITY.......................................*By Carl Barus* 495
NOTE ON TORSIONAL MEASUREMENT OF VARIATIONS OF THE ACCELERATION OF GRAVITY BY INTERFERENCE METHODS.....................*By Carl Barus* 498
FURTHER MEASUREMENTS OF THE EFFECTS OF PRESSURE ON RESISTANCE........
By P. W. Bridgman 505
CHARACTERISTIC ABSORPTION OF X-RAYS: L SERIES.........................
By William Duane and R. A. Patterson 509
ON THE RELATIVE POSITIONS AND INTENSITIES OF LINES IN X-RAY SPECTRA......
By William Duane and R. A. Patterson 518
ON A DIFFERENTIAL EQUATION OCCURRING IN PAGE'S THEORY OF ELECTROMAGNETISM...*By H. Bateman* 528
ON A CONDITION FOR HELMHOLTZ'S EQUATION SIMILAR TO LAMÉ'S............
By Arthur Gordon Webster 605
THE ABSORPTION OF X-RAYS BY CHEMICAL ELEMENTS OF HIGH ATOMIC NUMBERS
By William Duane, Hugo Fricke and Wilhelm Stenström 607

PAGE

THE THOMSON EFFECT AND THERMAL CONDUCTION IN METALS.................. *By Edwin H. Hall* 613

THE INTENSITIES OF X-RAYS OF THE L-SERIES, III. CRITICAL POTENTIALS OF THE PLATINUM AND TUNGSTEN LINES ....*By Frank C. Hoyt* 639

SOME NEW METHODS IN INTERIOR BALLISTICS..........*By Arthur Gordon Webster* 648

LUMINESCENCE AT HIGH TEMPERATURES.......*By E. L. Nichols and D. T. Wilbur* 693

EXPERIMENTS WITH THE TUBE RESISTANCE FURNACE ON THE EFFECT OF POTENTIAL DIFFERENCE..*By Arthur S. King* 701

CHEMISTRY

THE CHEMICAL ISOLATION OF VITAMINES.......*By C. N. Myers and C. Voegtlin* 3

THE NATURE AND FUNCTION OF THE ANTINEURITIC VITAMINE .*By R. Adams Dutcher* 10

THE IONIZATION AND ACTIVITY OF LARGELY IONIZED SUBSTANCES .... *By A. A. Noyes and D. A. MacInnes* 18

THE PROTEIN REQUIREMENT OF MAINTENANCE IN MAN......*By H. C. Sherman* 38

AN APPARENT HIGH PRESSURE DUE TO ADSORPTION, AND HEAT OF ADSORPTION, AND THE DENSITY OF GAS-MASK CHARCOALS......................... *By William D. Harkins and D. T. Ewing* 49

THE INFLUENCE OF IONS ON THE OSMOTIC PRESSURE OF SOLUTIONS......... *By Jacques Loeb* 211

THE RELATION BETWEEN THE ISO-ELECTRIC POINT OF A GLOBULIN AND ITS SOLUBILITY AND ACID-COMBINING CAPACITY IN SALT SOLUTION...*By E. J. Cohn* 256

ALKYL AMIDES OF ISOTHIOCYANACETIC ACID.................................. *By Treat B. Johnson, Arthur J. Hill and E. B. Kelsey* 290

ANOMALOUS OSMOSE...*By F. E. Bartell* 306

ANALYTICAL NOTE ON CERTAIN RHYTHMIC RELATIONS IN ORGANIC SYSTEMS..... *By Alfred J. Lotka* 410

THE IONIZATION OF STRONG ELECTROLYTES.............*By William D. Harkins* 601

HYDROGEN ION CONCENTRATION OF THE CONTENTS OF THE SMALL INTESTINE...... *By J. F. McClendon* 690

GEOLOGY AND PALEONTOLOGY

RECONSTRUCTION OF THE SKELETON OF THE SAUROPOD DINOSAUR CAMARASAURUS COPE (MOROSAURUS MARSH).............*By H. F. Osborn and C. C. Mook* 15

RESTORATION OF CAMARASAURUS AND LIFE MODEL..........*By W. K. Gregory* 16

PLATO'S ATLANTIS IN PALAEOGEOGRAPHY....................*By W. D. Matthew* 17

DIFFERENTIATION BY DEFORMATION........................*By N. L. Bowen* 159

A GENERAL SINKING OF SEA-LEVEL IN RECENT TIME....*By Reginald A. Daly* 246

FOSSIL PLANTS FROM THE LATE CRETACEOUS OF TENNESSEE................. *By Edward W. Berry* 333

ON CHONDRULES AND CHONDRITIC STRUCTURE IN METEORITES................ *By George P. Merrill* 449

THE KILAUEA VOLCANO OBSERVATORY...........*By a Committee of the Academy* 706

BOTANY

THE FORMATION OF THE CELL PLATE IN THE CAMBIUM OF THE HIGHER PLANTS.... *By Irving W. Bailey* 197

THE INFLUENCE OF COLD IN STIMULATING THE GROWTH OF PLANTS........... *By Frederick V. Coville* 434

THE GENUS BOTRYCHIUM AND ITS RELATIONSHIPS..........*By D. H. Campbell* 502

ZOOLOGY

THE TRANSPLANTING OF SEA ANEMONES BY HERMIT CRABS....*By R. P. Cowles* 40

PAGE

The Development of Connective Tissue in the Amphibian Embryo........ *By George A. Baitsell* 77

On the Hyperplasia of Nerve Centers Resulting from Excessive Peripheral Loading........ *By S. R. Detwiler* 96

Evidence on the Nature of Nuclear Activity........ *By D. S. Tennent* 217

The Effect of the Concentration of Nitrates on the Reducing Powers of Bacteria........ *By R. W. Glaser* 272

Studies on the Transformation of the Intestinal Flora, with Special Reference to the Implantation of Bacillus Acidophilus, I. Feeding Experiments with Albino Rats... *By Harry A. Cheplin and Leo F. Rettger* 423

Upon an Important Method of Studying Problems of Relationship and of Geographical Distribution........ *By Maynard M. Metcalf* 432

Preliminary Report on Pterergates in Pogonomyrmex Californicus...... *By Harlow Shapley* 687

Functional Regulations in Animals with Composite Spinal Cords.......... *By S. R. Detwiler* 695

Studies on the Transformations of the Intestinal Flora, with Special Reference to the Implantation of Bacillus Acidophilus, II. Feeding Experiments on Man........ *By Harry A. Cheplin and Leo F. Rettger* 704

GENETICS

Selective Fertilization in Pollen Mixtures........ *B. D. F. Jones* 66

Model of the Linkage System of Eleven Second Chromosome Genes of Drosophila........ *By W. E. Castle* 73

Transmission of Eye-Defects Induced in Rabbits by Means of Lens-Sensitized Fowl-Serum........ *By M. F. Guyer and E. A. Smith* 134

The Evidence for the Linear Order of the Genes........ *By T. H. Morgan, A. H. Sturtevant, and C. B. Bridges*.. 162

The Arrangement of Genes in Drosophila Virilis........ *By C. W. Metz* 164

A Contribution of Genetics to the Practical Breeding of Dairy Cattle.... *By R. Pearl* 225

Calculating Ancestral Influence in Man........ *By Harry H. Laughlin* 235

A Note on the Human Sex Ratio........ *By C. C. Little* 250

The Relative Importance of Heredity and Environment in Determining the Piebald Pattern of Guinea-Pigs........ *By Sewall Wright* 320

Observations on the Sterility of Mutant Hybrids in Drosophila Virilis *By C. W. Metz* 421

Homologous Genes and Linear Linkage in Drosophila Virilis........ *By Alexander Weinstein* 625

The Mutant Crossveinless in Drosophila Melanogaster........ *By Calvin B. Bridges* 660

Is Crossing Over a Function of Distance?........ *By J. A. Detlefsen* 663

Interspecific Hybrids in Crepis........ *By E. B. Babcock and J. L. Collins* 670

PHYSIOLOGY AND PATHOLOGY

The Basal Metabolism of Boys from 1 to 13 Years of Age........ *By Francis G. Benedict* 7

The Effect of Physical Agents on the Resistance of Mice to Cancer...... *By J. S. Murphy* 35

The Adjustment to the Barometer of the Hemato-Respiratory Functions in Man........ *By Yandell Henderson* 62

Calcium and Magnesium Metabolism in Certain Diseases........ *By Frank P. Underhill, James A. Honeij and L. Jean Bogert* 79

PAGE
Encephalitis and Poliomyelitis ... *By Simon Flexner* 103
Leptospira Icteroides and Yellow Fever ... *By Hideyo Noguchi* 110
Human Retinal Adaptation ... *By Selig Hecht* 112
On the Mechanism of Fever Reduction by Drugs ... *By Henry G. Barbour and Julian B. Hermann* 136
Thermokinetics of Liometopum Apiculatum Mayr ... *By Harlow Shapley* 204
The Effect of Anesthetics on Living Cells ... *By Grade Medes and J. F. McClendon* 243
The Carbon-Dioxide: Heat Ratio in Cattle ... *By H. P. Armsby, J. August Fries, and Winifred W. Braman* 263
The Dynamics of a Fluctuating Growth Rate ... *By H. S. Reed* 397
Are the Superior Cervical Ganglia Indispensable to the Maintenance of Life? ... *By S. J. Meltzer* 532
Observations on the Body Temperature of Dry Cows ... *By Max Kriss* 539
Blood Concentration Changes in Influenza ... *By Frank P. Underhill and Michael Ringer* 692

ANTHROPOLOGY AND PSYCHOLOGY

Central American Calendars and the Gregorian Day ... *By Herbert J. Spinden* 56
Anthropometry of the Siouan Tribes ... *By Louis R. Sullivan* 131
A Study of the Persistence of Vision ... *By A. C. Hardy* 221
The Influence of Environment upon Development ... *By Franz Boas* 489

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AN EXAMPLE OF TORSIONAL VISCOUS RETROGRESSION

By Carl Barus

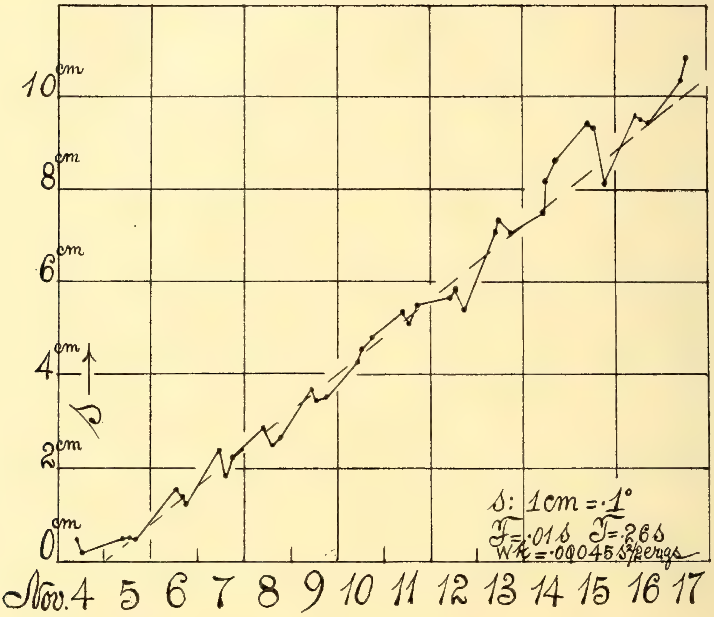
Department of Physics, Brown University

Communicated November 22, 1919

In order to determine to what degree it would be possible to eliminate radiant forces, I have recently installed a gravitational needle of the usual form, in a glass case, but with the balls at the end as heavy as the torsion wire would safely carry. The latter was hand-drawn (music) wire 0.022 cm. in diameter and 154 cm. long, hanging from a torsion head attached to the pier. The balls were each over 1500 grams in mass and 29.3 cm. apart, between centers. The torsion coefficient of the system was found to be 154. The deflections of an externally attracting mass (also 1500 grams) were read off with mirror and telescope at a distance of 290 cm. This makes the force to be measured $F = 0.009s$, if s is the deflection observed in cm. The period of the system would be about 14 minutes. As the two equal masses may be approached within less than 6 cm. of each other (between centers), a gravitational force of about 4.2×10^{-3} dynes is to be registered and this would be equivalent to a deflection of $s = 0.47$ cm. Hence, by commutation, a total deflection of about 1 cm. may be expected, which could be much increased by increasing the masses ($M = 1500$ g.) on the outside and inside of the case. On the quadratic interferometer with achromatic fringes, the deflection s instanced, would be equivalent to a normal displacement of mirror $\Delta N = 0.023$ cm., readable to 10^{-4} cm. directly, or to 4×10^{-5} per fringe. So far as mere observation is concerned, the reading could be adequately made within 0.1%. I was in hopes, therefore, that the incipient motion of the needle, since air resistance, etc., is here negligible, could be studied as a case of uniformly varied motion.

Observations.—As these were made in a room, at first heated mildly and eventually more vigorously (see curve) they are useless for gravitational work. They are interesting, however, in their bearing on Maxwell's theory of viscosity, as they give evidence of a concealed torsional strain, gradually but continuously vanishing; or of a spontaneous tendency of the wire to regain the original molecular condition, free from strain. An example of the observation taken in November is given in the figure, and it shows the deflection of the needle (left without direct interference) on successive days.

As to the kinks in the curve which increase enormously (with the advent of cold weather, the need of more heat increases), they may be due to thermal expansion in the supports of the telescope, or to radiant forces.



But as they are always accompanied by a corresponding vibration of the needle, the latter cause must be in preponderance, so that radiant forces are not, even in case of these large masses, to be disregarded.

Apart from the irregularities, the curve, as a whole, rises uniformly, because of the viscous detorsion specified, at a rate of about 0.08° of detorsion per day, and it has kept this up uniformly for months. One may notice that in view of the large weight (over 3 kg.) suspended, the wire is kept at relatively low viscosity, i. e., full of unstable molecular configurations. Utilizing these, the torsional set obtains facility of exit. How the wire acquired this torsional strain is hard to see, unless it was reeled helically. This was probably the case, as the free wire, placed on a plane, shows a helical form, with a pitch of about 2 feet. That the strong tensile strain, which is also vanishing, should, in the suspended wire, make its exit torsionally is improbable, for there would be no reason for the sign of the twist; but the tensile strain probably developed the torsional strain in the lapse of time on the reel.

To eliminate the viscous torsional discrepancy, it would suffice to use the method developed by Dr. Strouhal and myself, of boiling the wire for 20 or 30 hours in water. Such a wire is then practically free from instabilities at atmospheric temperatures.

Finally, since the torque is $0.26s$, the deviation angle $0.0017s$ and the daily detorsion $0.8 = s$, it is interesting to notice that energy is being dissipated at the rate of 0.00014 ergs per day, by the decay of torsion (very small as it is) above. Much more, no doubt, is released in the decay of the intense traction. If the body were not exceedingly opaque, the strained metal would probably be phosphorescent.

THE CHEMICAL ISOLATION OF VITAMINES

BY C. N. MYERS AND CARL VOEGTLIN

DIVISION OF PHARMACOLOGY, HYGIENIC LABORATORY, UNITED STATES PUBLIC HEALTH SERVICE

Communicated by W. A. Noyes, November 17, 1919

Vitamines are substances of unknown chemical composition occurring in certain foodstuffs, vegetable and animal tissues. They are essential for the maintenance of health and for normal growth of animals and men. The lower organisms also, such as plants and bacteria, seem to require a certain amount of these vitamines for reproduction and growth. The existence of at least three vitamines is known at the present time, namely, the antiscorbutic vitamine, the deficiency of which in the diet causes scurvy; the so-called fat-soluble vitamine, occurring in butter fat and certain other fats; and the antineuritic vitamine, a sufficient amount of which must be consumed by animals and men in order to prevent the disease called beri-beri. All of these three vitamines are essential for normal growth in man.

During the last few years a study of the relation of vitamines to growth and the maintenance of health has been a fertile field for biologists. These researches have resulted in conclusions of great scientific and practical value. There can be no doubt, however, that many mistaken ideas have arisen in this brief period, due mainly to a lack of knowledge of the chemical character of vitamines. Thus, a few investigators have attempted to ascribe definite chemical and physical properties to substances which at best were possessed only in their crudest and impure form. These inaccuracies can be attributed only to a lack of fundamental, chemical knowledge on the part of these investigators. Every chemist thinks in terms of solubility, dissociation, and other physical chemical properties, but always only in case of a pure substance or mixtures of known composition and not in the case of crude mixtures and extracts of unknown composition such as the present-day vitamine preparations. For these reasons work on the chemical composition of vitamines is urgently needed as the previous attempts along these lines have been more or less unsuccessful. This paper deals principally with the antineuritic vitamine.

As it has been found that brewer's yeast is very rich in antineuritic vitamine, this material was selected for this investigation. At first autolyzed yeast was used, but on account of its complex composition it was later on substituted by dried yeast. The general conclusions derived from this work are as follows:

Autolyzed yeast filtrate, on account of its complexity, represents an unsatisfactory material for the chemical isolation of the antineuritic vitamine. Mastic emulsion, Lloyd's reagent (fullers' earth), ferric phosphate, etc., are unsatisfactory adsorbing reagents in that they lack specificity. These reagents remove, also, inactive basic materials which cannot be separated by our present methods from the active material. Olive oil and oleic acid will remove the antineuritic substance from autolyzed yeast filtrate, thus showing that it is fat-soluble as well as water-soluble in the form of a crude extract. Stachhydrine, trigonelline and allied betaines show no antineuritic activity. Histidine and its esters are likewise inactive.

The active material is readily extracted from dried yeast by means of acid methyl alcohol. The extract can be purified by use of the Funk silver method and the mercuric sulphate procedure, yielding an apparently crystalline active substance. This substance becomes inactive upon drying and it is believed that impurities still remain which can be removed with additional modifications of the method described in this paper (see, *J. Biol. Chem.*). The present method eliminates purines, histidine, proteins and albumoses, leaving a liquid that can be crystallized and probably contains histamine or histamine-like substances. The physiological action of the active fractions resembles that of extracts from the mucosa of the small intestine, when the intestinal and yeast extract are purified in the same manner.

A NEW METHOD OF DETERMINING THE SOLAR CONSTANT OF RADIATION

By C. G. Abbot

Smithsonian Astrophysical Observatory

Read before the Academy, November 10, 1919

The Smithsonian Astrophysical Observatory has been engaged for more than ten years in measuring the intensity of the solar radiation at the earth's surface under such conditions that the intensity as it would be outside the atmosphere may be computed from the observations. The method employed hitherto has been the method of Langley which consists of two essential parts. First, measurements with the pyrheliometer of the intensity of solar radiation at the earth's surface. Second, measurement by the spectro-bolometer, covering several hours' time, designed to determine the transparency of the atmosphere for all colors in the spectrum. This latter part of the work demands a uniform transparency of the atmosphere during the several hours required for determining it. This condition is never perfectly met by terrestrial conditions. The changes in the atmospheric transparency not infrequently introduce an error of 1 and sometimes 2 or 3% in the results of the observations at the best stations, although on many days of observation the condition of uniform transparency is so well fulfilled that the results appear to be accurate to a fraction of 1%. If we were interested only in the absolute value of the solar constant of radiation it would be possible to nearly eliminate this source of error by a sufficient number of observations. But it has been shown that it is highly probable, I may say indeed certain, that the solar radiation is variable from day to day through a range of several per cent. The source of error which has been described is a principal difficulty against determining the real variations of the sun on which variations of the weather seem to depend.

My colleagues and myself have long sought to discover a method of determining the solar radiation outside the atmosphere which would be independent of these variations of transparency. We sought to do this through the observations of the brightness of the sky. It is well known that the sky owes its brightness to the scattering of the solar rays by the molecules and small particles which compose our atmosphere. The more hazy the sky, the greater its brightness and the less the atmospheric transparency. Accordingly, it seemed to us hopeful to determine the atmospheric transmission coefficients in terms of the brightness of the sky. We employed in the investigation a new instrument which we call the pyranometer designed to measure the brightness of the whole sky or of any fraction of it, either on a horizontal surface or on a surface at right angles to the average direction of such a portion of the sky as might be under observation. While at Calama, Chile, in June, 1919, I took up the matter with Mr. Moore, the director of the Smithsonian observing station there. He placed at my disposal determinations of the transparency of the atmosphere for nearly forty wave-lengths, pyranometer values representing the intensity of the radiation of the sky in a zone 15° wide surrounding but not including the sun, and measurements of the intensity of the solar radiation by the pyrheliometer and by the spectro-bolometer. All of these values were tabulated with the solar constant values for 60 days of observation. The pyranometer values and values of a function which we call ρ/ρ_{sc} were taken on each day at the periods when the air masses were 2 and 3, respectively, that is to say, when the zenith distance of the sun is about 60° and about 70°.

The function ρ/ρ_{sc} is determined in this way: A spectrum-energy curve taken through the solar spectrum shows deep depressions in the infra-red spectrum, one of which was termed by Langley $\rho\sigma\tau$. Measurement is made of the ordinate of the smooth curve drawn across the top of the depression, and another measurement of the ordinate at the minimum of the depression. The ratio of these two ordinates forms the function ρ/ρ_{sc}. It is a measure, as Mr. Fowle has shown, of the quantity of humidity prevailing in the atmosphere between the observer and the sun. Mr. Fowle indeed has worked out a method of determining the actual amount of precipitable water in the vertical column of the atmosphere by a mere measurement of the value ρ/ρ_{sc}.

Since the haziness of the atmosphere is closely related to its humidity, but since the haziness also depends upon the amount of dust taken up from the soil by the wind, or such as is occasionally distributed in the upper air by volcanic eruptions, we hoped to get a complete indicator of the haziness prevailing by a combination of the value ρ/ρ_{sc} with the value of the sky brightness, determined by the pyranometer, in the immediate neighborhood of the sun. We formed the function "sky brightness divided by ρ/ρ_{sc}," which we will call "*F*." After a number of unsuccessful investigations I was led by a suggestion of Mr. Moore's to find that the values of the atmospheric transmission coefficients for the individual wave-lengths in the spectrum could be represented in terms of the function "*F*" by smooth curves in which atmospheric transmission coefficients were plotted as ordinates and the function "*F*" as abscissae. All of the data for the sixty days available were plotted in this manner for each of the 40 wave-lengths above mentioned. The atmospheric transmission coefficient varies by only 2 or 3, or at most 5% in any part of the spectrum, while the function "*F*" varies through a range of from 100 to 1000 or more on a certain scale. Hence, it is clear that great errors in determining the function "*F*" would produce very small errors in determining the atmospheric transmission.

The measurements required to determine the function "*F*" may be all completed within ten minutes. Entering the plots above mentioned with the values of the function "*F*" as thus determined, we take out from the plots the atmospheric transmission coefficients for all wave-lengths in the spectrum. With these data we may immediately determine the solar constant of radiation by the usual method of computation.

The new method has been in application at Calama, Chile, since about July 1. Measurements by the older method, on which it is primarily based, have been taken on about one-third of the days, and measurements by the new method on practically all days since July 1. It is found that the agreement between the results of the older method and of the new abridged method is very close. In a comparison recently made, covering 53 days on which observations by both methods were available, it was found that on 32 days the result by the new and the old methods agreed within less than 1%, and on 45 days within less than 2%. It was found, also, that on the days in which the agreement was not good, some change in transparency appeared to be occurring which would reasonably account for the discrepancy on the ground that the value by the old method had been impaired thereby.

The new method is applicable on many more days than the old, and it has the further advantage that several independent observations of the solar constant of radiation may be made by the new method on a single day of observation. We believe that by checking the new method against the old from time to time it will be possible to assure ourselves of the accuracy of it, and we propose to employ it principally, hereafter, in our determinations of the solar constant of radiation.

THE BASAL METABOLISM OF BOYS FROM 1 TO 13 YEARS OF AGE

BY FRANCIS G. BENEDICT

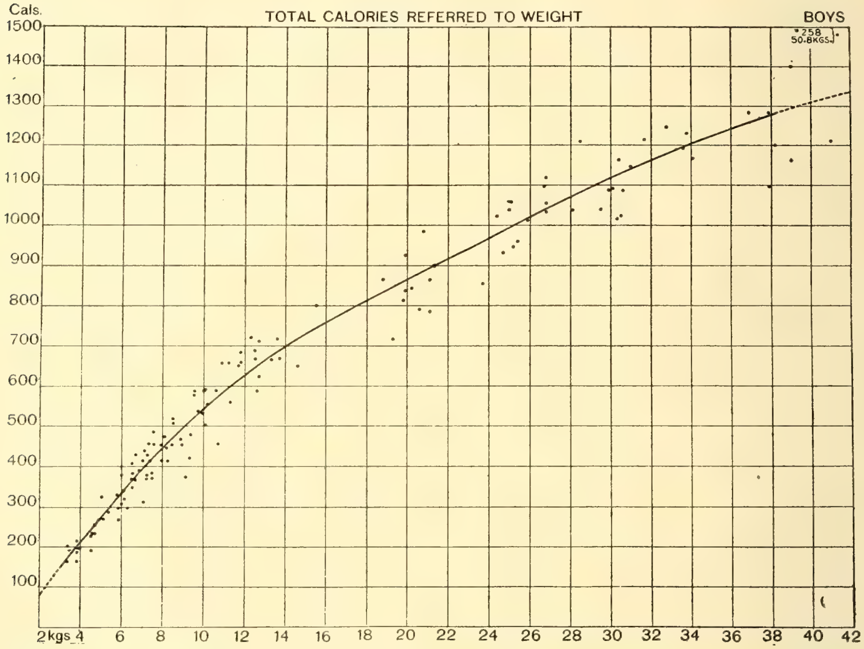
NUTRITION LABORATORY, CARNEGIE INSTITUTION OF WASHINGTON, BOSTON, MASSACHUSETTS

Read before the Academy, November 10, 1919

The Nutrition Laboratory has been occupied for a decade in an effort to chart the field of the basal metabolism of humans of both sexes and of all ages. By basal metabolism is meant the heat production for maintenance during complete muscular repose and without active digestion. From a biometric analysis[1] of the studies already made of men and women, it is clear that weight, stature, age, and sex all have an independent influence upon the basal metabolism.

With growing boys the rapid changes in weight, stature, and age make metabolism measurements of unusual interest. The data that are reported herewith, representing 128 observations on boys ranging in age from a few months to 13 years and above, were all obtained with the coöperation of Dr. Fritz B. Talbot of Boston and the detailed results of the investigation will shortly be published in conjunction with him by the Carnegie Institution of Washington.

The total calories per 24 hours referred to weight are indicated for all the subjects in the chart. A general inspection of this chart shows a distinct trend which follows a reasonably regular course. The scatter of the points about this curve is perhaps surprisingly small, especially when we consider the usual scatter with adults. Indeed, until the collection of more extensive data shows a different picture, it seems possible to use this curve for predicting the probable basal metabolism of boys within this age-period. Although at first sight it would appear as if the grouping of the points around the curve were closer for the boys weighing less than 10 kg. than for those of greater weight, percentagewise, the deviations are greater at the lower weights. The curve has, therefore, been arbitrarily divided for consideration into two sections showing the values obtained with boys below and above 10 kg., respectively.



BASAL HEAT PRODUCTION OF BOYS PER 24 HOURS REFERRED TO BODY WEIGHT

By comparing the actually measured heat output per 24 hours for these boys with the predicted heat output read from this curve for corresponding weights, we find that with 60 subjects below 10 kg., the prediction average is 368 calories against an average of 364 calories from measured values. With 68 subjects above 10 kg., the average predicted value is 911 calories as compared with 907 calories obtained by actual measurement.

These pictures can be looked upon only as a proof of the legitimacy of the curve as sketched. But when individual cases are considered, we find that with boys under 10 kg. there is an average plus or minus deviation of predicted from actual of 30 calories or somewhat over 8%. With the 68 values above 10 kg. the predictions are considerably closer, since the deviations are plus or minus 54 calories on the average, or an average difference of 6.3%. It thus appears that from this curve alone one may predict the heat output of boys weighing 10 kg. and above to within an average accuracy of a little over 6%.

From data obtained by the Nutrition Laboratory of the basal metabolism of 136 men, Dr. J. Arthur Harris[1] has derived by biometric analysis a multiple prediction formula which may be used for predicting the 24-hour basal heat production of men, as follows:

$$h = + 66.4730 + 13.7516w + 5.0033s - 6.7550a$$

In this formula h = total heat production per 24 hours, w = weight in kilograms, s = stature in centimeters, and a = age in years.

Although in publishing this formula, it was specially emphasized that its application to the lower ages was not yet justified, owing to the fact that of the 136 subjects studied, but few were below 20 years of age, it has been of unusual interest to attempt a prediction of the heat output of these boys, using the multiple prediction formula for men. By this entirely independent method of prediction, we find that the average metabolism of the group of boys above 10 kg. is predicted to within 6 calories, that is, 907 calories actual as against 901 calories predicted. The average deviation for individual predictions is plus or minus 56 calories or 6.3%, giving a prediction with an accuracy identical with that obtained from the curve. This is of considerable practical importance as evidence that the metabolism of males ranging from boys of 10 kg. up to full grown adults, including the period of old age, may be predicted with this formula with an accuracy of not far from plus or minus 6%. For ordinary purposes the pediatrician will prefer to use the values drawn from the curve. To simplify its use, a table giving the most probable heat production of boys per 24 hours predicted directly from body weight has been prepared.

BASAL HEAT PRODUCTION OF BOYS PER 24 HOURS PREDICTED FROM BODY WEIGHT

WEIGHT	CALORIES	WEIGHT	CALORIES	WEIGHT	CALORIES	WEIGHT	CALORIES
kg.		*kg.*		*kg.*		*kg.*	
		11	590	21	885	31	1140
		12	625	22	910	32	1160
3	150	13	660	23	940	33	1180
4	210	14	695	24	965	34	1200
5	270	15	725	25	990	35	1220
6	330	16	755	26	1020	36	1240
7	390	17	780	27	1045	37	1255
8	445	18	805	28	1070	38	1275
9	495	19	830	29	1090		
10	545	20	860	30	1115		

While with adults it was clearly demonstrated that in predicting the heat output consideration should be given not only to body weight but likewise to age and stature, it is evident from this treatment that the prediction from the curve based on body weight alone is of equal value with that from the formula, taking into consideration not only weight but also stature and age. This is probably in large part explained by the fact that with boys the changes in age, weight, and stature are closely correlated.

[1] Harris and Benedict, *Carnegie Inst., Washington, Pub.*, No. 279, 1919; see also these PROCEEDINGS, **4**, 1918 (370–373).

THE NATURE AND FUNCTION OF THE ANTINEURITIC VITAMINE[1]

BY R. ADAMS DUTCHER

SECTION OF ANIMAL NUTRITION, DIVISION OF AGRICULTURAL BIOCHEMISTRY, MINNESOTA AGRICULTURAL EXPERIMENT STATION

Communicated by W. A. Noyes, November 17, 1919

Introductory.—Prior to and immediately following the year 1897 various theories were advanced by different investigators relative to the etiology of the oriental disease known as beri-beri. The merits and demerits of the various theories extant at that period are discussed in a very comprehensive way by W. L. Braddon in his book, "The Cause and Prevention of Beri-Beri." It will suffice to say that the prevailing theories held at that time explained the etiology of the disease in terms of bacteria, bacterial poisons, or poisons contained within the food.

During the years 1889 to 1897 Eijkman and his colleagues had advanced the study of the disease to the point where they were able to demonstrate that beri-beri could be produced at will on a diet consisting solely of polished rice, but that a cure could be effected and a recurrence of the disease prevented by the use of hand-milled rice. Eijkman[2] also showed that a disease similar to human beri-beri could be produced in fowls by diets composed of polished rice and that the paralytic symptoms disappeared upon the administration of rice polishings or alcoholic extracts of rice polishings. Schaumann,[3] in 1910, introduced a phosphorus-deficiency theory, basing his convictions upon the fact that all curative foodstuffs possessed a high percentage of phosphorus, while in the case of the non-curative foods the reverse seemed to be true. Schaumann believed the organic phosphorus-containing compounds to be essential in the treatment of beri-beri. This theory was propounded at a time when a great deal of importance was being attached to the lipoid content of the dietary. Other writers[4,5] have shown that a parallelism exists between the phosphorus content of cereals and their vitamine activity, but this parallelism is considered to be fortuitous rather than an actual chemical relationship.

Views Concerning Its Chemical Characteristics.—In 1911 Funk[6] announced that he had been successful in isolating an organic base, $C_{17}H_{18}O_4N(HNO_3)$, from rice polishings which possessed a curative action. On account of the fact that this nitrogenous substance was apparently indispensable to life, Funk gave it the title "vitamine." This was the origin of the vitamine hypothesis.

Since 1911–12 a number of investigators have studied many phases of the vitamine problem with the result that much has been learned concerning the vitamine content of various food materials and the effect of these diets on the growth and development of animals. However, very little advancement has been made in ascertaining with greater certainty the chemical nature of the anti-beri-beri vitamine. Following the publication of Funk's article, Edie[7] and his co-workers announced the isolation of a basic substance from yeast to which was ascribed the formula $N(CH_3)_3C_4H_7O_2(NHO_3)$. They named the substance "Torulin." Suzuki, Shimamura and Odake[8] also publishing the same year obtained a pure chemical substance from rice polishings which possessed a curative action. This substance they identified as nicotinic acid. During the same year Schaumann[9] prepared a phosphorus-free nitrogenous base which had curative properties. He advanced the hypothesis that this base acts as an "activator," stimulating the synthesis of phosphatids which are essential in the regeneration of degenerated nervous tissue.

Funk[10] believed that the curative substance was chemically related to the pyrimidines or purines. To test this hypothesis he fed allantoin, hydantoin, thymus nucleic acid and cytidine, obtaining varying degrees of improvement in polyneuritic birds.

Williams[11] found that the hydroxyl pyridines possessed curative properties which were lost on standing; this, he explains, is probably due to tautomerism and he suggests that the instability of vitamines may be explained on a similar basis. In a later publication[12] this writer concludes that the curative form of α-hydroxyl pyridine is structurally a pseudobetaine. He postulates that nicotinic acid and possibly vitamines are of a similar nature. Harden and Zilva[13] confirmed the chemical facts regarding the α-hydroxyl pyridines and adenine, as put forth by Williams, but failed to obtain relief in avian polyneuritis when these preparations were fed. Voegtlin and White[14] also failed to obtain results as reported by Williams and Seidell.[15]

Views Concerning Its Metabolic Functions.—Funk[16] made the observation that when no food was metabolized beri-beri symptoms did not appear, while an increase in food intake, especially carbohydrate food, hastened the paralytic symptoms. This has been substantiated by other investigators, including the writer, except that we are inclined to believe that overloading the oxidative mechanism with any energy-producing food will usually hasten the onset of symptoms. Funk and Schönborn[17] associate this type of vitamine with carbohydrate metabolism and especially starch metabolism. On vitamine-free diets these authors obtained hyperglycemia with diminished amounts of hepatic glycogen. Vitamine-free and carbohydrate-free diets, in the opinion of these writers, prevents in some manner, the synthesis of glycogen from protein and fat. Vitamine diminished the hyperglycemia and increased the liver glycogen.

The first work recorded, so far as the writer is aware, concerning the relation of vitamine to pathology, is that of Funk and Douglas[18] and Douglas.[19] Pathological examination of the pituitary, thyroid, suprarenals, ovary, testes, kidney, liver, pancreas and spleen showed that on a vitamine-free diet all glands diminished in size and underwent degenerative changes. The thymus gland was caused to disappear and then return by the administration of vitamine.

Drummond[20] has published data indicating that creatinuria follows the feeding of diets deficient in the anti-neuritic vitamine. In this work rats were used as the experimental animals.

Studies made in this laboratory have led us to believe that the vitamine functions indirectly as a metabolic stimulant. We have observed[21] that the body temperature falls during the development of avian polyneuritis and increases after vitamine administration. Measurement of the catalase content of the tissues in polyneuritic birds revealed a drop in catalase activity to a point 56% below normal, indicating that oxidative processes are depressed when the organism is not receiving the normal amount of vitamine. It is not improbable that the depression of body oxidations is accompanied by the formation of toxic metabolic products which affect the nervous system, bringing about the typical paralysis and other symptoms.

The mechanism of the stimulation and cure, when vitamine is fed, is not clear. Certainly the process is not a simple one. It would appear that there is some relationship between vitamine intake and endocrine activity. Mr. S. D. Wilkins, working in this laboratory, has recently caponized Single Comb White Leghorn cockerels whose sole diet, for from three to five weeks, was polished rice. The testicles were found to be atrophied to a surprising degree, often being not more than 20% of normal in size and weight. He has been able to show the effect of diet in this connection by removing but one testicle in each rice-fed bird and immediately afterward adding green alfalfa to the rice diet. The birds on the rice-alfalfa diet grow and increase in weight. Upon caponizing these birds the remaining testicle has increased in size and weight, approaching that of the normal birds. Jackson and Stewart[22] point out that underfeeding brings about atrophy of practically all vital organs, including the testes, accompanied by a loss in body weight. Refeeding does not produce normal growth and none of the animals attain the size and weight of the control animals. This, these writers point out, differs from the results of Osborne and Mendel[23] who concluded that the capacity to grow is not lost on inadequate diets.

The remarkable growth of the animals refed by Osborne and Mendel may have been due to the fact that the diets were adequate in quantity and quality with respect to vitamines and other ingredients with the exception of protein. The organs and ductless glands probably received sufficient nourishment and stimulation to prevent appreciable atrophy. In the case of the underfed rats of Jackson and Stewart the diet was quantitatively inadequate with the result that pathological changes (atrophy) went too far for the normal processes to be restored.

We have been able to obtain decided atrophy of the testes in cockerels without a loss in body weight, which eliminates any possibility of explaining the condition on a starvation or inanition basis. McCarrison[24] has obtained similar results with pigeons, finding atrophy of many of the ductless glands and vital organs. While inanition plays a part, this author believes the unusual pathological condition to be due to the absence of vitamines, which he calls "nucleopasts," due to the fact that he believes them to be essential for nuclear metabolism.

From the standpoint of the feeder and the live-stock producer, as well as from the sociological standpoint, this work suggests two important things which merit emphasis: first, the question of diet in relation to sexual vitality, sterility, etc., and second, the importance of green, fresh feed for all growing animals, especially poultry. Proprietary poultry feeds are often largely carbohydrate in composition, poor in protein and low in vitamine and mineral matter. Many diseases are undoubtedly brought about by poor diets, lowering the resistance of the animal to invading organisms. The rôle that the vitamine plays in this connection in unknown.

We have cured limberneck in fowls by administering a vitamine extract prepared from wheat germ. In each case recovery was preceded by copious bowel evacuations. The action in this case is probably not a typical vitamine action, but there is little doubt but that the vitamine plays its part in the rapid recovery of the birds. Neither do we wish to leave the impression that limberneck is a deficiency disease; in fact, we have been unable to produce it by any method of feeding or treatment. Uhlmann[25] states that water-soluble vitamine acts in a manner very similar to pilocarpine, stimulating gland secretion. It is very possible that the laxative action of many fruits, vegetables, whole grains and brans, is due, in a measure, to increased gland secretion brought about by vitamine stimulation.

With Uhlmann's work in mind and also the work of Kendall,[26] who isolated thyroxin, the hormone of the thyroid gland, we[27] have fed polyneuritic pigeons the stimulants thyroxin, desiccated thyroid, pilocarpine hydrochloride and tethelin. The thyroxin and thyroid were fed because of Kendall's findings that energy production was dependent upon thyroid activity. Pilocarpine hydrochloride was used because of Uhlmann's work mentioned above. Tethelin, the growth-controlling principle of the anterior lobe of the pituitary body,[28] was fed for similar reasons. All of these substances produced definite relief and cessation of polyneuritic symptoms. The response was slower in all cases than when vitamines are fed.

The hormones from the ductless glands undoubtedly influence to a marked degree the activity, health, growth and well-being of animals. May it not be possible that the hormone supply is dependent upon the vitamine content of the food? As a result of our observations, we wish to suggest tentatively that the activity of the organs of internal secretion are dependent upon the stimulatory action of the vitamine. Whether this is in the nature of a nerve stimulant, nuclear nutrient or the chemical nucleus of a hormone is, of course, a matter of speculation.

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RECONSTRUCTION OF THE SKELETON OF THE SAUROPOD DINOSAUR CAMARASAURUS COPE (MOROSAURUS MARSH)

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The principles of modern research in vertebrate palæontology are illustrated in the fifteen years' work resulting in the restoration of the massive sauropod dinosaur known as *Camarasaurus,* the "chambered saurian."

The animal was found near Canyon City, Colorado, in March, 1877. The first bones were described by Cope, August 23, 1877. The first attempted restoration was by Ryder, December 21, 1877. The bones analyzed by this research were found probably to belong to six individuals of *Camarasaurus* mingled with the remains of some carnivorous dinosaurs, all from the summit of the Morrison formation, now regarded as of Jurassic-Cretaceous age. In these two quarries Cope named nine new genera and fourteen new species of dinosaurs, none of which have found their way into palæontologic literature, excepting *Camarasaurus.* Out of these twenty-three names we unravel three genera, namely:

One species of *Camarasaurus,* identical with *Morosaurus* Marsh.

One species of *Amphicœlias,* close to *Diplodocus* Marsh.

One species of *Epanterias,* close to *Allosaurus* Marsh.

The working out of the *Camarasaurus* skeleton results in both the articulated restoration and the restoration of the musculature. The following are the principal characters: The neck is very flexible; anterior vertebrae of the back also freely movable; the division between the latter and the relatively rigid posterior dorsals is sharp. Double spines of the cervicals and flexible anterior dorsals show through the skin of the restoration. Spines of the relatively inflexible dorsals, sacrals and caudals are comparatively low and broad. The breadth of neck, thorax, and pelvis, as shown by the bones, is relatively greater than in other sauropodous dinosaurs. The tail had no terminal whip-lash. The scapula is very massive and expanded at the summit. The cross-section of the thorax is relatively much broader than in other Sauropoda. The general conclusion is that *Camarasaurus* was a very broad and massive, slow-moving sauropod, in fact, the most massive reptile in proportions that has ever been found.

This study is part of the senior author's research for the *Monograph on the Sauropoda* in preparation for the U. S. Geological Survey.

RESTORATION OF CAMARASAURUS AND LIFE MODEL

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The muscular restoration of *Camarasaurus* was preceded by careful studies of the consecutive poses of the skeleton of *Apatosaurus* (*Brontosaurus*) in three phases of the stride. These poses were worked out in a minature model of the complete skeleton, with flexible joints. The conclusion reached is that *Apatosaurus* was a walking and not a crawling quadruped. As compared with living crocodiles the body was well raised off the ground in the stride. This is contrary to the ideas of Tornier, Hay and others, who advocated a crawling stride. The limbs, however, had not the fore-and-aft pendulum action of the perfected mammalian quadrupeds; the stride had a wide, lateral swing.

Subsequently a complete restoration of the musculature of *Camarasaurus* was made in comparison with living reptiles. The prominence of the muscle lines is justified by studies of recent alligators, in which certain muscle groups stand out as low humps in action. The pose selected was based directly on a photograph of a rapidly walking alligator, the right fore foot having just descended without the natural continuation of the stride by the hind feet.

The special points noted in the position of the bones of the skeleton are the following: Scapula gently, not sharply, inclined backward; inferior position of the scapulocoracoid, which was brought relatively near its fellow on the opposite side; spreading upper border of the scapula implying an essentially reptilian arrangement of the trapezius and other muscles; moderate bending and eversion of the elbow in the stride, the motions of the arm bones being essentially similar to those of the crocodiles but with the elbow joint less strongly flexed; digitigrade position of the bones of the fore limbs and manus. The fore limb as a whole contrasted widely with that of the mammal. Pelvic musculature and arch fundamentally similar to those of the Crocodilians. At no time did the head of the femur fill the acetabulum. The backbone is elevated above the shoulder blade in the walking pose. The tail of *Camarasaurus* is relatively smaller in transverse section than that of Crocodilians, with correspondingly weak muscles.

Camarasaurus might well have been an efficient wader. It was positively devoid of special adaptations for swimming; the pectoral and pelvic arches, and the limbs and backbone were adapted to support the great weight of the body presumably on land, as well as in wading; the tail was relatively small and feeble. There was a high ratio of limb power to weight.

The most decisive points in favor of the theory that *Camarasaurus* was able to walk with the body well raised off the ground are the following: (1) Inability to flex strongly the elbows, evidenced by the characters of the distal facet of the humerus. (2) Ability to turn the femur well forward, like that of carnivorous dinosaurs. (3) Detailed resemblances of the femur to that of *Stegosaurus*, assuredly a straight-limbed reptile. (4) Total dissimilarity between the humerus and femur of sauropods and those of primitive Permian reptiles.

PLATO'S ATLANTIS IN PALAEOGEOGRAPHY

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The classic story of Atlantis has been supposed by some writers to be a genuine tradition, and to be supported by scientific evidence, which they believe indicates a land bridge across the Atlantic in former geologic times. The island of Atlantis as described by Plato they suppose to be a remnant of this bridge. Examination of the story in detail shows that it is a fable, and that the scientific evidence does not lend any support whatsoever to it nor vice versa.

The existence of a transatlantic bridge in Tertiary or pre-Tertiary times is a legitimate scientific question, which should not be confused with the fabulous story of Plato. The present writer does not, however, believe that such bridges are necessary. Their existence during the Tertiary period cannot apparently be reconciled with the known history of vertebrate evolution on the two sides of the Atlantic. The supposed arguments in their favor from the distribution of certain lower animals and plants can all be otherwise explained; and the arguments that favor a general permanency of the ocean basins afford grave objections to any such bridges, especially of such recent date, geologically.

Transatlantic bridges in pre-Tertiary times are theoretically more plausible simply because there is less positive evidence against them. The evidence adduced in their favor appears to be similar in kind to that for Tertiary land bridges, and not any more convincing. If in the one case it must be otherwise interpreted, then it should be so in the other.

The conformation of the bottom of the Atlantic is very little suggestive of any former bridges, except in the extreme north (from Newfoundland to Ireland, or across Greenland, Iceland and Scandinavia). A long, depressed, irregular trough or series of "deeps" on each side fronts the continental borders which are areas of elevation and erosion. These deeps may have been formed by the withdrawal of material that has gone to build up the lighter fragmented rocks of the continental platform borders, aided by the loading of heavier calcareous and ferruginous deep-sea deposits. Since the principal elevation and erosion of the continental border strips fronting the Atlantic took place during the Palæozoic, it is suggested that the present conformation of the Atlantic bottom dates back in part at least to that era.

THE IONIZATION AND ACTIVITY OF LARGELY IONIZED SUBSTANCES

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In applications of the ionic theory of solutions it is customary to employ, in accordance with the original hypothesis of Arrhenius, as a measure of the degree of ionization of salts, acids, and bases, the ratio of the equivalent conductance of the substance at any given concentration to the limiting value of the equivalent conductance as the concentration approaches zero, where the ionization may be assumed to be complete.

This assumption, however, is not a necessary conclusion from the fundamental theory of ionic conduction. According to this theory, the equivalent conductance Λ (which is by definition the quantity of electricity which under a potential-difference of one volt passes per second between electrodes of indefinite extent 1 cm. apart, between which is placed that quantity of solution which contains one equivalent weight of the ionizing substance) is for a uniunivalent substance given by the expression $\Lambda = \gamma F(U^+ + U^-)$, in which γ is the fraction of the substance ionized (equal in this case to the number of equivalents of each ion present in the solution), F the quantity of electricity (96,500 coulombs) associated with each equivalent of ion, and U^+ and U^- the mobilities of the positive and negative ions (that is, their velocities through the solution under a potential-gradient of one volt per centimeter). For the equivalent conductance Λ_0 at zero concentration, where the ionization becomes complete, we have the corresponding expression $\Lambda_0 = F(U_0^+ + U_0^-)$. Combining these two equations we get

$$\frac{\Lambda}{\Lambda_0} = \gamma \frac{U^+ + U^-}{U_0^+ + U_0^-}.$$

From this equation it is evident that Λ/Λ_0 is equal to the ionization only when the mobilities of the ions can be assumed constant up to the concentration under consideration. That they should remain constant so long as the solution does not differ appreciably from water as a viscous medium, may seem reasonable; but, in view of possible electrical effects resulting from the large electric charges on the ions, it is by no means certain.

This simple assumption has been justified in the case of slightly ionized acids and bases, where the ion-concentration in the solution is small, by the fact that the so-determined ionization values change with the concentration just as the mass-action law requires. This, however, is not true even approximately in the case of salts and of the largely ionized acids and bases (such as hydrochloric acid and sodium hydroxide); and we are forced to conclude, either that owing to the change in the ion-mobilities the conductance-ratio is not a correct measure of ionization, or that the chemical activity or mass-action effect of ions, and perhaps also of the un-ionized molecules present with them, is not proportional to their concentration, as the ordinary mass-action law assumes.

In either case the conductance-ratio affords us no reliable information as to the chemical activity of ions; and we must turn to other properties for a quantitative measure of this important factor, which determines the equilibrium of all chemical reactions between salts, acids, and bases in solution, and also the magnitude of many physical properties thermodynamically related to the activities.

The term activity must, in the first place, be defined in a precise way; and from a chemical standpoint the most practical method is to define it, as was proposed by G. N. Lewis,[1] as the quantity which when substituted for the concentration of the substance in mass-action expressions will express its effect in determining the equilibrium. Thus the activity of the substance is its "effective concentration" from this mass-action view-point.

Correspondingly, the most obvious method of determining the relative activities of a substance in solutions of different concentrations is to find its concentrations in a gaseous phase in equilibrium with the solutions; for in gases at low pressure the concentration and activity can ordinarily be assumed to be proportional. For example, we know that the ratio of the activities of un-ionized hydrochloric acid in its 11-molal and 8-molal aqueous solution at 30° is 12.0, since the partial vapor-pressures of the hydrochloric acid in those solutions have been found to be 11.3 and 0.94 mm. of mercury. This quantity is also the ratio of the products of the activities of the hydrogen-ion and chloride-ion in the two solutions, since these products are by definition the quantities that must be substituted in the mass-action expression for the equilibrium of the reaction $HCl = H^+ + Cl^-$. Thus denoting the pressures in the two solutions by p_1 and p_2, the activities of the un-ionized molecules by a_1 and a_2, and those of the ions by a_1^+, a_1^-, and a_2^+, a_2^-, we get:

$$\frac{p_1}{p_2} = \frac{a_1}{a_2} = \frac{a_1^+\, a_1^-}{a_2^+\, a_2^-}.$$

This direct method is, however, of very limited applicability in the case of largely ionized substances, since they seldom have appreciable vapor-pressures. We have recourse, therefore, to a simple thermodynamic relation between activity and electromotive force. This relation may be derived by considering the work or free-energy decrease attending the transfer of one mol of the substance (for example, of 1 HCl) from the solution in which its vapor-pressure is p_1 to that in which it is p_2. This free-energy decrease $-\Delta F$ is given by the familiar expression $-\Delta F = RT \log (p_1/p_2)$. In this expression, in view of the above considerations, we may substitute for the vapor-pressure ratio the ratio $(a_1^+ a_1^-)/(a_2^+ a_2^-)$ of the product of the activities of the ions, yielding the equation

$$-\Delta F = RT \log \frac{a_1^+ \, a_1^-}{a_2^+ \, a_2^-}.$$

We thus obtain what may be regarded as a secondary, but more general definition or measure of activity.

The simplest process from a theoretical standpoint (aside from that already described involving passage through the vapor phase) by which a substance can be transferred from one solution to another is one in which this transfer is brought about in a voltaic cell. Thus in the case of hydrochloric acid, we can cause a transfer of one mol of the acid from concentration c_1 (activity a_1) to concentration c_2 (activity a_2) by causing one faraday (F coulombs) to pass through the cell

$$H_2 \text{ (1 atm.)}, HCl(c_2), AgCl + Ag, HCl(c_1), H_2 \text{ (1 atm.)}.$$

The electromotive force E of this cell multiplied by the quantity of electricity F is, therefore, the work which can be obtained from the change in state under consideration, or the free-energy change attending it. That is,

$$-\Delta F = \mathrm{EF} = RT \log \frac{a_1^+ \, a_1^-}{a_2^+ \, a_1^-} = RT \log \frac{c_1^2 \alpha_1^+ \, \alpha_1^-}{c_2^2 \alpha_2^+ \, \alpha_2^-}.$$

It will be noted that in the last of these expressions there has been written, in place of the activities a of the ions, products $c\alpha$, in which the quantity α, called the activity-coefficient, is evidently the factor by which the total concentration c of the substance must be multiplied to give the activity of the ion.

By the use of this electromotive-force method the authors of this paper have, with the aid of their students, carried out a series of determinations of the activities of typical substances. Two of these researches, those on potassium chloride[2] and on hydrochloric acid,[3] have already been published; and two more, on lithium chloride and potassium hydroxide, will soon be described in detail in the *Journal of the American Chemical Society*. These last investigations were carried out, with the aid of grants from the Carnegie Institution of Washington, by Mr. J. A. Beattie and Mr. Ming Chow, respectively, using cells, with dropping amalgam electrodes, of the following types:

Ag + AgCl, LiCl (c_2), Li in Hg, LiCl (c_1), AgCl + Ag.

Hg + HgO, KOH (c_2), K in Hg, KOH (c_1), HgO + Hg.

It is the purpose of this paper to summarize and compare the results of these investigations, and to state the general conclusions to which they lead.

From the observed values of the electromotive forces of these cells there were calculated by the equation given above the products $\alpha^+ \alpha^-$ of the activity-coefficients of the two ions at various concentrations, the value of the products at the lowest concentration (0.001 or 0.0035 molal) at which accurate measurements could be made being assumed equal to the conductance-ratio Λ/Λ_0 at that concentration. These activity-coefficient products were plotted against the logarithm of the concentrations, and those corresponding to round concentrations were read off.

The following table contains the values of the square root of the so-obtained products, that is, the values of the expression $(\alpha^+\alpha^-)^{1/2}$, which represents the geometrical mean of the activity-coefficients of the positive and negative ions of the substance. This table also contains the corresponding values of the conductance-ratio Λ/Λ_0, multiplied by the ratio η/η_0 of the viscosity of the solution to that of pure water, this last serving to correct approximately for the frictional resistance of the medium to the passage of the ions through it.

TABLE 1

ACTIVITY-COEFFICIENTS AND CONDUCTANCE-VISCOSITY RATIOS

Mols. per 1000 g. water	ACTIVITY-COEFFICIENTS				CONDUCTANCE-VISCOSITY RATIOS			
	KCl	LiCl	HCl	KOH	KCl	LiCl	HCl	KOH
0.001	0.979	0.976			0.979	0.976	0.990	
0.003	0.943	0.945	0.990	0.982	0.968	0.962	0.986	0.980
0.005	0.923	0.930	0.965	0.975	0.956	0.949	0.981	0.975
0.010	0.890	0.905	0.932	0.961	0.941	0.932	0.972	0.963
0.030	0.823	0.848	0.880	0.920	0.914	0.904	0.957	0.939
0.050	0.790	0.817	0.855	0.891	0.889	0.878	0.944	0.925
0.100	0.745	0.779	0.823	0.846	0.860	0.846	0.925	0.910
0.200	0.700	0.750	0.796	0.793	0.827	0.812	0.909	0.891
0.300	0.673	0.738	0.783	0.769	0.807	0.792	0.903	0.889
0.500	0.638	0.731	0.773	0.765	0.779	0.766	0.890	0.884
0.700	0.618	0.734	0.789	0.772	0.761	0.751	0.874	0.879
1.000	0.593	0.752	0.829	0.786	0.742	0.737	0.845	0.877
2.000			1.040					
3.000		1.164	1.402					

The table contains the results of only the above-described researches carried out under our direction. It should be mentioned, however, that exact electromotive-force measurements from which activities can be derived have also been made by Jahn[4] on potassium, sodium, and hydrogen chlorides, by Harned[5] on potassium chloride, and by Linhart[6] on hydrochloric acid. As the results of Harned supplement at higher concentrations those for potassium chloride here presented, it may be stated that, assuming a constant transference-number of 0.497 for the potassium ion and an activity-coefficient of 0.745 at 0.1 molal as given in the table, his data lead to the following values:

Normal concentration..........	0.2	0.3	0.5	0.7	1.0	2.0	3.0
Activity-coefficient............	0.688	0.657	0.624	0.608	0.593	0.572	0.586

The results presented in the table may be summarized as follows:

1. In the case of all four substances the activity-coefficient decreases with increasing concentration much more rapidly than does the conductance-viscosity ratio, the differences amounting to from 7 to 15% at 0.1 molal, and from 5 to 18% at 0.5 molal.

2. In the case of all the substances except potassium chloride the activity-coefficient, unlike the conductance-viscosity ratio, passes through a pronounced minimum in the neighborhood of 0.5 molal, afterwards increasing rapidly at the higher concentrations. Even potassium chloride, according to Harned's data, has a minimum activity-coefficient in the neighborhood of 2-normal.

3. The activity-coefficient even at moderate concentrations varies considerably with the nature of the substance; thus its value at 0.5 molal is 64% for potassium chloride, 73% for lithium chloride, and 77% for hydrochloric acid and for potassium hydroxide.

From these facts we may draw the general conclusions that the conductance ratio can no longer be regarded as even an approximate measure of the activity of the ions of largely ionized substances in their mass-action and thermodynamic relations; that this activity varies with the concentration, differently in the case of different substances; and that for the present it can be determined only empirically for each substance, with the aid of measurements of chemical equilibria, electromotive force or freezing-point.[7]

It is, moreover, evident that the activity-coefficient of the ion-constituents cannot be proportional to and mainly determined by the fraction of the substance ionized, for this fraction could not increase with increasing concentration unless the ionizing power of the medium becomes much greater at moderate concentrations; and even then, it could not become greater than unity, as is actually the case with the activity-coefficient of hydrochloric acid above 2-molal.

The results here presented do not show whether or not the conductance-ratio is equal to the degree of ionization. But these two quantities can hardly be equal in view of the fact that the conductances of the two ion-constituents of most uniunivalent substances seem to vary by different percentage amounts with increasing concentration, as may be seen from

Noyes and Falk's[8] summary of the experimentally determined transference-numbers. These numbers, which are equal to the ratio $U^+/(U^+ + U^-)$ of the mobility of the cation-constituent to the sum of the mobilities of the two ion-constituents, show variations that correspond to changes in the ratio U^+/U^- of the mobilities of the two ion-constituents between zero concentration and 0.3 normal, of $4^1/_2\%$ in the case of sodium chloride, $7^1/_2\%$ in the case of hydrochloric acid, and 24% in the case of lithium chloride. Moreover, MacInnes[9] has shown that the chloride-ion constituent has the same equivalent conductance in 0.1 normal solutions of lithium chloride, potassium chloride, and hydrochloric acid, although the conductance-ratios Λ/Λ_0 for these substances are 0.833, 0.862 and 0.925; so that, if we account for the constancy of the chloride-ion conductance by the probable assumptions that the three substances are equally ionized and that the chloride-ion has the same mobility in the three solutions, we must conclude that the hydrogen-ion and lithium-ion decrease in mobilities between zero concentration and 0.1 normal by amounts that differ from each other by 10%—a result that makes it not unreasonable to suppose that the whole decrease in equivalent conductance (of $7^1/_2\%$ and 16% in the two cases) may be due entirely to decrease in mobility of the ions, and not at all to decrease in ionization.

When, indeed, in addition to these conclusions that neither the activity-coefficients nor the conductance-ratio is determined primarily by the degree of ionization, we take into consideration the fact that there is no property which affords any direct evidence of the existence of un-ionized molecules in solutions of most of the largely ionized inorganic substances up to moderate concentrations, it seems advisable to adopt for the present the hypothesis that such substances are completely ionized, and to attribute the decrease in the conductance-ratio wholly to decrease of ion-mobility, and the change in activity-coefficient entirely to some unknown effect of a physical nature.

It would lead far beyond the scope of this paper to discuss the many classes of phenomena that seem to substantiate this assumption. A summarized description of them was given many years ago by one of the authors of this paper,[10] who at that time, however, suggested that they might be explained more fully by the hypothesis that the ions are partially united as a result solely of their electrical attraction into loosely bound molecules, which differ fundamentally from the stable molecules formed as a result of chemical affinity in accordance with the law of mass-action. The known facts may, however, prove to be better accounted for by the simple hypothesis of complete ionization, supplemented by some other, purely physical, explanation of the cause of the decrease of ion-mobility and of ion-activity with increasing concentration; and this now seems the most promising method of treatment, as has recently been urged by various authors.[11] As said above, we cannot here discuss in detail the hypothesis of complete ionization; but in closing it may be pointed out that it accounts for the remarkable facts that so many very dissimilar chemical substances (for example, hydrochloric acid and potassium chloride) seem to be equally ionized, and that a volatile substance like hydrochloric acid does not have an appreciable vapor-pressure even in 1-normal solution where 15% of it must be assumed to be in the un-ionized state, if the conductance-ratio is taken as a measure of ionization. It may also be mentioned that it avoids the improbable conclusions as to the abnormal activity of the un-ionized molecules to which solubility-effects, interpreted under the older assumptions, lead.[12]

[1] Lewis, *Proc. Amer. Acad.*, **43**, 1907 (259–293); *Zs. physik. Chem.*, **61**, 1908 (129–165).

[2] MacInnes and Parker, *J. Amer. Chem. Soc.*, **37**, 1915 (1445–1461).

[3] Ellis, *Ibid.*, **38**, 1916 (737–762); Noyes and Ellis, *Ibid.*, **39**, 1917 (2532–2544).

[4] Jahn, *Zs. physik. Chem.*, **33**, 1900 (559–576).

[5] Harned, *J. Amer. Chem. Soc.*, **38**, 1916 (1989).

[6] Linhart, *Ibid.*, **41**, 1919 (1175–1180).

[7] Lewis, *Ibid.*, **34**, 1912 (1635); Bates, *Ibid.*, **37**, 1915 (1421–1445).

[8] Noyes and Falk, *Ibid.*, **33**, 1911 (1454).

[9] MacInnes, *Ibid.*, **41**, 1919 (1086).

[10] Noyes, "The Physical Properties of Aqueous Salt Solutions in Relation to the Ionic Theory," *Congress of Arts and Sciences, St. Louis Exposition*, **4**, 1904 (317); *Science*, **20**, 1904 (582); abstract, *Zs. physik. Chem.*, **52** (635); also Noyes, *J. Amer. Chem. Soc.*, **30**, 1908 (335–353).

[11] Milner, *Phil. Mag.*, **35**, 1918 (214, 354); Ghosh, *Trans. Chem. Soc. (London)*, **113**, 1918 (449, 627); Bjerrum, *Zs. Elektrochem.*, **24**, 1918 (321).

[12] Bray, *J. Amer. Chem. Soc.*, **33**, 1911 (1673–1686).

THE COMMUTATIVITY OF ONE-PARAMETER TRANSFORMATIONS IN REAL VARIABLES

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If $X = \Sigma\xi_i(x_1 \ldots x_n)\frac{\partial}{\partial x_i}$ and $Y = \Sigma\eta_i(x_1 \ldots x_n)\frac{\partial}{\partial x_i}$ be the differential symbols of two one-parameter transformations, the known condition for the commutativity of the finite transformations is the identical vanishing of the commutator $(XY) = XY - YX$. The proof given by Lie-Engel, applying to the case of analytic functions, depends on expansions in powers of the canonical parameters. The computation of (XY) according to the original definition involves the use of the second partial derivatives of the coefficients ξ, η, but the final form, containing only first derivatives, is

$$(XY) = \sum_i (X\eta_i - Y\xi_i)\frac{\partial}{\partial x_i}. \tag{1}$$

The proof here sketched, in which the commutator is to be considered defined directly by (1), shows that for the case of real variables it is sufficient to assume the existence of continuous, first partial derivatives only of the ξ's and η's. The basis of the proof is formed by familiar theorems on the existence and uniqueness of the solutions of differential equations and their differentiability with respect to parameters. A minimum range of variation for which the results can be known to apply could be specified by the use of inequalities such as naturally occur in connection with those theorems.

With the canonical parameters denoted by α and β, and the corresponding finite transformations by X_α and Y_β, let the point $(x_{10}, \ldots, x_{n0})$ be transformed first by Y_β then by X_α, defining thus the functions $x_i(\alpha, \beta)$. These are then determined by the differential equations

$$D_\alpha x_i(\alpha,\beta) = \xi_i[x(\alpha,\beta)], \tag{2}$$

in which the values initial for $\alpha = 0$ are determined by

$$D_\beta x_i(0,\beta) = \eta_i[x(0,\beta)], \tag{3}$$

where x_i (0, 0) is to be x_{io}. The functions x_i (α, β) then exist for a certain range of variation of α and β; and with respect to the latter they have derivatives satisfying the equations

$$D_\alpha D_\beta x_i(\alpha,\beta) = \sum_j \xi_{ij}[x(\alpha,\beta)]D_\beta x_j(\alpha,\beta), \tag{4}$$

in which ξ_{ij} means $\partial\xi_i/\partial x_j$. Now $D_\alpha\eta_i[x(\alpha,\beta)]$ exists and is given by

$$D_\alpha\eta_i[x(\alpha,\beta)] = \sum_j \eta_{ij}[x(\alpha,\beta)]\xi_j[x(\alpha,\beta)], \tag{5}$$

which by virtue of the assumed identical vanishing of (XY) is equivalent to

$$D_\alpha\eta_i[x(\alpha,\beta)] = \sum_j \xi_{ij}[x(\alpha,\beta)]\eta_j[x(\alpha,\beta)]. \tag{6}$$

Comparison of (4) and (6) shows that the $D_\beta x_i(\alpha,\beta)$ and the $\eta_i[x(\alpha,\beta)]$ satisfy the same system of linear differential equations, and according to (3) they agree for $\alpha = 0$. Hence, for every α,β,

$$D_\beta x_i(\alpha,\beta) = \eta_i[x(\alpha,\beta)]. \tag{7}$$

Thus the functions $x_i(\alpha,\beta)$ are the same as would be obtained by the opposite sequence, X_α followed by Y_β, showing the condition in question to be sufficient.

A sense in which the vanishing of (XY) is also necessary appears likewise in connection with the differential equations. If for both orders of transformation the $x_i(\alpha,\beta)$ be the same, then corresponding to those orders is to be inferred the existence of the derivatives

$$D_\alpha D_\beta x_i = \sum_j \xi_{ij} D_\beta x_j = \sum_j \eta_j \xi_{ij} = Y\xi_i,$$

$$D_\beta D_\alpha x_i = \sum_j \eta_{ij} D_\alpha x_j = \sum_j \xi_j \eta_{ij} = X\eta_i.$$

If these are to agree the commutator must vanish at all points actually occurring in the transformation; a similar limitation in the meaning of "identical" may be understood in the proof of sufficiency.

THE INTENSITIES OF X-RAYS OF THE L SERIES

II. THE CRITICAL POTENTIALS OF THE PLATINUM LINES

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Introduction.—This work is a continuation of that of Webster and Clark, reported in these PROCEEDINGS[1] in 1917. Part of the present work was done at Harvard University with the apparatus described in the earlier paper, and part with my new apparatus at the Massachusetts Institute of Technology. The object in view is the investigation of the laws relating intensity to potential, for the L-series lines, for the purpose of comparison with current theories of X-ray spectra, and the present paper deals with the determination of the critical potentials of the platinum lines of medium intensity, the stronger ones having been reported in the previous paper and the fainter ones, observed only in tungsten by Dershem[2] and Overn,[3] being so faint as to require a much more prolonged study.

Apparatus.—The work at Harvard showed that with the slit widths needed for accurate intensity measurements it would be difficult to work with certainty on any lines but the strong ones, when the voltage was near the critical value. As the previous work of the author on the rhodium K series[4] had shown that photography gave good results in such work it was decided to use it here. In this case, where the lines are many and scattered and faint, the best instrument seemed to be the bent mica spectrograph of de Broglie and Lindemann[5] with which they have obtained excellent spectrograms. These show spectra of six orders, called first to sixth inclusive, though I think the last one, from its angle, must really be the seventh rather than the sixth, which must be very faint. The grating space is about 10 Ångstroms and the third and fifth orders are the strongest.

In the present work, to increase the intensity even at a sacrifice of resolving power, I used a long radius, either 40 or 80 cm., with the tube 75 cm. from the mica and the plate as shown in figure 1. In practice, even with the best mica, there are faults in its structure that make it necessary to take several photographs at each voltage with different positions of the mica so as to insure clear reflection for each line in one case or another. To avoid scattered rays the mica was held on the edges of two brass plates, far enough apart so that no direct rays struck them, and the direct beam through the mica was caught in a sheet of lead. The lines were identified from a chart made from the wave-length measurements of Siegbahn and Friman,[6] and the voltmeter (Chaffee type, as in the previous work) was calibrated by the wave-length λ_{min}, of the high frequency end of the third order spectrum as determined from this chart. In all exposures near any critical voltages the voltage was regulated to a mean deviation of $^1/_5$ to $^1/_3$ of 1% from the average value.

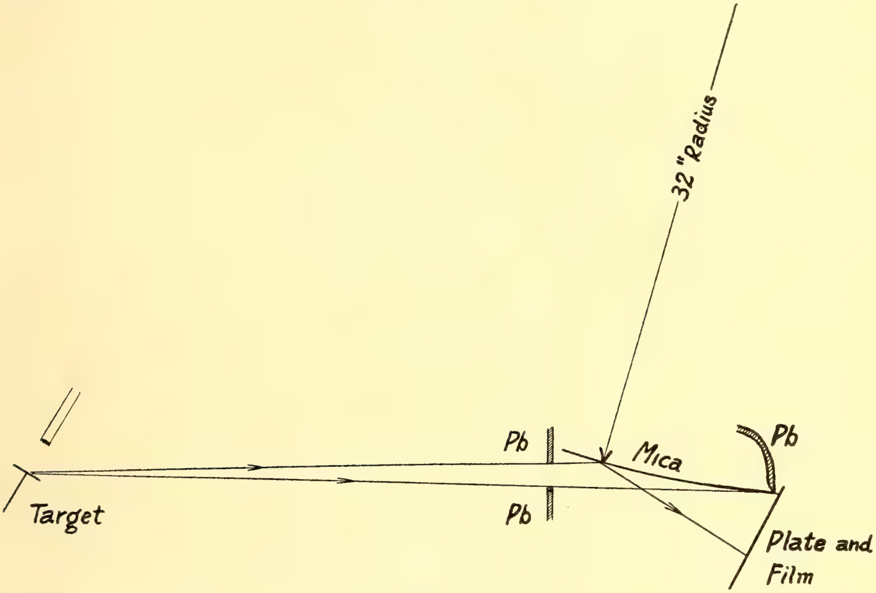

FIG. 1

An important improvement in the voltmeter, introduced in the work at Harvard, was the use of a single insulator only, directly under the suspended system, with fairly good electrostatic screening. This prevents disturbances from indeterminate charges on the insulator which may otherwise make serious errors.

Each spectrogram was taken on one or two Eastman Dupli-tized X-ray films backed by a Seed Graflex plate. For locating very faint lines this combination was useful because with the plate and film, or two films, in contact after development any relative motion of the two would make the spectrum flash out stronger when the images coincided. This method worked especially well when they were still wet. The parts where the general radiation was not too strong were also examined by holding the film against a white paper in a strong light, and a negative lens was also used. Since these methods were needed it is obviously impossible to show the faint lines in a reproduction even when there was no doubt of their existence in the original. Therefore, no reproduction is attempted here.

I wish here to express my thanks to Mr. D. S. Piston, who took most of the photographs in this investigation.

The potential was furnished by a modified form of Hull's[7] apparatus shown in figure 2. The 500-cycle current came from a Crocker-Wheeler motor generator loaned by the Submarine Signal Co., through the generosity of Mr. H. W. J. Fay. The transformers and inductances were made mostly from parts of induction coils contributed by the Boston City Hospital, through the kindness of Dr. F. H. Williams. The 60-cycle one had the small secondaries so placed that the heating current in either kenotron was the same whether the other was on or not. The condensers were of glass plates and sheet iron, immersed in oil and protected as in Hull's apparatus, by corona gaps.

With connection *A*, shown by the short-dash line, only one kenotron is used, and the potential on it rises to more than twice that of the D. C. line, while the frequency of the fluctuation on that line is 500~, the same as that of the transformer. But with connection *B*, using two kenotrons, the potential on each is that of the D. C. line itself, that of the transformer is half that amount, and the fluctuation frequency on the D. C. line is doubled. It should be noticed that to produce this double frequency it is most essential that no part of the transformer secondary or first or second condensers should be grounded.

The constants of the apparatus were approximately as follows: $C_1 = 3 \times 10^{-3}$ mfd, $C_2 = C_3 = 1.5 \times 10^{-3}$ mfd, L_1 (sum of the two sides plus twice the mutual inductance) = 20,000 henries, L_2 = 15,000 henries. Such large inductances are not strictly necessary, and were used only because they were at hand. They were measured by using 90 kv. (measured by spark) at 500~ with an A. C. voltmeter (current type) in series in the middle of the line. As the voltage fluctuation is approximately $\frac{I}{C_1C_2C_3L_1L_2\omega^5}$, it is extremely small, probably not over a few volts under the worst conditions. The resistance of L_1 and L_2 together, measured by D. C., was about 0.3 megohm. When running, a current type voltmeter in series with C_2 and C_3 showed no measurable current, though of course it showed easily the current in C_1.

Incidentally, it may be noted that the above expression for the fluctuation should contain also the factor $(1 - C_2M\omega^2)$, where M is the mutual inductance between L_1 and L_2. This factor could readily be used to neu-

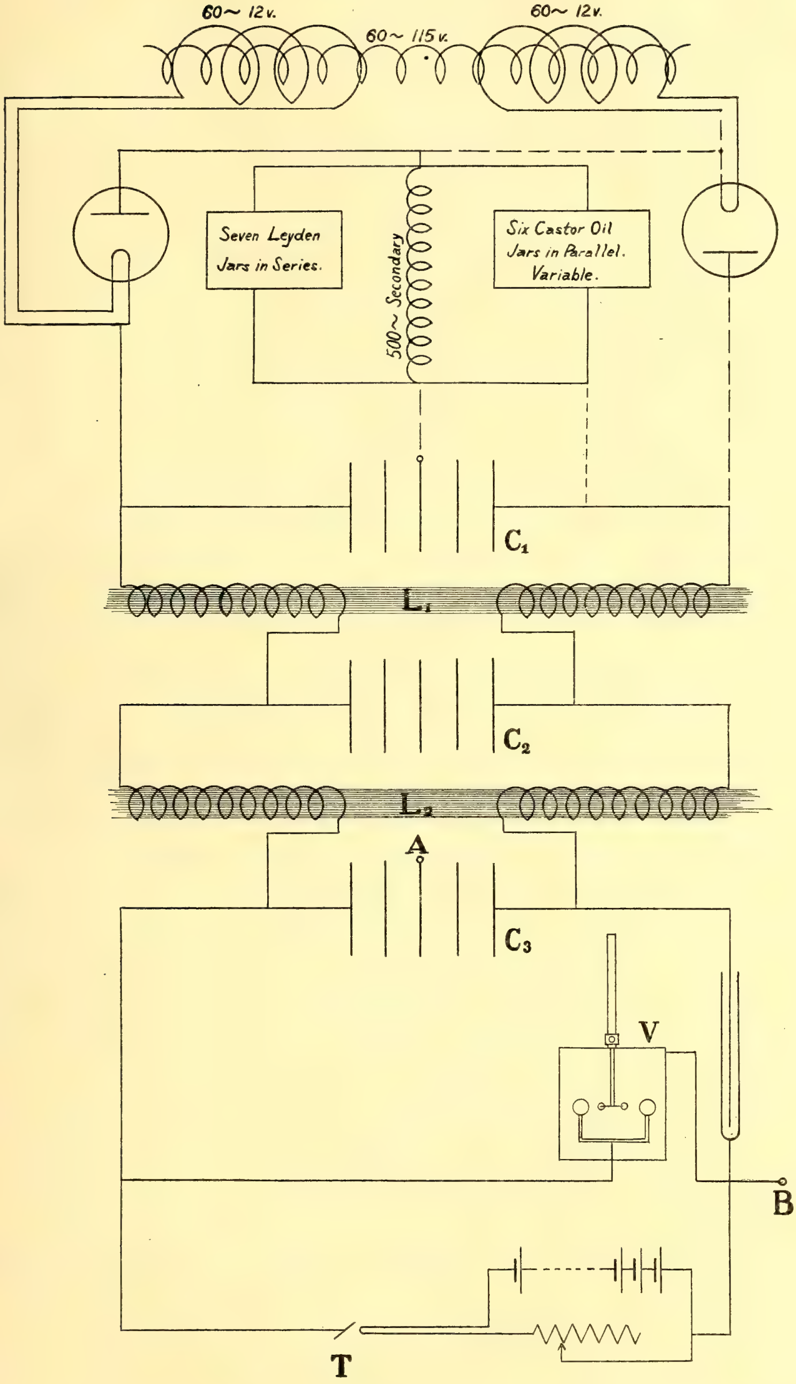
60~ 12v.
60~ 115v.
60~ 12v.
Seven Leyden Jars in Series.
500~ Secondary
Six Castor Oil Jars in Parallel. Variable.
C_1
L_1
C_2
L_2
A
C_3
V
B
T

FIG. 2

Note: Short-dash line, connection *A* only. Long-dash lines, connection *B* only. Ground only at point *A* at high potentials and only at point *B* at low.

tralize the slight remaining fluctuation, as at 2 meters distance, with L_1 and L_2 parallel, $M = 100$ henries. In this case, however, no attempt was made to adjust M accurately, but L_2 was tipped to a nearly vertical position, 2 meters from L_1, which was horizontal, and L_1 was set perpendicular to the 500 ∼ transformer, and also to the 60 ∼ one, which were both horizontal and far apart.

An important point is the condensers across the secondary of the transformer. To eliminate unsteadiness due to fluctuations on the 115-volt D. C. line, that ran the motor-generator, the generator field was excited by a storage battery and the variable condenser was then adjusted for resonance. Under these conditions, the potential on C_1 being a maximum with respect to speed changes, a small change in the speed of the motor could affect it only by a second order amount, always negative, therefore, not such as to bring out spectrum lines falsely. This arrangement proved invaluable in making long exposures. The only fluctuations that ever made trouble were small ones due to current changes produced by the traces of gas that cannot be removed from the tube without melting the platinum target to do so.

I wish here to express my thanks to Mr. R. M. Frye for valuable assistance in the construction of this apparatus.

The X-ray tube was the same one used in the previous work, but, through the kindness of Dr. Coolidge, it had been provided with a thin window of a type used by Blake and Duane,[8] made by blowing a thin, spherical shell, convex inward. This greatly reduced the exposures required.

Results.—Considering each line separately, the results are as follows:

α_1. This line was definitely shown by the ionization method to belong to L_1, and fixed the critical potential of L_1 most accurately, the average of the best measurements being 11.47 ± 0.05 kv. Since conditions were adapted for intensity measurements but no attempt was made to measure wave-lengths, the value of h found there will not be used, but rather the value recommended by Birge,[9] which is 6.554×10^{-27} erg sec., with Millikan's e. This gives $\lambda_{L_1} = 1.076 \pm 0.005$ Å. The films show α_1 very strong when $\lambda_{min} = 1.01$ Å, and not at all at 1.08.

α_2. In the mica films this line is resolved from α_1 only in the 5th order, but as this order is strong, it shows clearly when λ_{min} is around 0.90 Å, and definitely, though faintly, at 1.01. Since the critical wave-length of L_2 is 0.935 Å (see β_1), this places α_2 in L_1. The ratio of its intensity to that of α_1 was tested at Harvard by the ionization method, with results shown in figure 3. Since the ratio is the same at 40.0 kv. as at 17.85, the law of constant intensity ratios for lines of the same series holds in this case.

Ir α_1. This line, due to an impurity of iridium in the target, shows faintly but definitely when $\lambda_{min} = 1.01$ Å, but not at 1.08.

l. This line is too faint to show near its critical potential in any order but the third, and there it is covered at high voltages by the fourth order β_1. But β_1 belongs to L_2, and even its strong third order line is gone at λ_{min} = 0.945 Å, while a line still appears at the position of 4 β_1 and 3 *l*. This must be 3 *l*. It is definitely visible though somewhat fainter than

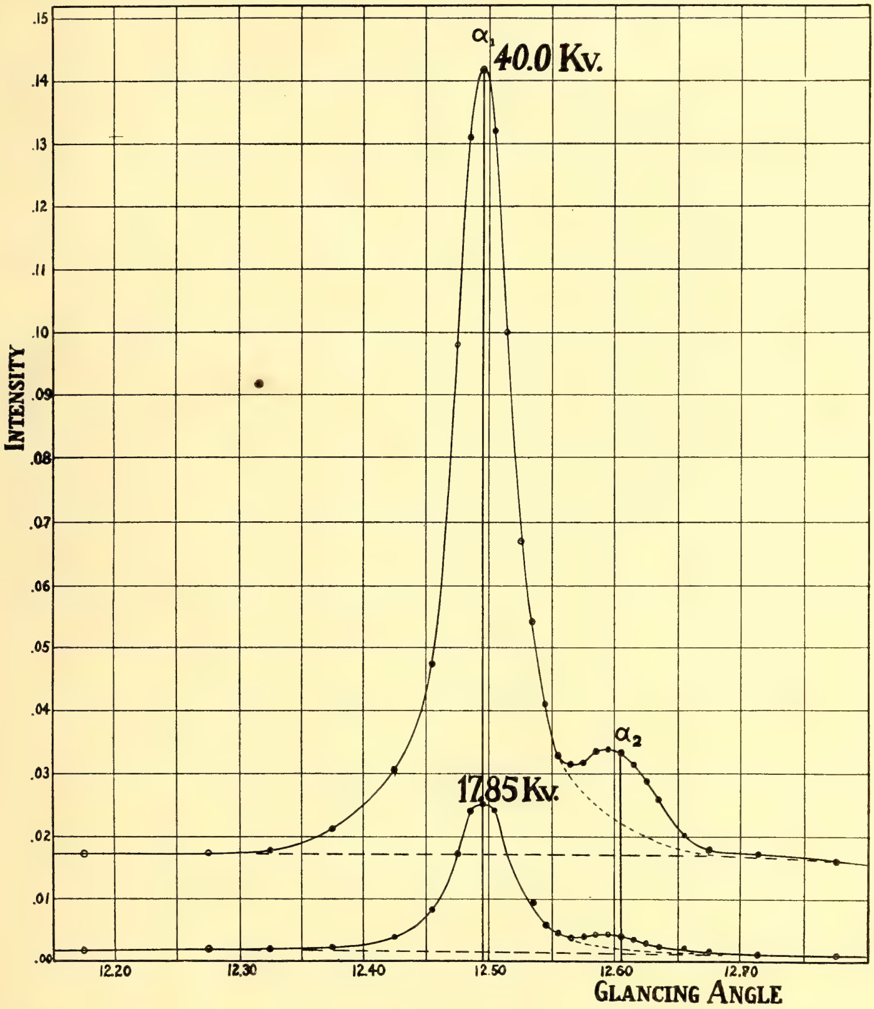

FIG. 3

Reckoning the intensities by measurement of the vertical lines drawn to the peaks in the figure, the ratio of α_1 to α_2 is 1.11 at 17.85 kv. and 1.10 at 4.00.

3 *Ir* α_1. It is doubtful at 1.01 Å, but the fact that it shows at 0.945 places it in L_1.

β_2. This line shows strongly when λ_{min} = 1.01 Å, but not at all at 1.08, as one might expect from the ionization work previously reported, which placed it in L_1.

β_5. This line shows well at high voltages, and faintly but distinctly when λ_{min} = 1.01 Å. This places it in L_1.

β_6. In the calcite spectrograms taken at Harvard there is a faint companion of β_5 at a wave-length greater by 0.010 ± 0.002 Å. This also shows in some high resolution mica films, where the tube was 3 meters from the mica instead of 75 cm., and in a few of the low resolution mica films, at λ_{min} = 0.88, 0.910 and 0.945 Å. In all the others β_5 is wide. The fact that it shows as low as 0.945 Å places it in L_1, and its intensities in the calcite films indicate that it follows qualitatively the constant intensity ratio law. At least it does not violate it in any such way as an L_2 line would when compared with an L_1. Since β_5 and β_6 are both in L_1 their wave-length difference would be nearly the same in tungsten as in platinum. β_6 is, therefore, probably the analogue of the tungsten line discovered by Dershem and given by Overn as 1.2212 Å, β_5 being his line 1.2097 Å.

β_1. This line belongs to L_2 and gave the most definite results by ionization for determining λ_{L_2}. With the present h this is 0.935 ± 0.004 Å. The films show β_1 plainly at 0.910 Å, but not at all at 0.945 Å even in the third order.

η. The third order of this line unfortunately coincides with the fourth of γ_2, which belongs, as noted below, to L_2. But the fourth order is faint and the third strong, and the fourth γ_1 which is very near 3 η and 4 γ_2 is but very little stronger than they. Since 3 γ_1 is much stronger than 3 γ_2 this means that most of the intensity of this combined line is that of 3 η. Now the combined line and 4 γ_1 show the same intensity ratio, as nearly as one can tell, at all voltages, and are visible down to λ_{min} = 0.910 Å, but not at 0.945. At these low voltages 4 γ_2 alone would certainly be invisible, and if η and γ_1 were not in the same series the change in their ratio would be unmistakable even with no measuring instruments. Hence, we may fairly confidently assign η to L_2.

β_4. This line gave results by ionization showing a critical wave-length 0.935 ± 0.02 Å, the accuracy not being equal to that obtained with β_1 because it is rather faint. The films, however, show a faint line in this position even at λ_{min} = 1.01 Å, far below the critical potential of L_2. This is probably the same as a line which is just resolved from Pt β_4 in the calcite and high resolution mica spectrograms. It is called Ir β_2 because its wave-length is that of the iridium line and it obviously does not belong to L_2. But as all lines are rather wider in the mica photographs and the work of Dershem and Overn showed a line in tungsten that might be near this point in platinum, further work with a high resolving power and long exposures is needed. As the present spectrograph is unsuitable for this and several other problems presented by the very faint lines, a more suitable one is now being constructed by Mr. F. C. Hoyt.

Ir β_1 *and Ir* β_2. These lines appear in the calcite and high resolution mica films, and behave like *Pt* β_1 and *Pt* β_2, the changes in relative intensities with voltage being very striking in both cases.

β_3. This line is obscured by β_2 except in the calcite films. Its behavior there is similar to that of the L_2 lines and quite different from that of the L_1. It must be in either L_2 or L_3.

λ_1. The films show this line clearly when $\lambda_{min} = 0.910$, but not at all at 0.945, confirming the ionization work that assigned it to L_2.

γ_2 *and* γ_3. These two lines are not resolved except in three films at 35, 25 and 18 kv. In these the tube was removed 3 meters from the mica instead of 75 cm. In this case the exposure at 35 kv. was 7 hours and at 18 kv., 30 hours. In these films γ_2 and γ_3 are just resolved in the third order and well separated in the fifth. These films were taken for three purposes, to confirm the existence of β_6, *Ir* β_1 and *Ir* β_2, to get the absorption spectrum discussed below, and to see if any difference could be found in the intensity ratio of γ_2 and γ_3 at different voltages. γ_3 in each film is fainter than γ_2, and it appeared slightly more so at 18 kv. than at 35, but the difference in the ratio was too small to base any definite conclusions on it, though if it exists they must belong to different series. The low resolution films show the combined line $\gamma_{2,3}$ well at high potentials, faintly but definitely at $\lambda_{min} = 0.895$ Å, and it is barely visible under the best conditions at 0.907. Since $\lambda_{\gamma_4} = 0.900$ Å, this line cannot be in the same series with γ_4 but must have a critical wave-length longer than 0.907 and, therefore, presumably belong to L_2 where $\lambda_{L_2} = 0.935$ Å. Now the absorption spectra discussed below indicate that γ_3 is distinctly more absorbed than γ_2, showing an absorption limit, A_2, corresponding to L_2, lying between them. This apparently means that γ_3 cannot belong to L_2 and, therefore, that γ_2 is probably the line showing at $\lambda_{min} = 0.907$ Å. But it does *not* mean definitely that γ_3 has the same critical potential as γ_4, although the natural assumption is that it has. This is important in connection with Sommerfeld's theory to be discussed in the next paper.

γ_4. This line is so near the bromine absorption limit that any work on it is uncertain, and with the mica spectrograph 3 γ_4 is obscured by 2 *Ir* α_1 and 5 γ_4 by 4 β_4. But since $\lambda_{\gamma_4} < \lambda_{L_2}$ or λ_{L_1} it can hardly belong to either of these series, and must be ascribed to L_3.

The Faint Lines.—In this class are included the lines found only by Dershem and Overn, in tungsten. For these, further work will be necessary, with high resolving power and very long exposures.

The Critical Points.—The question arises whether the critical wave-lengths determined by potentials are identical with the absorption limits, as they seem to be in the K series. The critical wave-lengths found by Clark and me for L_1 and L_2 were identical, within limits of error, with the wave-lengths of β_5 and γ_2, respectively, provided $h = 6.554 \times 10^{-27}$ erg sec., and, therefore the results of various measurements of absorption limits are compared with these lines in the following table:

TABLE 1

ELEMENT	λ_{A_1}	λ_{β_5}	$\lambda_{\beta_5}-\lambda_{A_1}$	λ_{A_2}	λ_{γ_2}	$\lambda_{\gamma_2}-\lambda_{A_2}$	λ_{A_3}	λ_{γ_4}	$\lambda_{\gamma_4}-\lambda_{A_3}$
Tungsten	1.230^{10}	1.2097^{3}	—0.020	1.081^{10}	1.0596^{3}	—0.021	1.025^{10}	1.0263^{3}	+0.001
Tungsten	1.215^{11}	1.2097^{3}	—0.005						
Platinum	1.070^{11}	1.072^{6}	+0.002						
Platinum	1.072^{12}	1.072^{6}	0.000	0.934^{12}	0.033^{6}	—0.001			
Gold	1.041^{11}	1.035^{6}	—0.006	0.901^{11}	0.898^{6}	—0.003	0.861^{11}	0.869^{6}	+0.008
Gold	1.042^{12}	1.035^{6}	—0.007	0.914^{12}	0.898^{6}	—0.016			
Mercury	1.009^{11}	1.013^{13}	+0.004						
Thallium	0.977^{11}	0.977^{6}	0.000	0.843^{11}	0.844_{6}	+0.001			
Lead	0.948^{11}	0.953^{13}	+0.005	0.814^{11}	0.820^{6}	+0.006			
Lead	0.949^{14}	0.953^{13}	+0.004	0.813^{14}	0.820^{6}	+0.007	0.781^{14}	0.792^{6}	+0.011
Bismuth	0.924^{11}	0.923^{6}	—0.001	0.789^{11}	0.794^{6}	+0.005	0.756^{11}	0.762^{6}	+0.006
Thorium	0.760^{11}	0.767^{13}	+0.007	0.627^{11}	0.637^{13}	+0.010	0.607^{11}	0.610^{13}	+0.003
Uranium	0.721^{11}	0.726^{13}	+0.005	0.591^{11}	0.598^{13}	+0.007	0.567^{11}	0.571^{13}	+0.004
Mean....................			—0.001			0.000			+0.006

From this table it would appear that A_1, A_2 and A_3 are probably at nearly the same wave-lengths as β_5, γ_2 and γ_4, respectively, in every element. But most of these results were obtained with different apparatus for the two kinds of measurement, and there is considerable disagreement. As even a slight difference in wave-length one way or the other is of great theoretical interest, it seemed desirable to check this point by using a screen of H_2PtCl_6 on blank films. This was attempted in two of the high resolving power spectrograms. The results were not very satisfactory, owing to the strong absorption by the other substances than platinum in the screen, and the presence of the less absorbed radiation of higher frequencies, in the fifth order, at the most interesting part of the third. But they seem to indicate that A_1 is within a very few thousandths of an Ångstrom of β_5, and that A_2 is between γ_2 and γ_3, γ_3 being apparently the more strongly absorbed of these lines. This is consistent with the conclusion reached above, that γ_2 belongs to L_2 and γ_3 to L_3, but the evidence is unsatisfactory and none was obtained on A_3. This apparatus was not designed for high resolving power but it is hoped that this point will be settled with the other apparatus now being constructed by Mr. F. C. Hoyt for work on the fainter lines.

Summary of Results.—We have assigned the lines to the various series as follows:

To L_1, l, α_2, α_1, β_2, β_6 and β_5;
To L_2, η, β_4, β_1, γ_1, γ_2 and perhaps β_3;
To L_3, γ_4, probably γ_3 and perhaps β_3.

Unassigned, all of Dershem's and Overn's faint lines except β_6, which is probably one of them.

Critical points:
$\lambda_{L_1} = 1.076 \pm 0.005$ Å, approximately $= \lambda_{\beta_5}$;
$\lambda_{L_2} = 0.935 \pm 0.004$ Å, approximately $= \lambda_{\gamma_2}$;
λ_{L_1}, λ_{L_2} and λ_{L_3} are probably exactly equal to λ_{A_1}, λ_{A_2} and λ_{A_3} though more work is needed on this point.

Intensity ratios: constant for any pair of lines of the same series, with changes in voltage, as far as they have been investigated.

[1] Webster, D. L., and Clark, H., these PROCEEDINGS, **3**, 1917 (181–185).

[2] Dershem, E., *Physic. Rev., Ithaca,* **11**, 1918 (461–476).

[3] Overn, O. W., *Ibid.,* **13**, 1919 (137–142).

[4] Webster, D. L., *Ibid.,* **9**, 1916 (599–613).

[5] De Broglie, M., and Lindemann, F. A., *C. R. Acad. Sci., Paris,* **158**, 1914 (944); de Broglie, M., *J. Physique, Paris,* **4**, 1914 (265–267); see also independent invention by Rohmann, H., *Physik. Zs., Leipzig,* **15**, 1915 (510–512).

[6] Siegbahn, M., and Friman, E., *Phil. Mag., London,* **32**, 1916 (39–49); or Siegbahn, M., *Jahrb. Radioakt. Elektronik, Aachen,* **13**, 1916 (296–341).

[7] Hull, A. W., *G. E. Review, Schenectady,* **19**, 1916 (173–181).

[8] Blake, F. C., and Duane, W., *Physic. Rev., Ithaca,* **10**, 1917 (625).

[9] Birge, R. T., *Ibid.,* **14**, 1919 (361–368).

[10] Duane, W., and Shimizu, T., *Ibid.,* **14**, 1919 (67–73).

[11] De Broglie, M., *J. Physique, Paris,* **6**, 1916 (161–168), with an addition of 1.5′ to all angles as advised by him in a personal communication.

[12] Wagner, E., *Ann. Physik, Leipzig,* **46**, 1915 (868–893).

[13] Calculated from Siegbahn and Friman's wave-lengths of other lines.

[14] Duane, W., and Shimizu, T., these PROCEEDINGS, **5**, 1919 (198–200).

THE EFFECT OF PHYSICAL AGENTS ON THE RESISTANCE OF MICE TO CANCER

BY JAMES B. MURPHY

ROCKEFELLER INSTITUTE FOR MEDICAL RESEARCH, NEW YORK CITY

Read before the Academy, November 10, 1919

The report which I wish to present today has to do with further progress in the work which I reported before the Academy in June, 1915. I will review that work briefly in order to orient you with the observations made since then.

The fundamental point in immunity to transplanted cancer is that there are two types of resistance, the so-called natural and induced immunity. Mice may be rendered resistant by an injection of a quantity of homologous living tissue given at least a week or ten days before the cancer inoculation; this is called induced immunity. A variable proportion of mice inoculated with a transplantable tumor will be resistant; this is called natural immunity. The histological manifestation of resistance about an introduced cancer graft in these two types of immunity is the same and is characterized by a lymphoid infiltration of the tissues around, and later, an actual invasion of the graft.

We first showed that there was an actual increase in the circulating lymphocytes, accompanied by hyperplasia of the lymphoid elements following cancer inoculation into immune animals. If this crisis was prevented by the destruction of the lymphocytes the immunity of the animal was destroyed. Furthermore, animals with tested immunity could be rendered susceptible by a similar destruction of the lymphocytes. The method we used for the destruction of the lymphocytes was that of small repeated doses of X-rays. It was established that this procedure would practically deplete the animal of lymphoid tissue without apparent injury to other important structures and without detectable influence on the animal's general health.

In the course of this investigation of the action of X-rays it was noted that while repeated small or a single large dose would destroy the lymphoid tissue, a single, small exposure to a ray of suitable quality would stimulate the lymphocytes. The stimulation was judged by the numbers of circulating lymphocytes and the state of activity of the germinal centers in the spleen and lymph nodes as indicated by the number of mitotic figures present. This method of increasing the lymphoid elements of the body was first used in an attempt to increase the resistance of mice to replants of their own spontaneous tumors. By this treatment we increased the resistance from 3.4% in our control series to 50% in the treated series. By a similar induced lymphocytosis we have been able to develop a resistance to transplanted cancers which compares most favorably with the immunity following tissue injection. The procedure here was to expose a number of mice to a stimulating dose of X-rays and then inoculate them with a transplantable cancer a week later, at the same time inoculating a series of controls. The following table gives the result of these experiments:

EXPERIMENT NUMBER	I	II	III	AVERAGE
Takes in X-rayed mice	25.0%	29.0%	28.6%	27.5%
Takes in control mice	77.8%	87.5%	60.0%	75.1%

We next turned our attention to finding other methods of producing lymphoid stimulation. Several agents have been found other than X-rays, but only one so far which gives a reaction extensive enough and of sufficient duration for experimental purposes. Dry heat ranging from 55 to 65° C. applied to the whole animal for five minutes was found to produce a stimulation of the lymphocytes and of lymphoid tissue of marked intensity, the increase in the number of lymphocytes persisting for a number of days. Mice treated in this manner and then inoculated with cancer a week later showed a striking degree of immunity as compared with the control animals inoculated with the same tumor at the same time. The following table gives the result of these experiments:

EXPERIMENT NUMBER	I	II	III	AVERAGE
Takes in heated mice.....	30.0%	30.0%	50.0%	36.6%
Takes in control mice.....	77.8%	78.8%	94.5%	83.6%
Number of mice used.....	20	33	54	Total 107

The effect of heat stimulation was also tested on the resistance of mice with spontaneous tumors to replants of their own cancers. The result in this case was even better than that obtained with the small doses of X-rays. The heated mice had an immunity of 59.4% as compared with 50% in the X-rayed series and 3.4% in the untreated animals.

It will be noted that in our experiments with the transplanted tumors we made our transplantations one week after exposure to the heat or the X-rays. This time was selected at first because the greatest stimulation occurred then. Further experiments indicated that if animals treated by either of these methods were inoculated during the first two or three days after treatment there was either no immunity or very slight evidence of it. The following table illustrates this point:

	AVERAGE
Takes in mice X-rayed 1 week before inoculation........................	27.7%
Takes in mice X-rayed 1 to 3 days before inoculation....................	60.0%
Takes in control mice..	67.0%
Takes in mice heated 1 week before inoculation.........................	36.6%
Takes in mice heated 1 to 3 days before inoculation....................	77.7%
Takes in control mice..	86.2%

This observation offers a very interesting parallel with that on immunity induced by tissue injection. The immunity is non-operative during the first few days and is at its best between seven and ten days after the injection. It seems to me of considerable interest that we are able to induce the same degree of immunity by the use of physical agents as by the use of what we may call a biological agent. The points in common are: the lymphocytosis induced by the physical agents, which occurs normally after the use of the biological agent, and the latent period before the immunity, become evident after the use of all three of the agents.

Summary.—It was first shown that animals immune to transplantable cancer showed a large increase in the circulating lymphocytes after cancer inoculation. This reaction is accompanied by hyperactivity of the lymphoid tissue of the spleen and lymph nodes. If the reaction is prevented by the destruction of the lymphoid tissue the immunity is destroyed, and the animal becomes susceptible to cancer inoculation.

Two methods have been found whereby the lymphocytes may be markedly increased along with a heightened activity of the germinal lymphoid centers: (1) by exposure to diffuse, small doses of X-rays; and (2) by exposure to dry heat. The lymphocytosis and lymphoid hyperplasia induced by these two physical agents are associated with an immunity to transplanted cancer equally as great as that arising from tissue injections.

Furthermore, these methods are capable of increasing the resistance of mice to replants of their own spontaneous tumors. From the evidence at hand it seems then that the immunity aroused by these two physical agents has at least one other point of similarity to that induced by tissue injection, namely, a period of latency after the exposure before the immunity becomes evident. Whether the tissue injection, the small dose of X-rays, or the dry heat induce changes in the organism other than those associated with increase in the lymphoid tissue which would account for the immunity, is impossible to state at the present time; but the evidence now at hand points at least to the lymphoid tissue as an important agent in the immunity reaction to transplanted cancer of mice.

The work reported in this paper was carried out with the assistance of Herbert D. Taylor, John J. Morton, W. D. Witherbee, Waro Nakahara, and Ernest Sturm.

THE PROTEIN REQUIREMENT OF MAINTENANCE IN MAN

BY H. C. SHERMAN

DEPARTMENT OF CHEMISTRY, COLUMBIA UNIVERSITY

Communicated by W. A. Noyes, November 17, 1919

During the past four years a number of experiments have been carried out to test the nutritive requirements of maintenance in healthy men and women, and the efficiency of diets derived chiefly from the cereal grains in meeting these requirements. In connection with this work the results of all available data from previous investigations which seemed to lend themselves to direct quantitative comparison, have been brought together.

Probably the best present indication of the amount of protein or nitrogen actually required for the maintenance of the average adult is to be obtained by averaging the observed output of nitrogen in all available experiments, upon normal men and women, in which the energy value of the food was appropriate to the size and activity of the subject and the intake of nitrogen appears to have been just about sufficient to result in equilibrium of intake and output. Since a considerable and rather variable amount of time is required for the body to adjust its rate of nitrogen output to the rate of intake, it is probable that the "indicated protein requirement" obtained by averaging all available experiments will be somewhat greater than the minimum amount of protein on which the same subjects could actually have established and maintained equilibrium had the experiments been sufficiently prolonged. In other words, for the practical purpose of indicating how much protein the food must furnish in order to provide for adult maintenance such an average will err on the side of safety in that it may be expected to be appreciably above the minimum which would actually suffice to keep normal individuals in equilibrium. Two other influences tending toward an over-estimate of the protein requirement should also be noted since one or both of them appear to have played a significant part in the great majority of experiments which have been made in the past. These are: First, the tendency in experiments of this sort to reduce the intake of food as a whole, thus creating a deficit in the energy supply, which must interfere with the economical use of protein; second, hesitation, because of past over-estimates of the protein requirement, to reduce the amount of protein in the food to a sufficiently low figure to really test the minimum on which equilibrium could be established and maintained.

Every effort has here been made to decide in an objective manner which of the past experiments should be included in the average; yet an element of judgment necessarily enters into the selection, especially as some experiments have been more carefully planned and more rigorously controlled than others.

According to the rigorousness of the selection, the results may be given as follows: First, 109 experiments show a range of 21 to 65 grams and an average of 44.4 grams of protein per 70 kg. of body weight per day; second, 94 of these experiments show a range of 29 to 56 grams with an average of 42.8 grams; third, 76 of the same experiments show a range of 30 to 50 grams with an average of 40.6 grams. For convenience of comparison, all the results whether the subjects be men or women were here reduced to a uniform basis of 70 kg. of body weight—the usual basis for the statement of food requirements "per man per day."

These data reveal no evidence of a sex difference in protein requirement, since 67 experiments upon men indicated a requirement of 0.633 gram and 42 experiments upon women indicated a requirement of 0.637 gram of protein per kilogram of body weight per day.

In our own more recent experiments, including both men and women, although the protein of the food was almost entirely that of the cereal grains, the amounts required for maintenance have usually been less than the averages above given. Thus, a man of 80 kg. established and maintained equilibrium on a daily intake of 37.5 grams of protein, of which 96% was furnished by ordinary wheat bread. A woman of 55 kg. found 27 grams of maize protein (from corn meal) almost sufficient, while 30 grams per day of protein, of which 88% was from corn meal, 10% from milk, and 2% from apple, proved more than sufficient. The same subject showed essentially the same requirement when oatmeal was substituted for corn meal as the chief source of protein, and a second woman subject also found fully adequate a diet which furnished not over 0.6 gram of protein per kilogram of body weight, about nine-tenths of the protein being derived from oatmeal and one-tenth from milk.

Thus it appears that in the maintenance of healthy men and women an intake of not over 35 to 45 grams of protein "per man" of 70 kg. per day is sufficient, even when the protein is not selected as of especially high nutritive efficiency, and that a "standard" allowance of 1 gram protein per kilogram of body weight per day provides a margin of safety of from 50 to 100% above the minimum amount actually required to maintain equilibrium.

For growth and reproduction the amounts of protein required are relatively larger than for maintenance, and the selection of the protein is also of greater importance.

The data of all of the experiments here considered are summarized, with fuller discussion and with citation of original sources, in a paper which is being offered for publication in the *Journal of Biological Chemistry.*

THE TRANSPLANTING OF SEA ANEMONES BY HERMIT CRABS

By R. P. Cowles

Department of Zoölogy, Johns Hopkins University

Communicated by H. S. Jennings, November 6, 1919

It is well known that hermit crabs inhabit the vacant shells of molluscs, that as they grow they change from smaller shells to shells of larger size and that hydroids, sea anemones and other animals are frequently attached to these shells. Several observers have reported that they have seen hermit crabs remove sea anemones from their old habitations and transplant them on their new shells. Such observations are mentioned in a few zoölogical text-books but usually the treatment is such as to leave the impression that there is an element of doubt as to the occurrence of the habit. For this reason I shall describe the behavior of individuals belonging to two species of hermit crabs, *Pagurus deformis* and *Pagurus asper*, occurring in the sea about the Philippine Islands, which bear sea anemones on their shells (Dolidae, Strombidae, Cassis, etc.) and which transfer these animals when they change their shells. My observations are worthy of note since they were made in the presence of four co-workers (Dr. L. E. Griffin, Mr. Alvin Seale, Professor A. L. Day and Professor S. F. Light), and since the individuals of the species of hermits mentioned both usually carry two kinds of sea anemones, one, a species whose individuals are large, the other, a species whose individuals are small, the former being fixed, as a rule, on the top of the shell and the latter generally on the under side near the protruding head of the hermit crab.

The following account of the behavior of the hermit crabs and sea anemones with reference to one another is undoubtedly in harmony with what occurs in nature although it deals with observations made in the laboratory: A hermit inhabiting a shell bearing two large sea anemones on the upper surface and a small one in the mouth of the shell was placed in an aquarium containing a clean, empty Dolium shell of a larger size than the one in use. The hermit began to examine the new shell almost immediately, put its pinchers and some of its legs inside of the shell and remained in this position for a considerable length of time. Occasionally the pinchers were moved about and were used apparently to examine the outside of the shell. Then, suddenly, the abdomen was inserted into the new shell and the hermit without moving away grasped one of the large anemones clawing, pinching and pulling it. The anemone instead of contracting as it would if disturbed by the observer remained expanded even though the mauling it received was really very strenuous and although it was frequently jammed up against the newly occupied shell. After these movements had continued at least for ten minutes the base of the anemone began to loosen in several places. Instead of completing its work at once the hermit began to pinch and claw the other large anemone which withdrew its tentacles, soon however protruding them again. Then suddenly the hermit left the second anemone, picked off the small, white one and applied it at the mouth of the new shell almost directly under its own head, where it soon became firmly attached. The crab now attacked again one of the large anemones, removed it easily, turned it round and round between its legs and pinchers and finally pushed the base against the side of the new shell. It failed to become attached, slipped down on the hermit's legs, stuck to them for a moment, dropped off and finally attached itself on the glass bottom of the aquarium. Again the hermit crab seemed to lose interest in the anemone on which it was working and began tugging, pinching and scratching the only remaining anemone attached to its old shell. After a long period of handling and some little prying up of the edges of the base of the anemone, during which time the tentacles contracted completely, the anemone suddenly slipped off apparently of its own accord. (Strange as it may appear the crab seems never to cause any injury although the base of the anemone is very thin and when taken off by man is easily torn.) The hermit now caught the anemone which had just slipped off, rolled it around and around for a few seconds and then with the aid of the hind legs applied the tentacle-side against the shell. Again the anemone slipped off and was disregarded for a moment while the hermit picked up the other anemone. Still holding the latter it lifted up the one that had just slipped off and rolled them both around for some little time. Finally one became attached to the side of the shell by one edge of the base and was left to shift for itself. The other was passed up to the opposite side of the shell with the aid of the posterior legs of the hermit and soon both anemones became firmly attached with their tentacles beautifully expanded. While the actions of this hermit crab were not nearly so accurate as those of some others I have observed, yet like all of them it showed a constant and hurried activity which seemed to indicate that some inherited nervous condition existed which was directing its actions. Furthermore, during the time in which this remarkable behavior was taking place the hermit crab, although at other times reacting quickly to movements outside of the aquarium, showed almost no reaction to the stimuli which are ordinarily produced by people passing or moving in front of the aquarium. This account describes a single series of observations but several other series were made in which the behavior was similar.

It seems probable that the anemones become associated with the hermit crabs very early in life, and that possibly they become attached in larval life to mollusc shells inhabited by hermits.

While the anemones are undoubtedly benefited by their association with the hermit crabs since the latter lead a wandering life in heavy growths of eel grass which harbor many small organisms, and while they may often take morsels from decaying animals upon which the hermits feed, the writer has never seen the hermits transfer pieces of food to the sea anemones as has been described by one investigator.

In the absence of any experimental work on these animals it cannot be proved that the sea anemone affords any protection for the hermit but it seems probable that stinging cells of the former would prevent the hermits being eaten by fishes.

We cannot believe that the hermit crab during its life time has learned by experience that it derives any advantage from transplanting the sea anemone although we know that crabs in general do profit by experience, and yet assuming that the remarkable behavior of the hermit is due to instinct, that is, to an "inherited combination of reflexes" which have been so brought together by the nervous system that the behavior has become fixed and adaptive in the species, it is extremely difficult to conceive how it has come about.

SPECTRA OF EXPLOSIONS

By J. A. Anderson

Mount Wilson Observatory, Carnegie Institution of Washington

Communicated by G. E. Hale, December 2, 1919

Some experiments with explosions of fine, iron wires by electrical means have led to the development of a method of producing a brilliant continuous spectrum extending throughout the visible spectrum and as far into the ultra-violet as can be recorded by a quartz spectrograph. On the continuous background appear as absorption lines practically all the iron lines usually found in the arc. For example, in the green region between $\lambda 5150$ and $\lambda 5700$, which is unfavorable for reversals in general, this spectrum on a scale of 6*A* per millimeter shows 128 absorption lines due to iron; Burns' table for the same region contains 215 lines, and

Rowland gives 180 lines of intensity 1 or higher, as due to iron. Both of these are from spectrograms having a scale of 1A per millimeter.

The apparatus employed consists of a small 25,000 volt transformer with a mechanical rectifier to charge a glass plate condenser of 0.4 microfarad capacity. The condenser is discharged through a circuit consisting of a spark gap 2 cm. long, in series with which is a fine iron wire 5 cm. long and weighing about 2 mg. When the condenser discharges the wire explodes with a loud report and a brilliant flash of light, and if it is enclosed in a tube of wood having an internal diameter of 1 cm. or less, the continuous spectrum is observed on viewing the tube, end on. The average duration of the explosion is of the order of 10^{-5} seconds, and ten explosions are sufficient to record a fully exposed spectrum of the λ4000 region, using a grating spectrograph giving a dispersion of 6A per millimeter. An exposure to the sun in this region requires $^1/_{40}$ second, from which it follows that this new source of light is of the order of 100 times as bright as the sun. With a quartz spectrograph the ultra-violet region at λ2300 (scale 4A per millimeter) is well recorded with 10 explosions, and the region at λ3500 (scale 20A per millimeter) with 1 or 2 explosions.

A few trial exposures have been made using nickel, copper, and manganin wires. These indicate that with nickel, the spectrum is considerably brighter than with iron, while with copper it is much weaker. Manganin and iron give about the same intensity.

With a larger condenser and higher voltage it is hoped that the brightness may be materially increased.

REPORT OF THE AUTUMN MEETING

PREPARED BY THE HOME SECRETARY

The Autumn Meeting of the Academy was held at Yale University, New Haven, Ct., on November 10 and 11, 1919.

Fifty-six members were in attendance, as follows: Messrs. C. G. Abbot, Bailey, Benedict, Boltwood, Bumstead, Cannon, Cattell, Chittenden, Cross, Cushing, Dana, Davenport, Davis, Day, Donaldson, Elkin, Flexner, Gomberg, Gooch, E. H. Hall, Harrison, Hastings, Henderson, Howard, Jennings, Johnson, Kasner, Leuschner, Lusk, Lyman, Mayor, Meltzer, Mendel, C. E. Mendenhall, Millikan, E. S. Morse, E. F. Nichols, A. A. Noyes, H. F. Osborn, T. B. Osborne, Parker, Pearl, Prudden, Russell, Schuchert, Setchell, Squier, Stratton, A. Trowbridge, Verrill, Walcott, Webster, H. S. White, Edmund B. Wilson, Edwin B. Wilson, R. W. Wood.

BUSINESS SESSIONS

The President announced the deaths of W. G. Farlow, elected to membership in the Academy in 1879; Joseph Barrell, elected in 1919; Lord Rayleigh, elected foreign associate in 1898; and Emil Fischer, elected foreign associate in 1904.

The Home Secretary stated that a Section of Engineering had been established and that it is now constituted as follows: Messrs. H. L. Abbot, J. J. Carty, W. F. Durand, J. R. Freeman, H. M. Howe, F. B. Jewett, G. O. Squier, D. W. Taylor. All members of the Sections of Physics and Chemistry were given an opportunity to remain with the section with which they had been affiliated or to be placed in the Section of Engineering.

Messrs. L. O. Howard, C. E. Mendenhall, and David White were appointed to audit the accounts of the Treasurer.

Messrs. J. C. Merriam, Gano Dunn, L. J. Henderson, A. L. Day, and W. J. V. Osterhout were elected members, and Mr. Raymond Pearl was re-elected chairman, of the Editorial Board of the PROCEEDINGS, to serve for three years. Mr. E. B. Wilson was re-elected Managing Editor for one year.

The following cable, prepared by the Foreign Secretary at the request of the President and forwarded to *Nature*, was read:

"The President of the National Academy of Sciences requests me to offer his congratulations to *Nature* on the occasion of its jubilee. During a period of specialization *Nature's* extensive survey of the progress of research has stimulated wider vision and larger effort. In spite of repeated discouragement it has urged upon the statesmen of two generations the vital importance of science to the nation. At a time when the branches of science, no longer isolated, are uniting in common channels and when governments, once unappreciative, are recognizing the bearing of research on national security and public welfare, we rejoice in *Nature's* expanding influence and the higher opportunities opening to it in a newly ordered world."

The President reported that Mrs. Mary Clark Thompson had completed the establishment of a fund amounting to $10,000, the income of which is to be applied to a gold medal of appropriate design, to be awarded annually by the Academy for the most important services to geology and palaeontology. The medal is to be known as the MARY CLARK THOMPSON GOLD MEDAL. Mrs. Thompson previously gave an additional $1,000 for the preliminary expenses of dies, etc. The following recommendation, in acknowledgment of this gift, was adopted:

"That the National Academy of Sciences express to Mrs. Mary Clark Thompson the appreciation of its members, especially those working in the fields of geology and palaeontology, for this munificent gift to promote the recognition of research in these subjects."

In accordance with the recommendations of the Henry Draper Committee, the following grants and award of medals were approved:

1. $400 to Dr. S. A. MITCHELL, Director of the Leander McCormick Observatory, University of Virginia, to complete the purchase of a measuring microscope for use in the photographic determination of stellar paral-

laxes, on the basis of observations made with the 27-inch refracting telescope. The Academy awarded the sum of $250 from the Draper Fund to Dr. Mitchell in 1916 to apply on the purchase of this instrument. The microscope cost $650. The proposed grant of $400 will complete the purchase, in effect making the instrument the property of the Academy, and Professor Mitchell will devote an equivalent sum, $400, to the other needs of his parallax research.

2. $300 to Dr. JOEL STEBBINS, Professor of Astronomy in the University of Illinois, to assist in the further development and application of the photo-electric cell photometer.

3. $400 to Dr. FRANK SCHLESINGER, Director of the Allegheny Observatory, to enable him to test an automatic zenith camera for the determination of terrestrial latitudes with the expectation that the results will be more accurate than any hitherto obtained by other means. It is proposed that this instrument be mounted at least temporarily at the International Latitude Observatory, Ukiah, California, where the astronomer in charge, Mr. Neubauer, will operate it for a year or two as a labor of love. The grant is needed to install the instrument at Ukiah and to make certain auxiliary apparatus required in its operation.

The award of the HENRY DRAPER GOLD MEDAL to ALFRED FOWLER, F.R.S., Professor of Astrophysics, Imperial College, South Kensington, London, at the time of the stated meeting in April 1920, for his researches in celestial and laboratory spectroscopy, which have led to a valuable increase of our knowledge of sunspots, comets, and the stars—especially red stars of Secchi's Type III.

At the dinner held at the Hotel Taft on November 11, the DANIEL GIRAUD ELLIOT MEDAL AND HONORARIUM was presented to Mr. C. WILLIAM BEEBE, of the American Museum of Natural History, New York, for his work, *Monograph of the Pheasants.* Dr. Osborn, Chairman of the Committee on the Award of the Elliot Medal, outlined briefly the history of this award, and Mr. Beebe responded, giving a short, interesting talk about his researches on pheasants. Immediately following the dinner, President Walcott presented the following recommendations from the Council, which were unanimously adopted:

"That the medal for eminence in the application of science to the public welfare be awarded at the Annual Meeting in April 1920, to HERBERT C. HOOVER for his applications of science in the conservation, selection and distribution of food."

"That the Home Secretary be requested to transmit the thanks of the Academy to Yale University, President Hadley, the Board of Governors of the Graduates' Club, and to the members of the Academy forming the local committee, for the courtesies extended to the members of the National Academy of Sciences during the Autumn Meeting of 1919."

SCIENTIFIC SESSIONS

The following papers were presented at the scientific sessions, November 10 and 11. One asterisk denotes presentation by invitation; two asterisks, presentation only by title; a dagger denotes that the article has been offered for publication in the PROCEEDINGS.

W. M. Davis: Four cliff islands in the coral seas.

Edward Kasner: Some new theorems in the dynamics of a particle.

*Henry Laurens and Henry D. Hooker: The relative physiological efficiency of spectral lights of equal radiant energy content.

*Richard S. Lull: A study in synthetic paleontology.
**Biographical memoir of Samuel Wendell Williston.

*Yandell Henderson: The adjustment to the barometer of the haemato-respiratory functions in man.†

*George A. Baitsell: Development of connective tissue in the amphibian embryo.†

C. G. Abbot: A new method for determining the solar constant of radiation.†

*Leigh Page: A kinematic interpretation of electromagnetism.†

C. B. Davenport and Albert G. Love: Defects found in drafted men.

*James B. Murphy: The effect of physical agents on the resistance of mice to cancer.†

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CONTENTS

Page
PHYSICS.—AN EXAMPLE OF TORSIONAL VISCOUS RETROGRESSION....*By C. Barus* 1
CHEMISTRY.—THE CHEMICAL ISOLATION OF VITAMINES.......................... *By C. N. Myers and C. Voegtlin* 3
ASTRONOMY.—A NEW METHOD OF DETERMINING THE SOLAR CONSTANT OF RADIATION..*By C. G. Abbot* 4
PHYSIOLOGY.—THE BASAL METABOLISM OF BOYS FROM 1 TO 13 YEARS OF AGE. *By Francis G. Benedict* 7
CHEMISTRY.—THE NATURE AND FUNCTION OF THE ANTINEURITIC VITAMINE. *By R. Adams Dutcher* 10
PALAEONTOLOGY.—RECONSTRUCTION OF THE SKELETON OF THE *Sauropod Dinosaur Camarasaurus* COPE (*Morosaurus* MARSH)..*By H. F. Osborn and C. C. Mook* 15
RESTORATION OF *Camarasaurus* AND LIFE MODEL......*By W. K. Gregory* 16
PLATO'S ATLANTIS IN PALAEOGEOGRAPHY...............*By W. D. Matthew* 17
CHEMISTRY.—THE IONIZATION AND ACTIVITY OF LARGELY IONIZED SUBSTANCES. *By A. A. Noyes and D. A. MacInnes* 18
MATHEMATICS.—THE COMMUTATIVITY OF ONE-PARAMETER TRANSFORMATIONS IN REAL VARIABLES..*By A. C. Lunn* 24
PHYSICS.—THE INTENSITIES OF X-RAYS OF THE L SERIES II. THE CRITICAL POTENTIALS OF THE PLATINUM LINES.........................*By D. L. Webster* 26
PATHOLOGY.—THE EFFECT OF PHYSICAL AGENTS ON THE RESISTANCE OF MICE TO CANCER..*By J. B. Murphy* 35
CHEMISTRY.—THE PROTEIN REQUIREMENT OF MAINTENANCE IN MAN........... *By H. C. Sherman* 38
ZOÖLOGY.—THE TRANSPLANTING OF SEA ANEMONES BY HERMIT CRABS.......... *By R. P. Cowles* 40
PHYSICS.—SPECTRA OF EXPLOSIONS........................*By J. A. Anderson* 42
REPORT OF THE AUTUMN MEETING.. 43

VOLUME 6 FEBRUARY, 1920 NUMBER 2

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AN APPARENT HIGH PRESSURE DUE TO ADSORPTION, THE HEAT OF ADSORPTION, AND THE DENSITY OF GAS-MASK CHARCOALS

By William D. Harkins and D. T. Ewing

Kent Chemical Laboratory, University of Chicago

Communicated by J. Stieglitz, December 27, 1919

A knowledge of the volume of the pores, and of the density of the active carbon in gas-mask charcoal, is of importance in connection with the working out of a theory in regard to the efficiency of charcoal as an adsorbent of gases, so the problem of the determination of the pore volume was given to us by the National Research Council in connection with the work of problem 108. We have discovered that factors which have hitherto been unsuspected complicate such a determination, the interesting new phenomenon being that the volume of the liquid absorbed depends upon the physical properties of the liquid. Thus it has been found that the volume of water or of an organic liquid taken up by a definite weight of any charcoal which is very efficient as an adsorber of gases, increases as the compressibility of the liquid increases, or as either the viscosity or the surface tension decreases. This effect decreases as the adsorptive capacity decreases, and entirely disappears when the charcoal becomes valueless as a gas adsorbent.

While effects of the same general order were found whether the charcoal was put in the liquid directly from the canisters or from the bottles in which it was stored, or only after an extensive "outgassing" or heating and pumping out of the gases contained in it, concordant results were obtained only by the latter procedure. This is a well-known method for cleaning the surface of the charcoal, and the only new features introduced were those which concerned the completeness of the outgassing. The tube containing the charcoal was first exhausted by a rapid mercury condensation pump, and then heated up very slowly in vacuo until the temperature finally reached 600° C., keeping the pump running. The heating and evacuating usually continued for two days, though this time varied greatly with the nature of the charcoal, and was only ended when the pressure in the system fell to one ten-thousandth of a millimeter at this high temperature, and remained of that order for about six hours. The tube was sealed off while still at 600°, and was then opened under the hot liquid which had just been boiled to drive off dissolved gases. In some cases the charcoal was treated with the vapor of the liquid while it was still in the vacuum apparatus, and was then subjected to a second outgassing. It was found that it was not sufficient to reduce the pressure to 0.0001 mm. but that this low pressure had to be maintained for hours, if concordant results were to be obtained. Those who are familiar with vacuum work will realize that when a bulb almost completely filled with active charcoal is kept at 0.0001 mm. for hours at 600° C., a very low pressure when the temperature is considered, and is sealed off at this temperature and pressure, the pressure on cooling will fall to an exceedingly low value. It is probable that this pressure is much lower than can be obtained with the extremely efficient condensation pumps, that is, the pressure is probably lower than has ever been measured.

A few of the results obtained by us for a high-grade, steam-activated charcoal (E 602), a 47.2 minute charcoal as determined by the accelerated chlorpicrin test, or about 700 minutes by the ordinary test, are given in table 1. The densities given here are those which the carbon would have if it filled all the space not occupied by the liquid, and if the liquids were incompressible. The densities given are the averages of very closely agreeing values.

TABLE 1

DENSITY AND PORE-VOLUME DETERMINATIONS FOR A STEAM-ACTIVATED COCOANUT SHELL GAS-MASK CHARCOAL (DATA AT 20° C.)

1 LIQUID	2 DENSITY	3 PORE VOLUME IN 1 CU. CM.	4 PER CENT COMPRESSED 12000 ATMOSPHERES[1]	5 SURFACE TENSION DYNES PER CM.	6 VISCOSITY IN ABSOLUTE UNITS
Mercury	0.865			57.0	
Water	1.843	0.534	20.51	72.8	0.0101
Propyl alcohol	1.960	0.559	22.93	23.7	0.0223
Chloroform	1.992	0.566		27.13	0.0057
Benzene	2.008	0.568		28.88	0.0064
para-Xylene	2.018	0.571		28.33	0.0064
Petroleum ether	2.042	0.579			
Carbon bisulphide	2.057	0.580	25.37	31.38	0.0037
Acetone	2.112	0.590	27.0	23.50	0.0033
Ether	2.120	0.592	30.0	17.1	0.0024
Pentane	2.129	0.593	More than ether	About 15	0.0024

[1] NOTE.—The solubility of gases in these liquids increases in *general* with the per cent of compression or the compressibility, but the order is not exactly the same. The values for the density and for the pore volume listed in this table are calculated on the basis of the incorrect assumption that the liquids are incompressible. The same values would be obtained if the liquids are compressible but are not compressed.

When a tube containing the charcoal is outgassed, and mercury is admitted, this liquid fills what are technically known as the *voids* between the lumps, but we have found that it does not enter the very small *pores* in the lumps, so in this way the total volume of the lumps, and also the lump or "block" density, may be determined. For the charcoal given in table 1 this is 0.865. What is known as the apparent density of a charcoal is the mass of the charcoal contained in a 100 cu. cm. flask divided by 100, or it is the density of the carbon, pores, and voids, all taken together, and is usually about 0.5 for charcoals used by the Chemical Warfare Service. What is known usually as the "real density," though it is not the actual density, is that which is obtained by allowing water to penetrate the pores and by calculating the "real volume" of the carbon on the basis of the assumptions that the water completely fills the pores and that the water is not compressed by the attraction of the charcoal. The data of column 2 show that both of these assumptions cannot be correct. The densities of cocoanut shell charcoals as used by us when determined in water are 1.843 (table 1), 1.863, 1.835, and 1.808; that of the similar charcoal used, by Titoff,[1] 1.86; by Baerwald,[2] 1.92; and by Miss Homfray,[3] 1.66.

The compressibility data in table 1 are those of Bridgman,[4] those on surface tension were obtained in this laboratory, and the viscosity data, by different investigators. The compressibility increases in exactly the same order as either the pore volume or the density, except that the individual order for chloroform, benzene, and paraxylene is not known, though a study of Bridgman's curves of compression taken in connection with Richard's[5] data on their compressibility up to 500 atmospheres, makes the conclusion inevitable that as a class they lie between propyl alcohol and carbon bisulphide in accordance with their position in table 1. The curves plotted from Bridgman's data do not in general cross each other when all of the liquids are non-polar, while the curves for polar liquids not only cross each other, but also those for non-polar liquids.

The data indicate that the differences in the volume of liquid absorbed in 1 cu. cm. of the charcoal, listed as the pore volume in table 1, may be due either to the compressibility or the viscosity of the liquid, but on the whole they are in better agreement with the idea that it is the compressibility which is effective. The difference between the volume of ether and water absorbed is about 10%, and to produce this difference of volume by pressure exerted on the whole volume of liquid would require, according to Bridgman's data, between four and twelve thousand atmospheres. It is evident, however, that only a small fraction of the liquid is in actual contact with the charcoal. Dr. A. M. Williams[6] has calculated the volume of liquid in the primary film in the cocoanut charcoal used by Titoff as 0.30 ccm., which would be of the order of three-fifths of the pore volume. This would show that the average pressure in the primary film due to the attraction of the carbon is extremely high, and while the difference between the compression of ether and that of water does not vary in such a way as to make it possible to give an exact estimate, the pressure would probably need to be more than *twenty thousand atmospheres* higher than the normal internal pressure of the liquid. The film, according to Williams, has an average thickness of 4×10^{-8} cm., which is of the order of one or two molecules in thickness for molecules of the dimensions used in our work. The magnitude of this pressure, which is the amount that the adhesional pressure betwen the carbon and the liquid *exceeds* that in the liquid itself, is not at all surprising. The deductions from the compressibility theory indicate, if the theory is on a sound basis, that the average pressure given in our experiments is of the order of twice to three times as great as the highest pressures obtained by Bridgman.

This theory leads to low values for the density of active carbon, or a density of about 1.6. Some of the charcoals which we have studied do not absorb more ether than they do water, or this compressive effect seems wanting. These are just the charcoals which are extremely inefficient in adsorbing gases, that is, cedar wood charcoal, with a density of 1.50, and beechwood charcoal with a density of 1.65, which values are of the same order of magnitude as that calculated for cocoanut shell charcoal, or as the density in water of the rather low-grade cocoanut shell charcoal used by Miss Homfray. A charcoal made from anthracite coal alone has a density of 1.876 in water, and only 1.906 in ether, while a sample of carbonite (made from anthracite and pitch) of 19 minutes accelerated chlorpicrin test had a density of 1.934 in water and 2.075 in ether.

The data of table 1 suggest still another theory, that the differences in the volumes of the liquids absorbed are due to their viscosity, the idea being that some of the pores are so fine that the liquids which are more viscous are less able to penetrate than those which are more fluid, at least in the time allowed for penetration, from 24 to 48 hours. The first results which we obtained indicated that the quantity of liquid absorbed increases with the time, but this time effect disappeared when we adopted the extremely thorough method of outgassing previously described. We have heard that Professor Hulett of Princeton University has found such a time effect, but presumably on less highly outgassed charcoals. Mr. Monroe is now engaged in a study of this effect under the conditions used in our tests. The principal evidence against the viscosity theory to be found in the data of table 1, is that the viscosity value obtained for propyl alcohol is badly out of order, since propyl alcohol is twice as viscous as water, yet much more of the propyl alcohol penetrates the pores. This accords exactly with the compressibility theory.

Experiments on the heat of adsorption of liquids gave some interesting results. A highly outgassed cocoanut shell charcoal gave a heat of adsorption equal to 10.5 calories per gram of charcoal when immersed in water enough to completely fill its pores and voids. The same charcoal dried for ten days at 120 to 150° C., and then cooled over phosphorus pentoxide, had a smaller heat of adsorption, 7.0 calories, while it gave only 3.5 calories when it held a thin film of water equal to 0.04 gram per gram of charcoal. Thus the first tenth of the water which penetrates the pores gives rise to nearly all of the evolution of heat. Such experiments as this are now being carried out by precise methods, carefully measuring each amount of water added, in somewhat the same way as Titoff carried out his experiments on the heat of adsorption of gases, but with a number of added precautions. The results indicate that, in accord with the theory of Langmuir[7] and in agreement with the work of this laboratory on surfaces, the forces of adsorption or adhesion act only on an extremely thin layer of liquid.

The heat of adsorption of bone charcoal which had not been outgassed was found to be 18·5 calories, and in fullers' earth it was 32 calories, or almost the same values as were obtained by Gurvich[8] (18.5 and 30.2). Thus both of these solids have a very much higher heat of adsorption than even the best grades of cocoanut shell charcoal. While commonly known as the heat of adsorption, this is more precisely designated as the heat of spreading of a liquid on the surface of a solid. If $-Q$ is taken to represent the heat given off per square centimeter of surface; γ, the free surface energy; l, the latent heat of the surface; $-\Delta E$, the decrease of surface energy in spreading, and the subscript *sp* represents spreading; *s*, the solid, *a* and *i*, the interface, then

$$-Q_{sp} = -\Delta E_{sp} = \gamma_s + l_s - (\gamma_i + l_i). \qquad (1)$$

The adhesional heat and energy are defined by the equation

$$-Q_A = E_A = -\Delta E_s = \gamma_s + l_s + \gamma_l + l_l - (\gamma_i + l_i) \qquad (2)$$

or

$$-Q_{sp} = -Q_A - (\gamma_l + l_l). \qquad (3)$$

The heat of adsorption of enough gas to form a liquid in bulk, covering the solid surface at constant temperature (Q_T), is given by the equation

$$-Q_T = \gamma_s + l_s - (\gamma_i + l_i) + \lambda v \qquad (4)$$

where λ is the latent heat of vaporization of the liquid per unit volume of vapor. While we do not have the values of these quantities in the case of carbon, they have been determined in this laboratory for the interfaces between either mercury or water, and other liquids. The Pouillet effect, which is the heat given out when a liquid is adsorbed by a powder and is entirely analogous to that described here, has been attributed to the *compression* of the surface film of the liquid in contact with the powder. This compression, which was merely assumed from the standpoint of ideas on molecular attraction, is practically the same as that for which moderately direct evidence has been given in this paper. However, the above equations, which were developed by Harkins, show that the heat comes from the total energy changes in the film, which include both the solid carbon and the liquid of the interface. This heat is entirely analogous to that developed when a body falls from a height above the earth to its surface, when the mutual potential energy of the body and the earth are converted into molecular kinetic energy, or heat. In a similar way, when the surface of the liquid and of the carbon fall together, their mutual potential energy of molecular attraction, due to electromagnetic (largely electrical) forces, is converted into molecular kinetic energy, which is heat.

This theory of the heat of adsorption has been developed by Harkins, but has not been published, although the fundamental equations upon which it is based have been published in these PROCEEDINGS. The final equations, (1) and (4), as given above, were not, however, presented. The experimental work on interfaces as carried out in this laboratory indicates that the γ_s of equation (1) is very large, and that most of the heat which appears is due to the fact that the interfacial free energy carbon-liquid is very much smaller than the free surface energy of the solid carbon. Equation (3) shows that the heat of adsorption of the liquid is equal to the energy of adhesion between the liquid and the carbon, minus the total surface energy of the liquid. In the case of water this total surface energy is moderately large, so the heat of spreading of the liquid is considerably less than the adhesional energy.

If further data agree with those already found in indicating that the differences in the volumes of various liquids absorbed by activated cocoanut shell and other highly activated charcoals are due to a compression by the forces of adhesion of the order of twenty thousand atmospheres as the average pressure in a film of 4×10^{-8} cm. in thickness, which would indicate a much higher pressure for the first layer of atoms, then the results presented in this paper will be the first direct evidence of the existence of such high forces between solids and liquids, or in liquids. Thus, while the theories of Laplace and van der Waals indicate an internal pressure of 10,000 atmospheres in water, and usually from 200 to 4,000 atmospheres in an organic liquid, it has not been found possible to demonstrate experimentally forces greater than 34 atmospheres with water at 24.4° C., 39.5 atmospheres for ethyl alcohol at 22.5, and 72 atmospheres for ether at 17.7°.[9]

The area of the surface in 1 gram of Miss Homfray's charcoal was 1.31×10^6 sq. cm. per gram of charcoal, and in Titoff's charcoal it was 8×10^6, according to estimates made by Williams.[2] Lamb, Wilson, and Chaney[10] have estimated the surface in a gas-mask charcoal as 10^7 sq. cm. per gram, or almost the same as in Titoff's charcoal. In this laboratory Mr. Amando Clemente has been working on the adsorption of vapors at low pressures, and his work seems to indicate an area very well in agreement with that of Lamb, Wilson, and Chaney, but experiments are still incomplete so the final value is uncertain. His work indicates that the adsorptive capacity of charcoal at low pressures is greatly increased by repeated outgassing or by treating with the vapors or gases to be adsorbed several times between outgassings. Harned has noticed that in the latter way the speed of adsorption is increased, but the effect noticed here is on the equilibrium value. One gram of 750 minute charcoal, according to the ordinary chlorpicrin test, was outgassed, treated with benzene vapor, and then outgassed a second time. The volume of the whole apparatus was 2,200 cu. cm., and was filled at 24° with benzene vapor at a pressure of 1.5 mm. On opening this volume to the *one gram of charcoal*, the pressure fell in 12 hours to 0.00002 mm., or to less than *one seventy thousandth of the initial pressure*. The long time necessary was due to the fact that the equilibrium between the outside and inside of the lumps of charcoal is adjusted very slowly, since the diffusion is very slow. In a similar manner a pressure of 5.13 mm. was reduced to 0.00004 mm. In these experiments the charcoal was also at 24° C., so they illustrate the extremely great effect of the charcoal surface in increasing the life of the molecule[12] on the surface above what it would be on the surface of the liquid. In the second of these two experiments about one-tenth of the surface of the carbon was covered by molecules of benzene, provided the above estimates of the area of the surface are correct. On account of the great differences in the curvature of the surfaces of the pores, the different molecules are held by widely varying forces.

The apparatus first used in this work was similar to that described by Lemon,[11] but it was soon found that more rapid work could be done in an apparatus in which there are no stopcocks. Our work is the first which relates to the volume of different liquids taken up by charcoal. In addition to papers already cited, reference should be made to a paper by Chaney[12] on the activation of carbon, to papers by Langmuir[12] and by Harkins and his co-workers[13] on the theory of surfaces and adsorption, and to papers on adhesion by Harkins.[14] Thanks are due to Dr. Chaney for the preparation for special charcoals, and to the Gibbs Fund of the National Academy of Sciences for a grant which enabled us to purchase the vacuum apparatus.

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[12] Chaney, *Trans. Amer. Electrochem. Soc.*, 1919 (Advance sheets).

[13] Harkins, Brown, and Davies, *J. Amer. Chem. Soc.*, **39**, 1917 (354); Harkins, Davies, and Clark, *Ibid.*, (541); Harkins and King, *Ibid.*, **41**, 1919 (970); these PROCEEDINGS, **5**, 1919 (152).

[14] Harkins, these PROCEEDINGS, **5**, 1919 (December).

CENTRAL AMERICAN CALENDARS AND THE GREGORIAN DAY

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Communicated by H. F. Osborn, December 23, 1919

The day-for-day correlation of the Mayan, Aztecan, and Gregorian calendars depends upon mathematical relations and logical assumptions.

First, as regards the archaic Mayan system of counting time, seen in inscriptions on monuments, the following assumptions are regarded as established:

a. That the permutation cycle of 260 days made by combining 20 days and 13 numbers in sequence was continuously applied to days from a beginning day.

b. That the notational system by which the index number of a day was written down was also continuously applied and that the designation of a day in the permutation and in the notation was constant and determinable.

c. That the *tun* (360 days) and *katun* (7200 days) were not abstract time periods but instead were order values in the notational system and necessarily involved a context of the long count of days of the Mayas just as '06 or '19 necessarily involves a context in the long count of years that distinguishes European chronology.

d. That the Mayan *haab* was a vague year of 365 days divided into 18 long months of 20 days each and one short month of 5 days and that while the true length of the tropical year was known with tolerable accuracy to the Mayas no correction was directly applied to the *haab* count for fear of disorganizing the day count which was the common measure of all astronomical and mathematical cycles.

As regards the records and time counts used by the Mayas of northern Yucatan at the time of the Spanish conquest it is regarded as proven:

a. That an organic contact with the archaic long count was maintained through the orderly succession of *tuns* and *katuns* designated by the name of the day on which they were completed.

b. That a true contact and correlation of the U Kahlay Katunob cycle of the historical summaries in the Books of Chilan Balam and the archaic long count was affected when the initial series date at Chichen Itza was placed in the earliest occupation of this site in the historical summary. According to this correlation Katun 8 Ahau at the beginning of the historical summary coincides with the round number 9–0–0–0–0, 8 Ahau 13 Ceh, in the long count and the particular Katun 13 Ahau, completed early in the Spanish conquest of Yucatan, coincides with 12–9–0–0–0, 13 Ahau 8 Kankin.

c. That the much-discussed shift in the positions that the days could occupy in the months of the *haab* (whether real or apparent) had no effect on the continuity of day names determined by the permutation cycle and the ancient notation and that 7 Ix 1 Pop of the late Mayan statements must be regarded as equivalent to 7 Ix 2 Pop of the early Mayan statements.

d. That the "year bearer" method of designating the *haab* in northern Yucatan, while frankly at variance with the classical philosophy of counting only completed time units, nevertheless was organically tied into the ancient day count and that a year bearer 7 Ix designated a *haab* beginning with a day 7 Ix 2 Pop in the long count.

e. That the end of Katun 13 Ahau fell during the Spanish conquest of Yucatan in a year 7 Ix according to the following mathematical demonstration:

12–8–14–4–14 7 Ix 2 Pop
13– 6
12–9– 0–0 0 13 Ahau 8 Kankin

f. That the year 7 Ix corresponded to the latter part of the European year 1535 and the earlier part of 1536 on reasonable evidence fixing the correlation of several year bearers in the year-bearer cycle (52 × 365 days).

The ground having been laid in this fashion for a correlation of days the complete Mayan year given by Landa is brought into relationship with the long count as follows:

a. This year was one marked by 12 Kan as year bearer and, therefore, corresponded in the current year-bearer cycle to 1553–'54.

b. This initial day of the Landa year, namely, the day 12 Kan 1 Pop in the late Mayan nomenclature or 12 Kan 2 Pop in the ancient nomenclature, equaled 12–9–17–9–4, 12 Kan 2 Pop in the long count and it likewise equaled July 16, 1553, in the Julian Calendar and July 26, 1553, in the Gregorian calendar.

Having made this fundamental correlation it is evident that account must always be taken of the fact that the Mayan *haab* was a 365-day year while the Julian year had an average value of 365.25 days. Thus, if the first day of the year 12 Kan was July 26 in the year 1553, the first day of the year 6 Kan must have fallen on August 3 in 1521.

Among the Aztecs there was no long count of days put down in a fixed notation or at least no evidence of any such count has come down to us. There was, however, a permutation cycle of 260 days based on 20 names, that show etymological correspondence to the 20-day names of the Mayas, and 13 numbers. The historical records were mostly recorded in a year-bearer system. Structurally the Mexican year began with the month Toxcatl because this was the month that had as its initial day the particular day giving its name to the year.

The day-for-day correlation of the Aztecan calendar and the Gregorian calendar is based on the recorded position of a few days in both counts and on statements of the beginning days of the Aztecan months. These statements have constantly to be corrected as of a definite year owing to the retrogression of the 365-day Aztecan year in relation to the 365.25 day Julian year.

In the correlation as finally effected, the Aztecan year 3 Calli began ostensibly on a day 1 Calli, the first of the month Atlcaualco, but with the significant day 3 Calli as 1 Toxcatl. The day 1 Calli 1 Atlcaualco corresponded to February 12, 1521, Julian calendar, or February 22, Gregorian calendar.

A highly important fact develops when the Mayan and Aztecan calendars are brought together after each has been correlated to the Gregorian day. This fact is the practical identity of the two time counts which has never before been assumed or demonstrated. It should be sufficient to remove any doubts engendered by unavoidable adjudication of evidence where authorities are in conflict.

The Aztecan year bearers, Calli, Tochtli, Acatl, and Tecpatl are equivalent to the Mayan days Akbal, Lamat, Ben, and Eznab, that in the archaic Mayan calendar could occupy the position 1 Pop. But the Yucatan year bearers Kan, Muluc, Ix, and Cauac come one day later than these days. Therefore, the Mayan year bearers, Kan, Muluc, Ix or Cauac, should have numbers one in excess of the equivalent Aztecan year bearers. But it is clear that the Mayan year 6 Kan does not equal an Aztecan year 5 Calli, instead it equals a year 3 Calli. This discrepancy is not fundamental since the two years do not begin at equivalent positions in the tropical year. This initial day 6 Kan 1 Pop of the Mayan year 6 Kan fell on August 3, 1521, and in the Aztecan year 3 Calli found its practical equivalent in the first day of the month Tlaxochimaco that began with 5 Calli on August 1. More exactly 6 Kan equals 6 Cuetzpalin, the second day of the Aztecan month Tlaxochimaco, corresponding to August 2. We find, therefore, that the permutation cycles of the two calendars come within a day of exactly coinciding. The mathematical possibilities involved in this essential agreement would seem to assure the truthfulness of the Mayan correlation.

It is expected that this new possibility of turning Mayan and Mexican dates into the Gregorian calendar will open a new field for investigation.

Central American records on monuments and in codices have long been supposed to contain astronomical statements of eclipses, etc. Furthermore it may be remarked that by this demonstration American history is made more exact than the history of Egypt, Greece, or Rome. For instance, the dedicatory date on Stela 9 at Copan corresponds to March 31, 304 A.D., in the backward projection of our present Gregorian calendar. The earlier artistic classification of the present writer, and the general *katun* correlation of Morley, were correct within a very few years.

The data in full will appear in the *Anthropological Papers of the American Museum of Natural History*.

THE TORSIONAL MAGNETIC ENERGY ABSORPTION OF AN IRON CONDUCTOR[1]

By Carl Barus

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Communicated January 1, 1920

1. *Apparatus.*—The relations of torsion and magnetization have been studied by Wiedemann, Auerbach and many others since, chiefly in longitudinal fields. The torsion effect produced by a circular field is very small and difficult to ascertain. I, therefore, thought it of interest to make some measurements of this kind, using the displacement interferometer and achromatic fringes. The results were very definite and would easily have admitted of higher precision. The apparatus is shown in figure 1, where *AB* is a thin, low carbon steel tube, effectively 55 cm. long, having an average diameter of 0.875 cm., and walls 0.076 cm. thick.

The tube is firmly clutched below by a clamp, but free above. It carries the mirror *mm'* which is a strip of thin plate glass, silvered and slightly adjustable about a vertical and horizontal axis. The ends receive the component rays of the interferometer, so that any slight rotation of *mm'* about the vertical axis is at once registered by the displacement of fringes. Finally a strong electric current may be passed through the length of the tube, entering at *A* and leaving by the mercury cup *C*. The current must be reversible at pleasure

2. *Observations.*—The fringes are displaced (i.e., the tube receives magnetic set) immediately after closing the circuit. Closing it any number of times thereafter is ineffective to the fraction of a fringe. There is practically no temporary effect. On reversing the current, the fringes are markedly displaced in the opposite direction, again to hold the new position, however often the current is made and broken thereafter.

To obtain a temporary effect I surrounded the *AB* with a massive iron tube, about 6 inches long and 2 inches in diameter, clamped at the top *B* (bell-like) but otherwise free from it. Even now, with currents up to

20 amperes, I observed no temporary effect in excess of the quiver of the fringes.

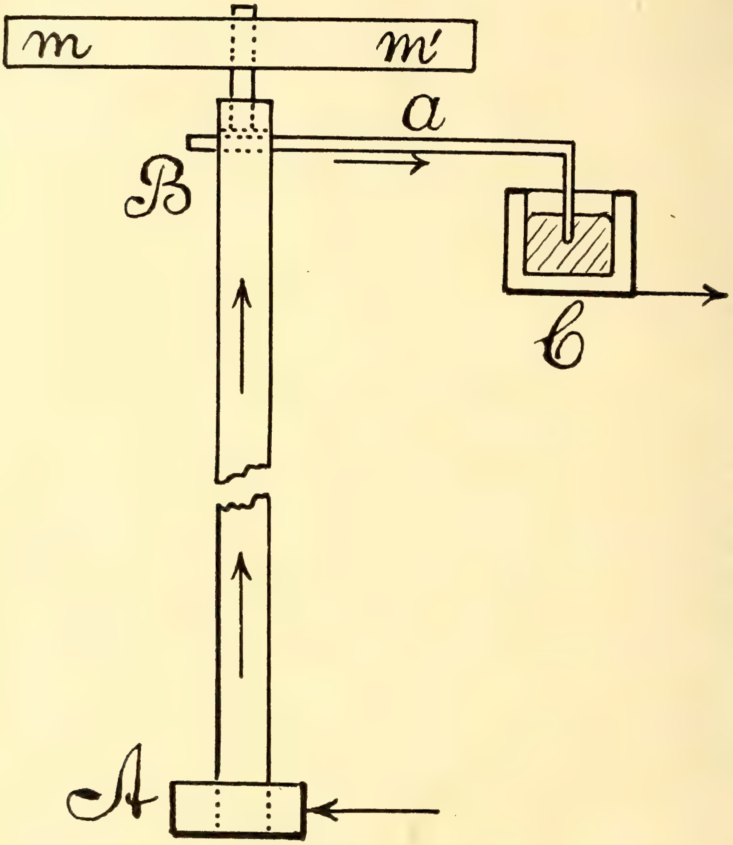

FIG. 1

The following data were obtained for the fringe displacement ΔN, showing the permanent effect of reversal of current:

Current,	3 Am.	$\Delta N =$	?
	10 Am.		0.00031 cm.
	20 Am.		0.00065 cm.

Below 3 amperes I was unable to observe a displacement. For larger currents the twist increases proportionally to the current passing the tube. It is probable that here, as in similar cases in magnetization, fields below a certain small value are ineffective, as though there were static friction.

A further observation is to be made: If a certain magnetic set is produced by a stronger current (say, 20 amperes), then a weaker current (say, 10 amperes) is unable to modify it, provided both currents are in the same direction. If this weaker current is reversed, the change of twist corresponding to the current will appear. The absolute value of the set thus depends upon the past history of the iron, however varied this may have been, with the understanding that the set produced by the maximum current in either direction is characteristic of the strength of that current. Differential values, in other words, remain the same.

3. *Data.*—Finally the numerical equivalents of the observations made may be stated. The elastic torsional coefficient for a tube of the dimensions given may be computed with the usual equation, using the differential form $dT/dr = 2\pi n r^3 \theta / l$ (mean radius r and length l) and taking the rigidity as $n = 8.2 \times 10^{11}$. If T is the torque corresponding to the twist of θ radians, the relation was found to be

$$T = 10^9 \times 4.7\theta.$$

On the interferometer, if the breadth of ray parallelogram is $b = 10$ cm. and $\Delta\theta$ is the rotation of the mirror mm' around a vertical axis, corresponding to the displacement of mirror ΔN (the mirror being at $i = 45°$ to the rays), the relation will be, since $\Delta\theta = \Delta N \cos i/b$,

$$\Delta\theta = 0.071\,\Delta N.$$

Hence, if we assume that the resistance to magnetic set is the same as the elastic resistance for the same twist $\Delta\theta$, the above values of ΔN will correspond to the following data:

Current	$\Delta N \times 10^5$	$T \times 10^{-5}$	E	$E/vol.$
10 Am.	31	1.03	1.13	.10
20 Am.	65	2.16	4.97	.43

The energy, E, potentialized by the magnetic set is computed as $T\Delta\theta/2$. From the volume of iron in the tube, 11.5 cm^3., the data of the last column follow.

My conception of this phenomenon is that of two concentric circular fields in opposite directions, one within the other on the outside of the tube, introducing a rather intense vortex sheet in the thin walls of the tube, in which the circular fields terminate.

4. *Longitudinal Field.*—The longitudinal strain produced by a longitudinal field is well known. The question may be asked whether in this case, in a free iron bar, there is any corresponding torsion. This was easily answered by slipping a helix over the steel tube. Feeding it with I amperes, the micrometer displacement ΔN was successively

$I =$	1.5	4.5	9.4	1.5	Am.
$10^5 \times \Delta N =$	15	10	10	5	Cm.

This means, no doubt, that the residual torsion left by the last experiments is being eliminated.

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[1] Advance note from a report to the Carnegie Institution of Washington, D. C.

THE ADJUSTMENT TO THE BAROMETER OF THE HEMATO-RESPIRATORY FUNCTIONS IN MAN

By Yandell Henderson

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Communicated by L. B. Mendel. Read before the Academy, November 10, 1919

It is well known that persons living at considerable elevations—such as Colorado, the cities in the Andes, et cetera—exhibit distinct functional adjustments. The amount of hemoglobin per unit volume of blood is increased, and a larger volume of air is breathed per unit mass of oxygen absorbed and CO_2 eliminated.

These are functional adjustments and not merely immediate compensations; for they are developed slowly through days or weeks, and the functions return to sea level values only very gradually. Thus, to illustrate by a single simple test, a man on Pike's Peak, who is acclimated to the altitude, can hold his breath for a much shorter time than when living at sea level. But on descending from the Peak, and during the first few hours thereafter, he can hold it scarcely longer than at the summit.

There is clear evidence that this condition and the increase of breathing are due to a decreased amount of alkali, chiefly $NaHCO_3$, in the blood.

By the now well-known equation of L. J. Henderson,

$$\frac{H\,CO_3}{NaHCO_3} \times K = C_H,$$

it is evident that, if respiration is to maintain the same C_H in the blood at an altitude that it does at sea level, the amount of CO_2 in solution in the blood must be reduced in proportion to the alkali. This is accomplished by an inversely proportional alteration, that is, an increase, of breathing.

It is generally supposed that under oxygen deficiency acids are produced in the tissues and retained in the blood, and that the alkali is thus neutralized and then eliminated through the urine.

Experiments on dogs by Dr. H. W. Haggard and myself, however, show that the processes involved are in many respects exactly the opposite of this supposition. Our experiments demonstrate that, before any considerable amount of alkali is lost, an abnormally large amount of CO_2 is eliminated by the excessive breathing induced by a lowered oxygen pressure in the air breathed. Then alkali passes out of the blood to compensate this alkalosis.

Thus the process is exactly the opposite of that occurring in other experiments which we have performed, in which acid was administered intravenously, and in which, therefore, the alkali was lowered first and the CO_2 in solution was lowered only secondarily.[1] Under low oxygen the lowering of the dissolved $CO_2(H_2CO_3)$ is primary, and that of the alkali secondary.

From these facts and from closely similar observations on aviators[2] during the war, it appears highly probable that in normal persons the blood alkali is controlled by the dissolved CO_2: more or less alkali being called into use in the blood to satisfy the equation above quoted, and to keep the C_H of the blood constant. The amount of dissolved CO_2 in the blood is controlled by the pulmonary ventilation; and fundamentally the ventilation is adjusted to the oxygen partial pressure of the air at the altitude at which the person lives.

Thus, as data from the report of the Pike's Peak expedition[3] tend to confirm, the partial pressure of oxygen in the lungs, the alveolar CO_2, the $CO_2(H_2CO_3)$ dissolved in the blood, and the amount of the blood alkali (the so-called "alkaline reserve"), each multiplied by a constant of its own, tend to vary in direct proportion to the mean barometer, minus the water vapor tension of the pulmonary air (about 45 mm.) at all altitudes.

[1] Haggard, H. W., and Henderson, Yandell, *J. Biol. Chem.*, **39**, No. 1, August, 1919 (163–261).

[2] Henderson, Yandell, *Science, New York*, N. S., **49**, No. 1271, May 9, 1919 (431–441).

[3] Douglas, Haldane, Henderson, and Schneider, *London Phil. Trans. Roy. Soc.*, B. **203**, 1913 (310).

A STUDY OF ABSORPTION SPECTRA WITH THE ELECTRIC FURNACE

BY ARTHUR S. KING

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Communicated by G. E. Hale, December 9, 1919

The tube resistance furnace has found a place as a useful light-source in spectroscopic work, but thus far it has been used mainly for the production of emission spectra. The long column of vapor, whose temperature can be controlled, offers interesting possibilities, however, in the field of absorption phenomena. In the experiments to be described, the continuous background was supplied by a plug of graphite placed in the center of the furnace tube, the heated portion of which was 20 cm. long and 12.5 mm. internal diameter. A close approach to black-body conditions is thus obtained, and the emission of the plugged tube is stronger than that of the metallic vapor filling the tube. An absorption spectrum results, which, by simply removing the plug, may be compared with the emission spectrum given by the vapor at the same temperature.

It is known from observations of reversed emission lines that in a given range of spectrum, some lines are more subject to reversal than others. The present experiments have shown this to result from a close connection between the tendency of a line to reverse and the two factors which enter into the classification of lines in furnace spectra, viz., the temperature at which a line is first radiated and its response to increase of temperature.

The phenomena may be briefly described by saying that when a temperature is reached sufficiently high to produce an absorption spectrum, the lines appearing are those of low-temperature class. As the temperature is raised, lines of successively higher classes appear. The lines of low-temperature class thus show the largest degree of absorbing power. At a given temperature, the absorption spectrum shows fewer lines than are given by the emission spectrum at the same temperature. The absent lines are of the higher temperature classes, which the vapor is able to emit, but which have relatively low absorbing power. For the iron and titanium spectra, the absorption spectrum is very similar to that given in emission at a temperature about 400° lower. As lines which are strong in the furnace and faint in the arc are usually lines of large absorptive power, the difference between the arc and the dark-line furnace spectrum is very striking.

The dependence of absorptive power on temperature class was well brought out by placing the graphite plug beyond the center of the tube away from the spectrograph, so that some of the metallic vapor was hotter than the plug. It was then possible to obtain emission and absorption lines at the same time. The high-temperature lines appeared in emission, while the plug was still hot enough to cause those of low-temperature class to show as absorption lines.

Lines which are being emitted by a vapor may fail to appear in absorption owing either to a low absorptive power and consequent quenching by the continuous spectrum, or to a balancing of emission and absorption which may occur when the continuous background is reduced in temperature so that its emission is of about the same strength as that from the metallic vapor. This latter condition was produced in the furnace by withdrawing the plug nearly to the end of the tube. Lines produced by the vapor in the central portion were then frequently neutralized by the balanced emission and absorption and were not to be seen. In this way a line may seem to be totally absent when in reality it is being strongly radiated by the vapor.

A method of producing absorption spectra without the use of a plug may be mentioned in this connection. When the furnace is operated in the usual way, with a metallic powder or salt in the open tube, the emission spectrum increases in intensity and number of lines as the temperature is raised, until it compares in richness with that of the arc. A still higher temperature causes a rapid increase in a continuous ground, which is probably due in part to the long column of dense metallic and carbon vapor and partly to light from the walls of the tube reflected by the vapor particles. This obliterates the emission lines and we have an absorption spectrum consisting of the lines which in the high-temperature emission spectrum were self-reversed. In general these are low-temperature lines, although they result from a high-temperature condition. This phenomenon may occur with the furnace in vacuo, so that the continuous spectrum cannot be regarded as resulting from a high-pressure condition. The possibility of producing an absorption spectrum accompanied by the suppression of many lines which are being emitted strongly at this temperature must be considered when dealing with an extended mass of vapor.

A STUDY OF THE EFFECT OF A MAGNETIC FIELD ON ELECTRIC FURNACE SPECTRA

BY ARTHUR S. KING

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Communicated by G. E. Hale, December 9, 1919

Investigations on the splitting of spectrum lines by the magnetic field have employed, except in rare instances, the electric spark as the source of light. It has proved impracticable to maintain an arc between the poles of a magnet, though an apparatus giving a succession of flashes has been employed. The flame, because of its weak luminosity, has but limited usefulness. The electric furnace, if its size does not prevent its use in the magnetic field, will evidently do work not at present taken care of. While the furnace does not give the enhanced lines peculiar to the spark, the other lines shown by the arc and spark occur for the most part in the furnace spectrum. In addition, the furnace is found to show a large number of lines which are produced only with great difficulty by either the arc or the spark, and for which we have as yet no data as to their magnetic separation. A further advantage over the spark for the lines common to both is the extreme sharpness of furnace lines when the apparatus is enclosed in a vacuum chamber, a feature which should add materially to the definition of the Zeeman components.

Pending the construction of a more powerful apparatus, a simple tube furnace was arranged for use in a magnetic field. A graphite tube, 10 cm. long, was placed axially between the poles of a Weiss electro-magnet. The jacket enclosing the tube and the contact blocks at the ends were water-cooled. A field of 6500 gausses separated the n components of most lines sufficiently to permit measurements of fair accuracy and to show the characteristic features of the source. The spectra of iron and vanadium received the chief attention, and measurements were made for a considerable number of lines having well-defined components. A comparison of lines common to furnace and spark showed no difference either in the number and arrangement of components, or in their separation in the two sources, the very different excitation in the two cases appearing to have no effect on the magnetic characteristics. The furnace and spark can thus be used to supplement each other in studying the magnetic behavior of all classes of lines. As was expected, separations were obtained for many characteristic furnace lines which the spark emits very faintly. An outstanding feature of these lines for the iron spectrum is a prevailing large separation and simple triplet structure. Lines of this class are often of special interest in the study of sun-spot spectra.

The furnace offers unique facilities for the production of the inverse Zeeman effect when a plug is placed in the tube. The absorption spectra resulting from this arrangement were discussed in the preceding communication. An extended study was made of the magnetic components given in absorption by this method. No difference was observed as to character or magnitude of separation as compared with the effects for the emission spectrum. A means of direct comparison with the magnetic effects for absorption lines in the solar spectrum is thus afforded, which may be expected to be very useful in tests as to polarization and other features at various angles to the lines of force.

SELECTIVE FERTILIZATION IN POLLEN MIXTURES

By D. F. Jones

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Communicated by T. B. Osborne, Read before the Academy, November 10, 1919

The fact of self-sterility or self-impotency is well established in many species of plants and in at least one family of animals. It is also believed that even in the normal condition of self-fertility the germ cells from different, unrelated individuals in some cases are more efficient in accomplishing fertilization than the germ cells from the same or similar individuals. Statements to this effect are made in text books on biology.

A simple method of accurately testing this assumption is available in the use of pollen mixtures which carry such inherited characters that the different kinds of seeds resulting from the pollinations can be distinguished. By taking pollen from two distinct types of maize, designated A and B, in approximately equal quantities, thoroughly mixing, and applying the mixture to the plants which furnish the pollen, it is possible to obtain and to separate the two kinds of seeds on each plant. On the A plants the seeds are $A \times A$ self-fertilized and $A \times B$ cross-fertilized. Similarly, on the B plants the seeds resulting from the mixture are $B \times A$ cross-fertilized, and $B \times B$ self-fertilized. The numbers of individuals in each class form a proportion such that the $A \times A$ seeds are to the $A \times B$ seeds (produced on A plants) as the $B \times A$ seeds are to the $B \times B$ seeds (produced on B plants). The end terms of this proportion represent self-fertilized, while the middle terms represent cross-fertilized seeds. If fertilization takes place at random the numbers form a perfect proportion, irrespective of the relative amounts of functional pollen present in the mixture and independent of the total number of seeds obtained from either the *A* or the *B* plants. If there is a deviation from a perfect proportion, this will be either in favor of cross-fertilization or self-fertilization.

Results of Mixed Pollinations Giving the Total Number of Seeds, and the Numbers in Each Class Forming Proportions, the Deviations of Which Expressed as Per cent, from the Closest True Proportions, Are Largely in Favor of the Plant's Own Kind of Pollen. *P* Is the Probability that the Deviations Are Due to the Differences of Random Sampling

POLLEN MIXTURE NUMBER	TOTAL NUMBER OF SEEDS	NUMBER OF SEEDS IN EACH CLASS				DEVIATION FROM TRUE PROPORTION, PER CENT	X^2	*P*
		A × *A*	*A* × *B*	*B* × *A*	*B* × *B*			
1	3430	1738	46	1602	44	+0.045	0.027	0.994
2	5636	2133	145	3080	278	+0.955	7.572	0.063
3	1362	229	14	770	349	+12.715	146.196	0.000
4	3344	710	126	1856	652	+5.465	61.203	0.000
5	1956	589	6	1290	71	+2.105	28.718	0.000
6	424	40	71	187	126	—11.850	23.858	0.000
7	3459	23	89	1507	1840	—12.245	235.357	0.000
8	7783	2185	956	2619	2023	+6.570	144.108	0.000
9	8729	2550	1288	2922	1969	+3.350	42.061	0.000
10	5408	1084	1154	997	2173	+8.495	162.687	0.000
11	3314	448	264	1505	1097	+2.540	8.937	0.030
12	3561	1724	719	749	369	+1.790	5.313	0.053
13	736	185	391	95	65	—13.625	55.051	0.000
14	792	424	150	156	62	+1.155	0.533	0.894
15	3168	2609	47	14	498	+47.750	2889.561	0.000
16	2224	723	8	74	1419	+46.975	1965.981	0.000
17	1410	1303	3	4	100	+47.960	1298.993	0.000
18	1599	4	21	1	1573	+7.970	137.532	0.000
19	2606	528	392	343	1343	+18.525	376.394	0.000
20	2753	897	77	1174	605	+13.050	283.002	0.000
	63694							

One great advantage of using endosperm characters in this way is that the several kinds of seed develop under nearly as favorable environmental conditions as it is possible to obtain, so that selective elimination of zygotes, which may favor one class more than another, is reduced to a minimum. In those plants in which the cross-fertilized and self-fertilized progeny cannot be distinguished until they are grown to maturity there is considerable likelihood that unequal germination and differential viability will tend to favor cross-fertilization. Animals are subject to the same objection as material for investigating this problem.

The method of mixed pollination in reciprocal applications is the simplest means of accurately testing the efficiency of two kinds of pollen in competition with each other. It is impossible to know definitely the relative numbers of the two kinds of fertilizing elements present in a mixture. Even if it were possible to obtain equivalent amounts of pollen or seminal fluids there would be no way of knowing the ratio of the two kinds of effective germ cells, since in many species such cells rapidly lose their viability.

In many experiments performed with maize, pollen was collected from a number of plants of two distinct but uniform types of plants. Approximately equal quantities of each kind of pollen were taken, thoroughly mixed by shaking in a bag, and applied to a number of plants of each of the two kinds supplying the pollen. The aim was to have from 1000 to 2000 seeds in each of the two parts of the proportion. Twenty such mixed pollinations were made, using many different types of maize, and altogether there resulted 63,694 seeds. These have been classified, counted, and a sample of each lot grown to test the accuracy of separation, and the proportions have been calculated as percentages. See table 1. Seventeen of the twenty show a deviation in favor of the plant's own kind of pollen, while only three show the reverse effect. Of the 17 experiments which indicate a prepotency of pollen on the stigmas of the plants by which it was produced, 2 are not significant when compared with the deviations expected in sampling. In the remaining 15 the deviations are so large that there can be no question that in maize there is a pronounced preference of the plant for its own kind of pollen, even though the foreign pollen is perfectly capable of accomplishing fertilization when not in competition.

Similar experiments have been carried out with another plant, the tomato. Two pollen mixtures were made, utilizing characters by which the seedlings could be separated. As with maize, the results give a deviation favoring the familiar pollen. However, the number of plants obtained was not large and classification was not so sure as in the experiments with maize.

Taking the results from maize alone, magnifying the experimental error to its fullest extent, giving due allowance to differential viability, and taking into consideration the differences arising from random sampling, the conclusion is inevasible that in this species at least there is a definite receptiveness of the plant to its own kind of pollen. This is notable in view of the great advantages which hybrid vigor gives immediately to the cross-fertilized seeds and the plants grown from them. The weight of cross-pollinated seed is increased as much as 50% in some cases. Both the embryo and endosperm are larger, the seeds have a higher specific gravity, and they mature faster, as shown by their lower water content at the end of the growing period. The increase in weight, expressed as per cent, permits a comparative estimation of the amount of heterosis shown by the various combinations involving different materials. There is a significant correlation between the amount of heterosis and the preference shown by the plant for its own pollen. Since heterosis is roughly proportional to germinal diversity, the greater the genetic differences there are the greater is the handicap placed upon the pollen when competing with pollen from the plants on which it is to act. This is a remarkable result, because *in proportion as the cross-fertilization benefits the progeny the less effective are the germ cells in accomplishing fertilization.*

It should be clearly kept in mind that the selective action shown in pollen mixtures is not the same as a differential potency of unlike gametes produced by one individual. In the latter case there is no positive proof that such an effect is ever obtained, where the different gametes are all viable, and in most cases there certainly is no such action.

It has been found that the same selective action is shown when first generation hybrids are paired as when strains which have been long inbred are used. In other words, the effect is apparent, whether the plants have a long line of similar ancestors back of them or whether their immediate parents are diverse. It is also shown both in plants of weak growth or of full vigor, and whether the gametes of either type or both are alike among themselves or are exceedingly diverse in the hereditary factors which they carry. The only feature in common is that the cytoplasm is alike for all the germ cells of one plant, and this cytoplasm, in self-fertilization, is the same in which the pollen fulfils its function. This indicates that the unequal fertilizing ability is governed by the rate of pollen tube growth, although it may be determined after the male gametes are brought to the egg.

There is current in biological literature the assumption that heterogeneity in protoplasmic structure is favorable to developmental efficiency. This idea has been proposed again and again and applied in many different ways. Stated in general terms, it implies that the union of diverse elements and the resulting lack of balance stimulates growth. This is a heritage from Darwinism, and the writer believes that it is founded upon fallacious reasoning and is not supported by the facts.

The hypothesis has been used in theories of rejuvenation, explanations of hybrid vigor, and speculations concerning selective fertilization. The necessity for sexual reproduction at some time to maintain organisms reproducing asexually is no longer admitted. That the process of forming gametes which reunite to make a new individual may bring about a reorganization of the protoplasm with elimination of waste products resulting in increased growth is easily conceivable, but the significance of such a procedure is not necessarily to be found in the bringing together of unlike elements. The vigor of hybridization has now been put on a basis of pure inheritance, and the physiological stimulation hypothesis is no longer needed. Homozygous factor combinations, according to present theory, are more efficient than heterozygous combinations of the same factors. It now seems that self-prepotency, except in those cases where a definite process has been developed to prevent self-fertilization, indicates that unlikeness instead of favoring fertilization is a hindrance.

So valuable have been the evolutionary advantages of sexual reproduction in increasing variability that many contrivances have been perfected to insure the fulfilment of the function responsible for its creation. Self-sterility or self-impotency is one of the many special adaptations which serve this purpose. The evidence for selective fertilization favoring organisms of the same type, or self-prepotency, is limited just now to one or possibly two species. Will it not be strange to find it so restricted? Is it not more likely to be a general phenomenon manifested in some degree by many organisms? Even in those cases where cross-fertilization is made imperative by a physiological impediment to self-fertilization the same tendency may operate although overwhelmed by the special adaptation. One cannot insist that such is the case, with the evidence isolated as it is at present. Neither is it maintained that the reaction of the cytoplasm of the pollen tubes with the tissues of the host, preceding fertilization, has any relation with the processes which go on within the cells after fertilization. But the prepotency of germ cells acting upon the same or similar individuals which produced them is another indication that homogeneity, likeness, similarity, familiarity, or however it may be described, in protoplasmic structure is consistent with and favorable to the highest developmental efficiency.

GROUPS GENERATED BY TWO OPERATORS, s_1, s_2, WHICH SATISFY THE CONDITIONS $s_1^m = s_2^n$, $(s_1s_2)^k = 1$, $s_1s_2 = s_2s_1$

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W. R. Hamilton observed, in 1856, that the groups of movements of the five Platonic solids may be defined by means of equations of the form

$$s_1^m = s_2^n = (s_1s_2)^k = 1.$$

Various generalizations of these groups were obtained during recent years by means of equations of the form

$$s_1^m = s_2^n,\ (s_1s_2)^k = 1.$$

In both of these cases only a few special values of m, n, k were considered. On the contrary, general values of m, n, k are considered in the present note, but an additional condition is imposed on s_1, s_2; viz., the condition that they shall be commutative operators. Hence all the groups generated by these two operators are abelian.

The main result obtained in this note may be stated as follows: *If two commutative operators, s_1 and s_2, satisfy the conditions $s_1^m = s_2^n$, $(s_1s_2)^k = 1$*

but are not restricted otherwise they generate the direct product of two cyclic groups of orders $(m + n)k/d$ *and* d*, respectively,* d *being the highest common factor of* m, n, k. In particular, a necessary and sufficient condition that the group G generated by s_1 and s_2 is cyclic is that m, n, k have no common factor greater than unity. For instance, the cyclic group of order 75 is determined by two commutative operators which satisfy the equations

$$s_1^6 = s_2^9, \ (s_1s_2)^5 = 1.$$

This group is also determined by $s_1^{10} = s_2^{15}$, $(s_1s_2)^3 = 1$, etc.

It may first be noted that G is always cyclic when m and n are relatively prime. In fact, when this condition is satisfied and p is any prime number which divides m then the Sylow subgroup of order p^α contained in the group generated by s_1 includes the Sylow subgroup of order p^β contained in the group generated by s_2, and $\alpha > \beta$ when k is divisible by p. When k is not divisible by p then $\alpha = \beta = 0$. Hence it results that the Sylow subgroups of G whose orders are not prime to both of the numbers m and n are cyclic whenever m and n are relatively prime. The other Sylow subgroups of G must also be cyclic since they are generated by s_1^m. This proves that G is cyclic whenever m and n are relatively prime.

When m and n are not relatively prime let d_1 represent their highest common factor. Just as before it follows that the Sylow subgroups of G whose orders are prime to d_1 are cyclic, and those whose orders are not prime to d_1 must also be cyclic when k is prime to d_1 since the generator of such a Sylow subgroup in the group generated by s_1 must be the inverse of such a subgroup in the group generated by s_2. It has, therefore, been proved that a sufficient condition that G be cyclic is that the three numbers m, n, k have no common factor greater than unity.

To prove that the order of G is $(m + n)k$ when this condition is satisfied it is only necessary to prove that the orders of s_1 and s_2 divide this number and that G contains at least one operator whose order is equal to this number. The former of these facts results directly from the following equations:

$$s_1^k = s_2^{-k}, \ s_1^{km} = s_2^{-km} = s_2^{kn}, \ s_2^{k(m+n)} = 1 = s_1^{k(m+n)}$$

To establish the latter fact it may be noted that when p^α, $\alpha > 0$, is the highest power of p which divides k, and p^β is the highest power of p which divides $m + n$, then we let t_1 represent a constituent of s_1 of order $p^{\alpha+\beta}$ and $t_1^{-1+lp^\beta}$ the corresponding constituent of s_2, where l is so chosen that $t_1^n = t_1^{-n+nlp^\beta}$. As n is prime to p, the product nl represents all the residues prime to p mod p^α, and hence it follows that G involves the cyclic subgroup of order $p^{\alpha+\beta}$.

It remains only to prove that when q^γ is the highest power of the prime number q which divides $m + n$, q being prime to k, G involves a cyclic subgroup of order q^γ. Let t_2 be a generator of such a group. As $t_2^{m+n} = 1$ it follows that $t_2^m = (t_2^{-1})^n$, and hence s_1 may be supposed to have

t_2 as its constituent whose order is a power of q while s_2 has t_2^{-1} as its corresponding constituent. It has, therefore, been proved that when m, n, k do not have a highest common factor greater than unity, and s_1 and s_2 are commutative, the largest group which they can generate subject to the condition $s_1^m = s_2^n$, $(s_1s_2)^k = 1$ is the cyclic group of order $(m + n)k$.

When the highest common factor of m, n, k is $d > 1$ the Sylow subgroups of G whose orders are prime to d are seen to be cyclic just as in the preceding case. The order of every operator in such a group must divide $(m + n)k/d$, since $s_1^k = s_2^{-k}$ and hence $s_1^{\frac{km}{d}} = s_2^{-\frac{km}{d}} = s_2^{\frac{nk}{d}}$, $s_1^{-\frac{kn}{d}} = s_1^{\frac{km}{d}}$. Let r be any prime divisor of d and let r^ϵ be the highest power of r which divides d while $r^{\theta + \epsilon}$ is the highest power of r which divides $(m + n)k$. From the preceding proof it follows that G involves an operator t_3 of order r^θ since such an operator generates a Sylow subgroup of the group determined by the equations

$$s_3^m = s_4^n,\ (s_3s_4)^{k/d} = 1.$$

Suppose that s_3 generates t_3. If an operator which generates the Sylow subgroup whose order is a power of r contained in the group generated by s_4 is multiplied by an independent operator of order r^ϵ this product and s_3 satisfy the conditions

$$s_1^m = s_2^n,\ (s_1s_2)^k = 1.$$

Hence it results that the Sylow subgroup of G whose order is a power of r involves two independent operators of orders r^θ and r^ϵ, respectively. To complete a proof of the theorem under consideration it is, therefore, only necessary to establish the fact that $r^{\theta + \epsilon}$ is the order of a Sylow subgroup of G.

When k is not divisible by $r^{\epsilon + 1}$ this fact is evident since the order of neither of the two operators s_1, s_2 is divisible by $r^{\theta+1}$ and the order of at least one of them is divisible by r^θ. If the order of the Sylow subgroup in question would exceed $r^{\theta + \epsilon}$ the order of the product of these two operators would have to be divisible by $r^{\epsilon + 1}$. When k is divisible by $r^{\epsilon + 1}$ at least one of the two numbers m, n is not divisible by $r^{\epsilon + 1}$. Suppose that m is not divisible by $r^{\epsilon + 1}$. Hence the order of the Sylow subgroup in question cannot exceed r^ϵ times the order of the corresponding Sylow subgroup in the group generated by s_2. This completes a proof of the theorem in italics announced in the second paragraph of this note.

This theorem is useful in the study of the generalized groups of the regular polyhedrons as may be seen from the fact that if two commutative operators satisfy the equations $s_1^3 = s_2^4$, $(s_1s_2)^2 = 1$, they generate the cyclic group of order 14 or one of its subgroups. This explains why the largest non-abelian group, which is generated by two operators satisfying these conditions, is a direct product of the group of order 7 and some other group (Miller, Blichfeldt, Dickson, *Finite Groups*, 1916, p. 157).

Mr. W. E. Edington, graduate student in the University of Illinois, directed my attention to the desirability of a study of the given general equations as regards commutative operators with a view to simplifying the treatment of the generalized polyhedron groups.

MODEL OF THE LINKAGE SYSTEM OF ELEVEN SECOND CHROMOSOME GENES OF DROSOPHILA

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Communicated December 26, 1919

In previous papers[1] reasons have been given for thinking the spatial relations of genes to be three-dimensional rather than linear as assumed in the hypothesis of Morgan and his colleagues. In particular it has been shown that in models made to represent the spatial relations of the genes in the sex chromosomes of two different species of Drosophila, using the original unmodified data, as reported by Morgan and Bridges[2] and by Metz,[3] the arrangement is apparently three-dimensional. Recently data have become available for modeling in a similar way the interrelations of the genes in the so-called "second chromosome" of Drosophila ampelophila.[4] Two views of such a model are seen in figures 1 and 2. The model shows the linkage relations of eleven genes, those for which the data are most complete and reliable according to Bridges and Morgan. The data are taken from table 140 of the publication mentioned. Each gene is represented in the model by a small ring of wire and it is connected with another gene by a wire as long in inches as is the cross-over value, in per cent, between the two genes joined by the wire. In the figures the model is suspended by S, the view shown in figure 2 being taken at right angles to that shown in figure 1. The eleven genes represented in the model are as follows:

S, star (0.0)
Sk, streak (15.4)
d, dachs (29.0)
b, black (46.5)
pr, purple (52.7)
vg, vestigial (65.0)
c, curved (73.5)
px, plexus (96.2)
a, arc (98.4)
sp, speck (105.1)
mr, morula (106.3)

The numbers in parentheses indicate the position of each gene in the linear "map" of Bridges and Morgan (p. 127). It will be observed that seven of the genes in this list are represented in the "map" at distances greater than 50 from star, which lies at 0. In the previous publications already cited, I have maintained that since cross-over percentages of 50 or more have not in any case been observed and are logically impossible as a result of linkage, map distances should not be shown in excess of 50. But further consideration of the matter leads me to withdraw this objection. If the arrangement of the genes is really linear, as assumed by Morgan, then double crossing-over is a sufficient explanation of why cross-over percentages do not increase as fast as map distances. But double crossing-over is the questionable phenomenon which need not be assumed if the arrangement is non-linear. Hence, by constructing models such as are shown in figures 1 and 2, I have attempted to ascertain whether a workable system could be found which was able to dispense with double cross-overs, assuming a non-linear arrangement. It seemed to me that such was the case even after the model seen in figures 1 and 2 had been constructed. I next examined the data given by Gowen[5] for the linkage relations of eight genes of the "third chromosome" of Drosophila. On constructing a model to show the spatial relations of the genes, without assuming the arrangement to be linear, and without making any allowances for double or triple crossing-over, I obtained a figure strikingly similar to

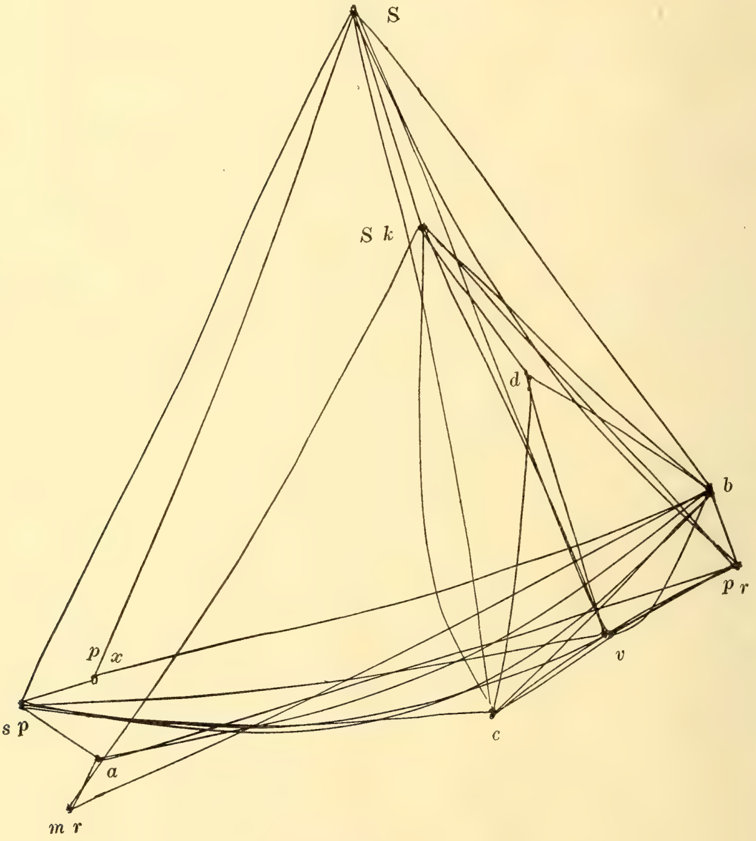

FIG. 1

the model of the sex-linked group of genes. The eight genes fell into a curved band lying nearly in one plane, the model being quite flat as viewed

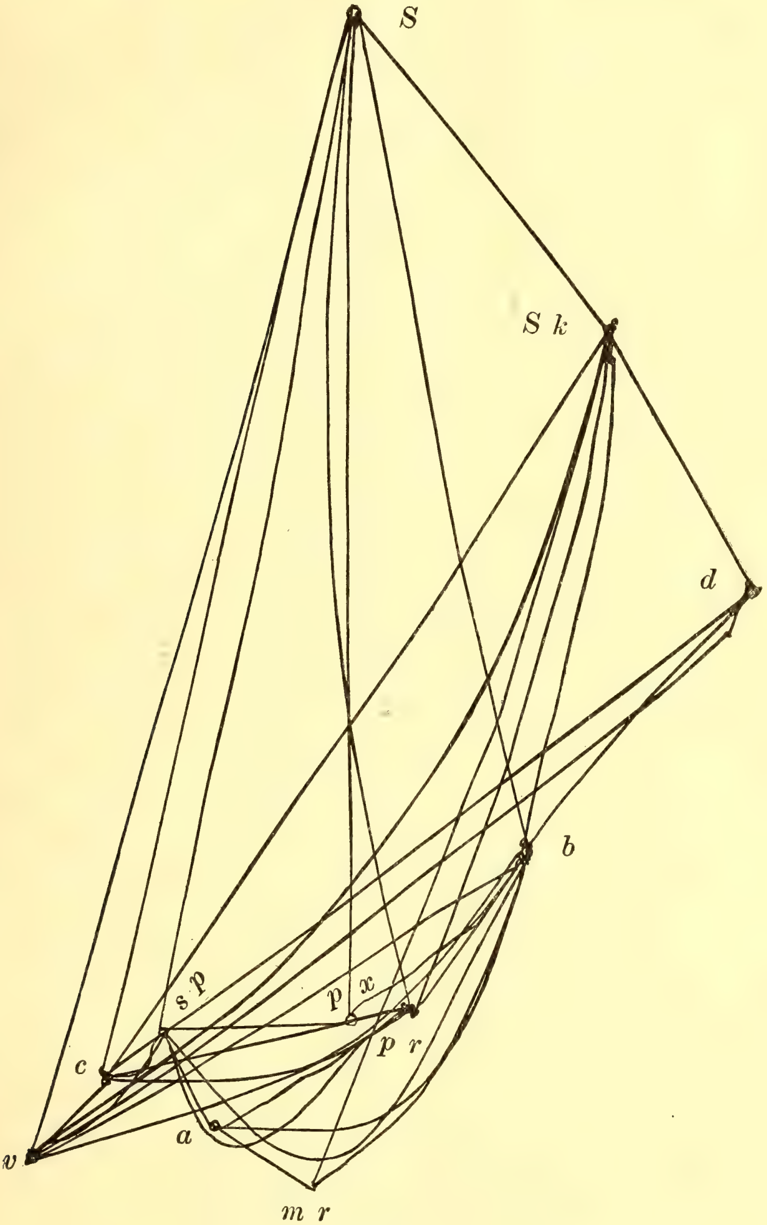

FIG. 2

edgewise. The figure indeed is, as Sturtevant, Bridges and Morgan[6] said concerning my model of the sex-chromosome linkage system, "capable of reduction to a curved line lying in one plane." Obviously the arrangement approaches the linear, but it is curvilinear rather than rectilinear as shown in the chromosome maps. But if we grant that the arrangement is in any sense linear, then it must be granted also that double and triple crossing-over are likely to occur, indeed we cannot in that case reasonably explain the observed regrouping of characters on any other assumptions. But if double crossing-over occurs oftenest when long distances are involved, it follows that the long distances will have been apparently shortened. If now in the model based on Gowen's observations, we lengthen the long distances by counting twice each assumed double cross-over and thrice each assumed triple cross-over, we do in fact obtain a distribution of the eight genes substantially in a *rectilinear band.* This strongly supports the linear hypothesis. Yet there remains the serious logical difficulty that the long distances were already long enough before we increased them by allowing for double crossing-over. When so increased they will of necessity be too long, since they will now exceed 50, the limiting value for cross-over percentages involving linkage. It must then be frankly recognized that map distances do not correspond with cross-over percentages (although they are based on them) except when the cross-over values are very low. This has been made clear by Haldane[7] who designates the map units "morgans" to distinguish them from the directly observed cross-over percentages. He gives a useful table for converting cross-over percentages into map-distances and vice versa. How widely the two may diverge is illustrated in the following table in which are shown the linkage relations of the gene, star, with other second chromosome genes.

	OBSERVED CROSS-OVER PERCENTAGE (TABLE 140, BRIDGES AND MORGAN)	LINEAR MAP DISTANCE (FIG. 72, BRIDGES AND MORGAN)
Star—streak	15.9	15.4
Star—dachs	Not given	29.0
Star—black	37.9	46.5
Star—purple	43.7	52.7
Star—vestigial	43.3	65.0
Star—curved	45.9	73.5
Star—plexus	46.7	96.2
Star—arc	Not given	98.4
Star—speck	Not given	105.1
Star—morula	Not given	106.3

It appears from the table that cross-over percentages, compared with map distances, increase very slowly after they have once become as high as 40. According to Haldane's table, the cross-over percentage would not attain 50 until the map-distance reaches infinity.

Has the model figured in this paper any value either to confirm or to disprove the linear hypothesis? It does not, like the models of chromosomes 1 and 3, show the genes in a curved band lying in one plane. But it does show them lying roughly in a linear chain winding cork-screw fashion through the model. If all wires over 40 inches long were cut, the system would straighten out so as to approach a linear arrangement. I conclude that the model supports the linear hypothesis, if it be supposed that the longer distances have been shortened by double crossing-over, and that map distances in such cases should exceed observed cross-over percentages.

[1] These Proceedings, **5,** 25, 32, and 500.

[2] *Carnegie Inst. Washington, Publ.* **237,** 1916.

[3] *Genetics,* **3,** 1918 (107–134).

[4] Bridges, C. B., and Morgan, T. H., "The Second Chromosome Group of Mutant Characters," *Carnegie Inst. Washington, Publ.* **278,** 1919 (123–304).

[5] Gowen, J. W., "A Biometrical Study of crossing Over." *Genetics,* **4,** May, 1919 (205–250).

[6] These Proceedings, **5,** May, 1919.

[7] Haldane, J. B. S., *Genetics,* **8,** Sept., 1919 (306).

THE DEVELOPMENT OF CONNECTIVE TISSUE IN THE AMPHIBIAN EMBRYO

By George A. Baitsell

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Communicated by R. G. Harrison. Read before the Academy, November 10, 1919

The researches[1] in which the author has been engaged for several years have shown, in brief, that the structure of the plasma clot of frog's blood is of such a nature that when influenced by the proper mechanical factors, e.g., tension or pressure, its structure will be radically changed. In cases of this kind a fusion and consolidation of the minute elements of the clot will take place and, as a result, long fibers will be formed which unite in wavy bundles, anastomose with other bundles and ramify in various directions throughout the clot, thus resulting in the formation of a material which, at least in its morphology, is like certain types of connective tissue. In wounds in frog's skin, the experiments showed that a fibrous tissue formed from a plasma clot functioned, at least temporarily, as a normal connective tissue and there was no evidence to show that it would ever be replaced.

The above results led naturally to the question as to whether a similar process normally takes place in the histogenesis of connective tissue in amphibian embryos. The results obtained from these investigations are as follows:

1. The primitive forerunner of connective tissue in frog embryos is an amorphous, gelatinous material which, because of the fact that it stains

very lightly in the early stages by any of the numerous methods tried, is difficult to detect. This ground substance can be demonstrated around the notochord soon after it is formed and shortly before the embryo has reached the tail bud stage. A little later this material, which is to form a part at least of the connective tissues, surrounds the medullary cord and a layer of it, following the body wall, extends ventrally on either side and in time completely encircles the body cavity. The formation of this matrix around the notochord occurs before there is any syncytium of the mesenchyme cells in this region. In fact, at this stage, the cells here present have not yet assumed the shape of typical mesenchyme cells which later appear. It is evident, therefore, that this primitive ground substance of connective tissue has arisen as an intercellular secretion of the embryonic cells[2] and not by a syncytial fusion of cytoplasm.

2. The ground substance having been formed, cells begin to move into it and wander through it. These are spherical at first but, as they move through the ground substance, they soon change into various shapes, becoming stellate, spindle-shaped, etc. The study of the sections shows that, in general, individual cells do not separate from the cell masses and move out into the various cavities and open spaces of the embryo until after the formation of the secreted ground substance which is the forerunner of the connective tissues. The present work, therefore, adds additional evidence in support of the conclusions which Harrison[3] and others have reached with regard to the stereotropism of cells in tissue cultures. Cells, whether in tissue cultures or in the developing embryo, have need of a supporting framework of some kind in order for migration from the main masses to take place.

3. The connective tissue fibers begin to arise in the ground substance soon after it has formed. In some cases, they form in the ground substance in regions which are free from cells so that it is very clear that they have not arisen by any intracellular action. Instances can be noted in which it appears that the fibers are formed by a gradual transformation of the ground substance, first, into a delicate net-like structure and then into the long fibers which are typical of connective tissue. In its morphological features, at least, this process gives evidence of being identical with the one previously observed in the transformation of the plasma clot.

4. In other regions of the ground substance into which the cells have wandered, the formation of the fibers can also be shown to be due to changes in the ground substance and not to a sloughing off of the cell protoplasm. A differential stain, such as Mallory's connective tissue stain, shows very clearly the limits of the cell cytoplasm and the beginning of the ground substance. In many cases the paths of the cells can be traced through the ground substance and the possibility arises that, in their movements through the ground substance, they may give off secretions which play a part in the final chemical constitution of the fibers.

However, the fact that the fibers can form in a cell-free ground substance shows that no part of the cell cytoplasm is necessary for their differentiation from the ground substance.

5. Any satisfactory theory of connective tissue formation must take into account the well-established fact that the great mass of the connective tissues, both ground substance and fibers, is formed when not in contact with cells. The adherents of the intracellular theory of connective tissue formation, holding that both ground substance and fibers are living material, although of a different grade from that in cells, maintain that the embryonic connective tissue after it has been formed by an intracellular action has the power to grow through the assimilation of food material, and the ability to differentiate new fibers.[4]

6. The results of the present researches may be summarized as follows:

(*a*) The origin of connective tissue lies in an intercellular secretion of the embryonic cells. This secretion constitutes the ground substance of the connective tissues and it increases in amount during development not by an assimilation of food materials and consequent growth but by the continued and additional secretions given off by the embryonic cells.

(*b*) This ground substance secreted by the cells is of such a nature that, under the influence of various chemical and mechanical factors, it forms the connective tissue fibers by means of a consolidation of the minute elements of which it is composed in apparently the same way that a transformation of the plasma clot occurs.

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[1] Baitsell, G. A., (*a*) *J. Exper. Med.*, **21**, 1915 (455); (*b*) *Ibid.*, **23**, 1916 (739); (*c*) *Amer. J. Physiol.*, **44**, 1917 (109).

[2] The presence of a primitive ground substance has been demonstrated by various investigators. E.g., see Merkel, F., *Anat. Hefte*, **38**, 1908 (321); and Szily, A., *Ibid.*, **37**, 1907 (649). Both of these articles contain a full review of the literature.

[3] Harrison, R. G., *J. Exper. Zoöl.*, **17**, 1914 (521).

[4] E.g., see Heidenhain, M., *Plasma und Zelle*, pp. 33, 38, etc.

CALCIUM AND MAGNESIUM METABOLISM IN CERTAIN DISEASES

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Communicated by L. B. Mendel. Read before the Academy, November 10, 1919

We wish to present some observations upon calcium and magnesium metabolism in leprosy and in multiple exostosis.

Clinically, leprosy is characterized by a *loss* from the body of bone salts; in multiple exostosis bone salts are *added* to the organism for the calcification of the new growths. It is thus apparent that in these two abnormal conditions the processes involved in calcium and magnesium metabolism must stand in sharp contrast. The literature contains little data relative to mineral metabolism in either of these diseases.

In the present investigation the general plan followed was to place the subjects of experimentation upon a known diet low in calcium and magnesium for a definite period. Calcium or magnesium was then added for a second period. Calcium intake was increased by the administration of milk. Magnesium was given in the form of the citrate. By the procedure outlined it is apparent that the organism was given a distinct task to perform. The manner in which this task was accomplished by the diseased organism was judged by comparison with normal subjects maintained under similar experimental conditions.

The experiments were made upon six subjects divided as follows:

(*a*) 2 normal subjects. (*b*) 2 leprous subjects. (*c*) 2 exostosis subjects.

In the study of leprosy one subject represented a well-advanced case; in the other the disease was in an incipient stage.

With exostosis one subject had advanced to the stage where the abnormal processes had become stabilized; the other case represented the active, less advanced stage of the disease.

Results.—When compared to normal individuals under the same experimental conditions leprous patients retain calcium in relatively large quantities whether the individual is maintained upon a calcium-poor or a calcium-rich diet; and the greater the intake of calcium the greater is the relative retention. In the more advanced stage of the disease the degree of retention is greater than in the early phase.

This retention cannot be explained by differences in calcium intake or by variation in the power of absorption. It is apparently an evidence of a metabolic demand for calcium induced by the disease. Clinically, it may be inferred that there is a need for calcium; experimentally, the organism gives evidence of this need by retention of calcium administered.

The study of magnesium exchange in normal and leprous subjects demonstrates that in the advanced stage of leprosy there is a tendency for a retention of magnesium. The degree of retention is much less marked than in the case of calcium.

With the exception of the retention of calcium and magnesium the leprous organism responds to changes in intake of calcium and magnesium in the same manner as that exhibited by the normal subject.

It is, perhaps, self-evident that the loss of bone salts in leprosy is but a manifestation of the disease processes. Nevertheless it is quite conceivable in dietary conditions where calcium is not particularly abundant that the lack of this element may play a material rôle in the rapidity of the progress of the disease. Conversely, the suggestion is pertinent that the progress of the disease may be greatly retarded or perhaps even alleviated if an abundance of calcium is present in the diet.

The suggestion that plenty of calcium and magnesium be supplied in the food in leprosy as a therapeutic measure is at least worthy of trial.

Multiple Exostosis.—From a consideration of the study of multiple exostosis it may be concluded that in the stabilized stage of this disease calcium exchange differs little from that of a normal individual whether the abnormal subject is maintained upon a calcium-poor or a calcium-rich diet.

In the progressive stage of the disease calcium metabolism is markedly different from the normal in that calcium is lost from the body in relatively large amounts when the subject is maintained upon a calcium-poor diet. This excessive elimination of calcium is excreted by both the urine and feces in a normal ratio. When placed on a calcium-rich diet calcium is retained in the body to an extent not widely deviating from that obtaining in normal subjects. A resumption to a calcium-poor diet again induces excessive calcium elimination.

In the stabilized stage of multiple exostosis magnesium excretion is two or three times as great as the intake whether the subject is maintained upon a diet poor or rich in magnesium.

In the progressive stage of the disease the general type of magnesium excretion resembles that of the stabilized stage but the *degree* of elimination is not so marked.

Magnesium added to the diet in the stabilized stage results in the prompt excretion of the same. The same test applied to the progressive stage gives evidence of the retention of some magnesium. The degree of retention, however, is much less than that shown by normal individuals.

Absorption of both calcium and magnesium in multiple exostosis is not inferior to that of normal subjects.

The facts enumerated suggest that in the early stages of this proliferative type of bone disease under discussion a check may perhaps be given to the progress of the disease by proper dietary procedure—restriction of calcium and magnesium intake.

Summary.—Clinically, in leprosy there is evidence of loss of bone salts; experimentally, the leprous organism gives evidence of a marked tendency to retain or conserve bone salts, more especially calcium.

Clinically, in multiple exostosis it may be inferred that bone salts are added to the body; experimentally, in the progressive stage of the disease the organism attempts to rid itself of bone salts, particularly calcium. In the stabilized stage the organism throws out excessive quantities of magnesium.

These facts suggest possible beneficial results in treatment by the institution of proper dietary procedures.

THE LARGER OPPORTUNITIES FOR RESEARCH ON THE RELATIONS OF SOLAR AND TERRESTRIAL RADIATION[1]

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Communicated by the National Research Council, December 8, 1919

The earth is maintained at its present temperature by a balance between the solar radiation received and the terrestrial radiation emitted. The mean intensity of the solar radiation as it is at the earth's mean distance outside the atmosphere is believed to be well known. However, it appears to be variable from year to year and also irregular over short intervals of days, weeks or months. These variations, which have been discovered and to a certain extent followed for about fifteen years by the Smithsonian Astrophysical Observatory at its station on Mt. Wilson, have lately been compared with terrestrial temperatures and atmospheric pressures at different stations of the world by several meteorologists, notably by Clayton, and it appears that the effects of solar variations are important as governing changes of terrestrial temperature.

This indicates two highly desirable researches: First, to make daily observations of the sun competent to determine its variations. Second, to determine by further statistical inquiries the exact influence of the variations of the sun on the terrestrial phenomena.

In order to carry out the first research thus outlined, it requires not less than four special solar radiation observing stations in the four most cloudless regions of the earth. One such is now in existence at Calama, Chile, where it is being maintained by the private funds of the Smithsonian Institution. If there was available the necessary income of thirty or forty thousand dollars a year, the Institution would establish the required observing stations and would arrange for the daily wireless transmission of the results obtained on solar radiation so that they might be available for meteorologists in all parts of the world to use for forecasting purposes. The same could be done by international coöperation if due regard were given to making the measurements homogeneous. It is not probable that any steps of this kind will be taken within a year or two. By that time the series of measurements being made at Calama, Chile, will enable meteorologists to determine to what extent the results will be valuable for forecasting purposes.

It would promote the proposed solar investigation if there could be devised autographic recording instruments for determining the exact value of the cloudiness at all times of the day when the sun is above the horizon. Hitherto measurements of cloudiness have been made at comparatively few stations, and as they depend upon personal estimates of observers who are apt to overlook wisps of cirrus clouds especially dangerous for solar radiation work, and as they are usually taken only two or three times in the day, they are quite inadequate to enable the investigator interested in establishing solar radiation stations to select the regions of the earth best meeting his requirements of cloudlessness. It would, therefore, be a very valuable investigation if some suitable autographic method of measuring cloudiness could be devised simple enough to be employed at numerous stations reported to be cloudless.

The whole subject of the cloudiness of the earth is very important. Recent measurements of Aldrich show that a cloud surface reflects about 78% of the solar radiation incident upon it. As the cloudiness of the earth is reputed to be about 50%, it is obvious what a large part clouds play in determining the temperature. They are effective not only in cutting off incoming radiation from the sun but also outgoing radiation from the earth. If the simple autographic apparatus above mentioned were available, it should be employed at numerous stations in all parts of the world and if possible on the ocean.

The measurement of solar radiation at several stations on the earth's surface by means of the pyrheliometer has occupied observers in many countries. Several series of observations have been carried on for many years by the U. S. Weather Bureau, and are published in excellent form in the *Monthly Weather Review.* Many copies of the Ångström pyrheliometer and of the Smithsonian silver disc pyrheliometer, whose constants are both so well known as to form together a homogeneous system of pyrheliometry, have been sent out to many quarters of the world. Only a few series of regular observations are at the present time being conducted with them, and it is greatly to be hoped that a large number of such series may be conducted regularly in the future. The method of reducing the results and publishing them can hardly be improved over that which is followed by the United States Weather Bureau, and that may well be taken as a model.

The study of the outgoing radiation from the earth has lately been carried on by several observers, notably by Mr. Fowle, who has investigated the effects of water vapor in long atmospheric columns in diminishing the rays of long wave-length such as the earth sends out. His investigations extend only to a wave-length of 17 microns because no suitable optical media transparent to waves of longer wave-length are available. It appears that potassium iodide may be such a medium and it is hoped shortly to produce large crystals of the salt suitable for making prisms for the study of long wave-lengths of terrestrial radiation.

In the meantime, valuable information has been obtained on terrestrial and atmospheric long wave-length radiation by Dr. A. K. Ångström by the use of the pyrgeometer or nocturnal radiation instrument devised by his father. An instrument applicable for the same purpose has also been devised by Messrs. Abbot and Aldrich of the Smithsonian Institution and is called by them the pyranometer. Neither instrument is entirely satisfactory for very long waves since both depend upon a knowledge of the absorption of blackened surfaces to long wave-lengths, and this is a matter which still lies in doubt. For satisfactory progress in this line of the measurement of nocturnal radiation, a new instrument depending upon the principle of the hollow chamber or absolutely black body ought to be devised.

The study of the effect of ozone on the temperature of the earth is one which ought not to be neglected. Next to cloudiness and atmospheric humidity, ozone is thought to play the most important part in its effect on terrestrial radiation; first, because the quantity of ozone in the atmosphere is believed to be variable, and, second, because its effect upon terrestrial radiation occurs near the wave-length 10 microns where water vapor is almost completely transparent. This research should include the determination of the variations of the quantity of atmospheric ozone and the measurement of the effect of its changes on the ozone band near 10 microns in wave-length.

A fuller discussion of the several points which have been raised above will be found in the following papers.

INVESTIGATIONS IN SOLAR RADIATION

Survey of the Present State of the Field

We may inquire: First, what is the intensity of the radiation of the sun on which all the life of the earth depends; second, is this intensity constant or variable from day to day and from year to year, and what are the effects of variability, if any?

1. *The Solar Constant.*—The question of the absolute amount of the solar radiation interests us not only for our own sake but for the sake of the future generations. What a valuable thing it would be if we knew at this time the intensity of the sun's radiation as it was in the times of the Egyptians and the Babylonians, and as it was in the intervening periods of Rome and the Middle Ages. It is plainly our duty to transmit to posterity accurate measure of the intensity of the sun's radiation as it is now, so that they will be in a better position in this branch of science than we find ourselves. We cannot expect to know the intensity of the sun's radiation as it would be in space at the earth's mean distance (that is, the so-called "solar constant of radiation") to that high degree of accuracy which we are accustomed to demand in many branches of physics. If we determine that value to one per cent, it is all we may reasonably hope for, because the intervention of the earth's atmosphere, with its changing amounts of haze and cloudiness will always interpose to the investigator an insuperable obstacle to the highest degree of accuracy. It may be that in the future apparatus can be shot up by means of rocket devices to go outside of the atmosphere altogether, but probably the sources of error of automatic apparatus for such a research will be found so large

that no greater degree of accuracy than to one per cent will be possible even with them.

The spectrobolometric investigations of this subject by the Astrophysical Observatory of the Smithsonian Institution which have been continued for the last seventeen years have yielded the general indication that the value of the solar constant of radiation is within one per cent of 1.93 calories per square centimeter per minute. This result is confirmed by experiments made with sounding balloons by the same observatory in coöperation with the U. S. Weather Bureau in 1914. Automatic recording pyrheliometers were sent to a height of about 25 kilometers. It was found that the intensity of the radiation there, at a point where 24/25 of the atmosphere was beneath the observing instrument, fell within the range of solar constant values as obtained by spectrobolometric work at the surface of the earth. The actual value obtained by the balloon work was 1.84 calories per square centimeter per minute. Allowing for the obstruction of the sun rays by the still superincumbent atmosphere a reasonable extrapolation would give 1.88 calories. This differs from 1.93 calories less than the error of the observations and is well within the range of solar constant values obtained at different days from surface observatories.

Apparently the close agreement of these widely different independent methods of obtaining the intensity of solar radiation outside our atmosphere permits us to say authoritatively that the mean value of the solar constant is at any rate between 1.9 and 2.0 calories per square centimeter per minute and most probably not greatly different from 1.93. A few men cling to the view that much higher values should be set for it—values between 3 and 4 calories per square centimeter per minute, but their arguments carry little weight, as it seems to me, and those best qualified to know are agreed that the values given above have a strong foundation.

The methods of observing the solar constant of radiation by means of the spectrobolometer have been carefully worked out and the whole subject has been published with satisfactory fullness and is in shape to be transmitted to the scientific men of the future in order to enable them to reproduce the measurements or to compare their own measurements with the results now obtained.

2. *The Sun's Variability.*—Passing now to the second branch of the subject, the investigations of the Smithsonian Astrophysical Observatory conducted at Washington, Mt. Wilson, Mt. Whitney, Bassour (Algeria), and now the investigations supported by the Smithsonian Institution from its private funds in North Carolina and Chile have all united in giving the impression that the solar radiation is not constant, but varies from day to day through a range of certainly five and possibly at times ten per cent. The conclusion that the sun is a variable star is confirmed in several ways but most notably by the results of measurements made by

the Smithsonian Astrophysical Observatory at Mt. Wilson, California, on the distribution of energy along the diameter of the solar image. These measurements indicate, as was well known before, that the edge of the sun's disc is less bright than the center, and that the contrast of brightness between the center and the edge varies according to the wave-length of light, being greater for short wave-lengths, less for long.

But the measurements of recent years have shown that not only is there a variation of contrast by wave-length, but also a variation of contrast with the time. The contrast in each wave-length is different for different days of observation and, on the average, for different years of observation. The changes of contrast have been compared with the changes of total radiation of the sun determined by the aid of the pyrheliometer and spectrobolometer, and it is found that there is a moderate degree of correlation between them. The correlation is of two kinds. For variations of long periods of years, high values of the solar constant are found associated with high values of contrast between the center and edge of the sun. On the contrary, for the short period variations of the solar radiation, occupying a few days, weeks, or months, it is found that high values of the solar radiation are associated with diminished values of the solar contrast.

The cause of this two-fold variation is reasonably explained. When the sun grows hotter and thus increases its output of radiation along with increased solar activity, as indicated by sun spots, prominences, and other visible solar phenomena, this would tend to cause a greater degree of contrast. For since if the solar temperature were zero there would be zero contrast, the higher the temperature the higher the contrast. But the sun is probably entirely gaseous, and certainly its outer layers are so, and these may become more turbid at times, just as the earth's atmosphere becomes more hazy at some times than at others. Accompanying increased turbidity of the solar atmosphere there would be found a diminished value of the solar constant of radiation. But since the path of the solar ray is oblique in the solar atmosphere near the edge of the sun, the path is longer there and the effects of the turbidity would be greater at the edges rather than at the center. Thus with the increase of turbidity the contrast of brightness would increase accompanying a diminished value of the solar constant of radiation. In this way it appears that the two-fold variations of the sun which have been found may be reasonably explained.

Investigations Required

1. *Solar Radiation Stations.*—We now pass to the applications of this discovery of the variation of the sun. What effects does it have upon the climate of the earth and upon other terrestrial phenomena? In order to cover satisfactorily this interesting and utilitarian branch of the investigation it is necessary to have good solar observations daily. Unfortunately there is no region in the world where the sky is so free from cloudiness

that a single observatory can obtain daily measurements of the intensity of solar radiation as it is outside the earth's atmosphere. The spectrobolometric investigation of the solar constant of radiation requires that the observations shall extend over several hours from the time when the sun's altitude is small to the time when it is large, so as to determine for many wave-lengths of the spectrum what increase of intensity corresponds to the diminution of the length of the path of the solar rays within the earth's atmosphere. In order to prevent error in the determination of the transparency of the earth's atmosphere, the sky must be uniformly clear during the interval just mentioned.

This is a condition very hard to fulfil. All stations in the world fulfil it well enough for certain days of the year, some stations for very many days of the year, but none for all. The experience of the Smithsonian Astrophysical Observatory in these matters indicates that probably there is no region in the world where more than 250 satisfactory days in the year can be confidently expected. Accordingly, it is highly desirable if these solar radiation changes have notable effects upon the earth's temperature and other meteorological and terrestrial phenomena, that several observatories shall join in making the measurements of the solar constant of radiation from day to day. On every day at least two values should be obtained strictly comparable with one another in their method of observation and with a probable accuracy as far as experimental work is concerned to the order of one per cent. The mean of two, or still better, of three or four such values, would give a probable accuracy of the final result to better than one per cent, and if a series of such observations extending over every day in the year for a course of several years were available, it would probably be possible to determine very well the effect of the changes of solar radiation on the terrestrial climate.

In order to carry on the work in this way, a considerable income is required. The experience of the Smithsonian Institution in its several expeditions to Africa and to South America, and to the western part of North America, indicates that an income of between five and ten thousand dollars, preferably as much as eight thousand dollars per year, for each station would be requisite. If four stations should be considered competent to carry out the problem, it would, therefore, require a yearly income of from thirty to forty thousand dollars for the whole series. It would be preferable that the work should be under a central direction so that the various measurements might be strictly comparable and homogeneous. If the Smithsonian Institution, with its long experience in these matters, had available for this purpose an income of the amount specified, that would offer a good way of dealing with the problem.

On the other hand, it may be that support for the project would be more easily obtained if the several governments of the regions where the observatories for this purpose might be favorably located should be approached and asked to take over the work. The establishment of such new observatories seems not to be urgent for another year, until the results obtained in Chile by the Smithsonian Expedition shall have been compared with the climate of Argentina, as is now being done by Mr. Helm Clayton, so as to see if there is a well-justified expectation that solar observations of this kind will be useful in meteorology.

2. *Autographic Cloud Observers.*—Looking to the possible establishment of such solar constant observing stations, the most important preliminary investigation relates to the choice of the most favorable situations. Experience hitherto has shown that the reported results of observations of cloudiness by meteorological services do not give a sound index of the availability of situations for this exacting work. In all of our experiments we have invariably been disappointed in not observing as many days as the reports of the cloud observations have led us to expect. Certain portions of the earth, it is true, have the reputation of being cloudless, among them the Chilean Desert, the Central and Western Provinces of Australia, some parts of South Africa, Upper Egypt, the Desert of Sahara, the Desert of Arabia, regions in British India, and possibly others. What would facilitate the establishment of solar observing stations more than anything else, in case they should seem desirable, would be to have a long series of cloud observations obtained by strictly automatic processes, independent of the judgment of observers, for a considerable number of the most promising regions, such as those I have mentioned. Such observations should cover the whole period when the sun is above the horizon.

Dr. Humphreys has proposed to me an instrument for such a preliminary investigation of cloudiness. It is based on the principle of photographing a reflecting ball from above. The sensitive surface would be that of a moving film or plate which would be exposed for an instant once in a quarter or half an hour, as would be most convenient, and would, therefore, give a series of photographs of all the clouds which occurred in the sky at the instants of observation. If this or some other suitable automatic device for photographing the whole sky could be prepared and employed by the meteorological services of the world which operate in the most cloudless regions, the work of establishing solar constant stations, if these should be needed, would be materially advanced. Otherwise, there is no method except to go to a region that is recommended and be disappointed and sorry that the region was chosen but without knowing where else to go, just as has occurred before. This is a disheartening, costly and time-consuming process, and might, perhaps, be avoided in the way above suggested.

3. *Pyrheliometric Observations.*—While the measurement of the solar radiation by means of spectrobolometric work is the only thorough way available in which to determine the variation of the sun, situated as the observer must necessarily be beneath an atmospheric sea full of haze and

other obstructing materials, yet it is highly important to have measurements of the radiation of the sun made at the earth's surface in many parts of the world. A series of measurements of this kind is being conducted by the U. S. Weather Bureau at its stations in Washington, Madison, Lincoln, and Santa Fe. This work, in charge of Professor Kimball, has gone on now for about ten years, and is being published in excellent form in the *Monthly Weather Review* of the U. S. Weather Bureau. Observations were also undertaken in Egypt and a report was published several years ago by Dr. Shaw. Observations were carried on for a number of years by the Harvard College Observatory at Arequipa, in Peru, but about a year ago the instrument was accidentally broken. The Arequipa series was then discontinued, partly because the Smithsonian Institution has its spectrobolometric and pyrheliometric station in Chile not far from Arequipa. Some measurements are being made also in Argentina, Brazil and elsewhere. There are also pyrheliometric instruments at the Island of Teneriffe which were in use before the war, but whether the measurements are being conducted there regularly now I am not aware. Dr. Dorno, of Davos, Switzerland, has made such measurements with many others of related subjects for a long time, and has just published a great volume of valuable results.

It would be very desirable if work of this kind could be carried on regularly at favorable stations employing strictly comparable instruments. There are about thirty copies of the Smithsonian silver disc pyrheliometer distributed to a number of different parts of the world but as stated above it is not probable that during the period of war they have been in regular use. Many of them are in very cloudy localities.

4. *Distribution of Radiation over the Solar Disc.*—As stated above, it has been found at the Smithsonian observing station on Mt. Wilson, in California, that the distribution of radiation along the diameter of the solar disc is subject to variability of two kinds—one of a long period of years, and the other of a short period of days, weeks, or months, and that these two kinds of variation are fairly closely correlated with the variations of the total radiation of the sun. The work of observing the radiation along the solar diameter requires the use of a long focus telescope and a spectrobolometer, and may very readily be carried on nearly simultaneously with measurements of the solar constant of radiation. Such investigation is going on regularly at Mt. Wilson during the summer season when the Smithsonian observers are working there. Perhaps it would not be necessary to set up other stations since the main object of this work was accomplished when it was shown that it confirmed the existence of the solar variability shown by the measurements of the solar radiation itself.

The method of observing the distribution of brightness along the solar diameter which we have adopted is to stop the clock-work of the coelostat which reflects the solar beam into the long focus telescope, so that the solar image drifts across the slit of the spectrobolometer in an east and west fashion. The question was, therefore, open for some time whether the distribution at right angles to this, that is, along the north and south diameter of the sun, would be substantially the same. In 1918, Mr. Aldrich investigated this matter by the aid of special mechanism associated with the coelostat and was able to show that if any difference in distribution exists between the north and south and the east and west diameter of the solar image, respectively, it is too small to recognize by this method and, indeed, is less than one per cent.

INVESTIGATIONS OF THE RELATIONS OF TERRESTRIAL CLIMATE TO RADIATION

Survey of the Present State of the Field

1. *Kinds of Radiation Concerned.*—The temperature of the world is maintained by the balance between the income of solar radiation and the outgo to space of terrestrial radiation. The former is principally included between the wave-lengths 0.3 and 3.0 microns. The latter is principally included between the wave-lengths 5.0 and 50 microns. Substances which are transparent for the one may be opaque for the other, though in these long ranges of wave-lengths substances may pass through several minima or maxima of opacity and transmissibility. Apart from cloudiness, which is the principal factor, the radiation of the sun is principally hindered in its passage to the surface of the earth by the water vapor of the terrestrial atmosphere and by the scattering of the molecules of the air and by the dust which the air carries. The losses of the direct solar beam by scattering from air molecules and dust are largely compensated for by the indirect solar radiation received from the whole hemisphere of the sky. On the other hand the rays lost from the direct beam by absorption in the water vapor of the earth's atmosphere, while they go to warm the atmosphere itself, are very differently applied to maintain the heat of the earth as a planet than they would be if they reached the earth's surface.

2. *Absorbents of Terrestrial Radiation.*—Apart from cloudiness, which is the principal hindering factor, the outgoing radiation of the earth's surface to space is hindered mainly by water vapor, ozone, and carbon dioxide. Of these the principal obstruction is water vapor which absorbs powerfully over great ranges of spectrum. The other two absorbents are each confined in their absorbing regions to comparatively narrow ranges of spectrum, but the ozone absorption band, at about 10 microns, occurs in a region where water vapor absorbs scarcely anything while the carbon dioxide absorption band at about 14 microns occurs in a region where water vapor is also powerfully absorbing. The atmospheric proportion of carbon dioxide is sensibly constant, while water vapor and ozone are variable. Accordingly, while water vapor is certainly the most important of the three,

probably ozone, although much less plentiful in the atmosphere, and certainly not more powerful as an absorber for the spectrum of a perfect radiator than carbon dioxide, is yet entitled to be regarded as second in importance on account of this peculiar posture of affairs.

3. *Studies of Terrestrial Radiation and Its Atmospheric Absorption.*—The study of these absorbing elements and their influence upon the spectrum has been carried on to a considerable extent by several investigators, notably by Paschen, Rubens and his collaborators, Miss Eva von Bahr, and lately by Mr. Fowle of the Astrophysical Observatory of the Smithsonian Institution. Mr. Fowle's researches[2] comprised the examination of the spectrum of the Nernst Glower up to a wave-length of about 17 microns, particularly as it is affected by the absorption occurring in long columns of atmospheric air containing known amounts of humidity and carbon dioxide. The experiments deal with quantities of from 0.003 to 3.0 centimeters of liquid water disposed in the atmosphere as water vapor. Owing to the lack of a suitable transparent refracting medium for optical purposes beyond the wave-length 17 microns, the important region of the terrestrial spectrum extending from 17 to 50 microns could not be investigated by Mr. Fowle. Somewhat later, Mr. Aldrich of the Astrophysical Observatory of the Smithsonian Institution made a long series of experiments (as yet unpublished) with crystals both natural and artificial and with other available substances, endeavoring to discover one which would be suitable for making a prism transparent to these rays. The only substance found which seemed to be notably superior to rock salt is potassium iodide, and this material has hitherto been available only in very minute crystals. Promising experiments made at the General Electric Company indicate that large crystals of potassium iodide for prisms may soon be available. If so, it will be possible to carry on this valuable spectrum investigation over a region hitherto very little explored. The dependence of terrestrial radiation on water vapor has been investigated to a considerable extent in another way by Dr. A. K. Ångström, who made observations in Algeria, Southern California, and lately in Sweden with the pyrgeometer, a nocturnal radiation measuring instrument invented by K. Ångström. Dr. Ångström's measurements were conducted during several months at a considerable number of stations varying in altitude from near sea level to that of Mt. Whitney, 4,400 meters, and in dryness from desert conditions to those of considerable humidity. At the same time measurements were made of the humidity prevailing in the air, partly at the surface by means of psychrometers and partly in the free air by means of kites and sounding balloons under the management of the United States Weather Bureau. This valuable investigation was supported by the Hodgkins Fund of the Smithsonian Institution, and was published as Vol. 65, No. 3, of the Smithsonian Miscellaneous Collections.

Investigations Required

1. *Ozone and Radiation.*—Very little is known concerning the influence of ozone on the earth's radiation. It is certain that a conspicuous band of this substance occurs at the wave-length of about 10 microns, where water vapor is almost completely transparent. It is a little hard to see how the investigation of the influence of ozone can be best undertaken. The region of 10 microns is very difficult to work with in the solar spectrum. All terrestrial surroundings give out waves of this wave-length. It is as if the investigator of the visible spectrum were troubled with stray light from every direction upon his photographic plates or other means of observation. Furthermore, and still more serious, the more intense rays of the solar spectrum, coming from regions of much shorter wave-length, are continually encountered as stray light in the spectrum, even hundreds of fold greater in intensity than the rays of the spectrum at the region in question. Rock salt must be employed for the optical train and its hygroscopic character is a serious difficulty. Finally the measurements ought to be quantitative in terms of energy. Something can be done to avoid stray light by reflecting the solar beam by means of several silvered ground glass mirrors. In this way the shorter wave-lengths are scattered, while the longer wave-lengths desired are reflected by virtue of the greatness of their wave-length, compared with the roughness of the mirrors. Yet the prospects of getting good bolographic representations of this region of spectrum day after day are not good. Perhaps the quantity of ozone existing in the air could be determined daily by measurements made in the ultra-violet spectrum by the aid of photography and these results could be compared with measurements of the nocturnal radiation made after the method of Ångström. Allowance being made for variations of temperature and water vapor, the influence of variations of the ozone might then appear. At all events, determination of the effects of ozone on the terrestrial radiation is a desirable investigation though so extremely difficult.

2. *Cloud Observations.*—The effect of clouds on terrestrial temperature is extremely important. Recent measurements by Mr. Aldrich of the Smithsonian Institution, made from a military balloon, show that a smooth layer of clouds reflects about 78% of the total direct and indirect solar radiation incident upon it from the sun and sky. Investigations of the quantity and distribution of the cloudiness of the surface of the earth have been published by Tisserenc du Bort, and Arrhenius. They depend, however, on very imperfect material. Cloud observations are much more fragmentary than temperature observations, and besides are not based upon standardized instruments in general, even if there be such, but are based on observers' estimates of the proportion of the sky obscured. The matter is so fundamental to the proper understanding

of the temperature of the world and its dependence on the balance of radiations of the sun and earth that it warrants careful attention. Some simple autographic means, theoretically satisfactory and instrumentally accurate, ought to be provided for estimating cloudiness, and this should be installed with the least possible delay at all first-rate meteorological stations in the world, and indeed as extensively as possible. The instrument should not be merely applicable to land conditions, but ought to be available for ships. It is hardly possible to overestimate the importance of this neglected branch of meteorology.

3. *Standard Instruments for Nocturnal Radiation.*—As mentioned above, Dr. Ångström has employed the pyrgeometer for an extensive and valuable research on nocturnal radiation and the determination of atmospheric re-radiation at different temperatures and under various other meteorological conditions. There has also been devised at the Smithsonian Institution, by Messrs. Abbot and Aldrich, an instrument called the pyranometer, which is applicable for the same purpose. Neither of these instruments, however, is thoroughly satisfactory, for this reason: They both present blackened flat surfaces which radiate towards the atmosphere and space, and it is assumed in the theory of the instrument that the absorbing and radiating properties of the blackening upon these strips is known. Experiments, however, seem to show that the materials used for the blackening purposes become less and less satisfactory for the longer wave-lengths, so that for rays beyond 15 microns there is a strong chance that the instruments are very imperfect. In order to really cover the objection it is necessary to produce a nocturnal radiation instrument which employs the well-known hollow receiver principle of the "black body." This is a matter of some difficulty because the intensity of the radiation to be considered is small, and it must be emitted and received at all angles within a hemisphere. There are very formidable difficulties in the way of a satisfactory solution of this requirement, but in order to give really definite information as to the nocturnal radiation of the earth and atmosphere they must be overcome. Associated with this problem is a definitive solution of the question of the constant of the fourth power radiation formula of Stefan. The measurements hitherto made by physicists on this constant, usually called σ, are in considerable disaccord. The margin of possible error is, perhaps, not very large, but it cannot be allowed as certain that we know this constant within 2%.

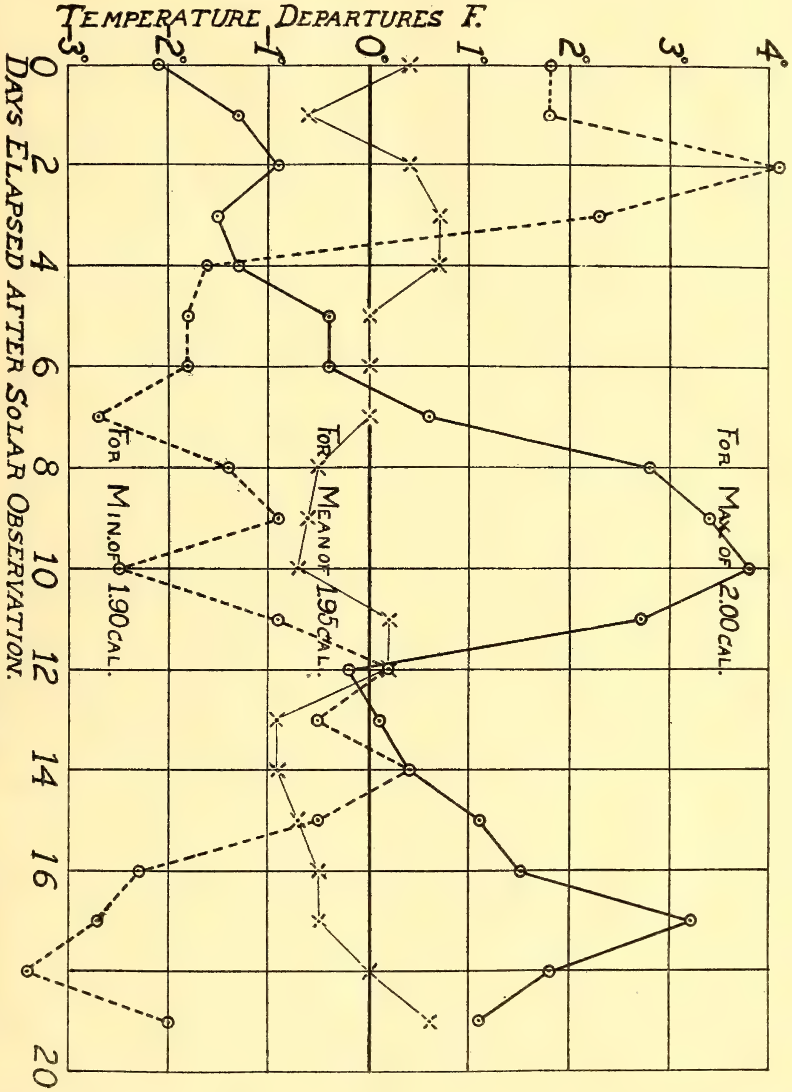
4. *Statistical Studies.*—As already stated, the sun is known to be a variable star. The effect of its variations on the terrestrial climate has lately been investigated to a considerable extent by Mr. H. H. Clayton[2] of Buenos Aires. Investigations of a similar kind have been made also by Dr. Arctowski of New York and by Dr. Helland-Hansen and Dr. Nansen[3] of Norway. These investigators have all reached the conclusion that the variations of the sun reported by the Smithsonian observers

at Mt. Wilson affect terrestrial temperatures to a considerable extent and also induce variations of terrestrial atmospheric pressure. Mr. Clayton's investigations have led him to the conclusion that the tropical and the polar regions of the earth are made warmer by temporary increases of solar radiation, while the temperate regions both north and south of the equator are made cooler by the same increase of radiation. Dr. Nansen, however, is of the opinion that when more thorough studies of the matter have been made, it will be found that the effects are not thus zonal, but rather are associated with the great action centers of atmospheric circulation. Dr. Nansen's view is that since a considerable portion of the sun's radiation is absorbed by the clouds, dust and water vapor in the atmosphere, a change in the amount of solar radiation may produce notable changes in the atmospheric circulation, and these, by altering the direction of the prevailing winds, may alter indirectly the temperature of meteorological stations. Hitherto the amount of investigation of the variation of the sun has been inadequate to give very satisfactory data to work with in determining these correlations. The present expedition of the Smithsonian Institution to Chile, it is hoped, will be continued for several years and will enable meteorologists to trace this relation much more thoroughly. Mr. Clayton, from the studies he has made and is making, is very sanguine about the value of solar radiation observations such as are being made by the Smithsonian observers.

The accompanying illustration, prepared from Clayton's studies, shows the remarkable courses of temperature departures at Buenos Aires, following high, intermediate and low values of the solar constant of radiation.

A very great deal of additional statistical inquiry is already possible in view of the series of solar constant observations reported by the Smithsonian observers on Mt. Wilson. There is now a much more continuous series of results available extending from July 27, 1918, as obtained at Calama, Chile. Meteorologists should be in a position to study these observations in connection with the temperature, pressure, cloudiness and wind at a very great number of stations distributed as well as possible over the earth's surface. It is only by such laborious investigation that a sound basis can be laid for the improvements in forecasting which Mr. Clayton thinks are now possible.

If it shall appear from such statistical investigations that the variations of the sun are really important for forecasting purposes, and if it shall prove possible to establish three or four additional solar radiation stations, widely scattered in the most cloudless regions in the world, then we may contemplate the possibility that from each such solar radiation station the results of each day might be sent out by wireless telegraphy, to be employed by the meteorological stations of the different countries for forecasting purposes. Experience in Calama, Chile, indicates that with a staff of three industrious and energetic persons it is possible to compute and send out the result of morning observations of a given day before night-fall. A brief empirical method for the determination of solar radiation has lately been devised at Calama and seems satisfactory, so that it is possible that the result of a morning of observation may be telegraphed to the world by noon.



THE GREAT AND PROLONGED INFLUENCE OF SOLAR CHANGES ON TERRESTRIAL TEMPERATURE

Average march of the mean temperature of Buenos Aires for 19 days following high, mean and low values of the solar constant of radiation for the years 1913, 1914, 1915 and 1918, in the months May to November. Solar radiation determined at Mount Wilson, California; temperatures by Argentine Weather Service.

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[1] This is issued as the second of a series of research surveys prepared under the auspices of the National Research Council. Reprinted in *Reprint and Circular Series of the National Research Council,* Number 7.

[2] *Smithsonian Misc. Coll. Washington,* **68,** No. 3, 1917.

[3] Now in course of publication by the Smithsonian Institution.

ON THE HYPERPLASIA OF NERVE CENTERS RESULTING FROM EXCESSIVE PERIPHERAL LOADING

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Communicated by R. G. Harrison. Read before the Academy, November 10, 1919

A number of investigators (Braus,[1] Shorey,[2] Dürken,[3] and Burr[4]) have shown that the destruction of peripheral areas in the embryo may result in an incomplete development (hypoplasia) of the nerve centers, either encephalic or spinal, which normally supply the peripheral area with nerves—the defective development resulting, supposedly, from a lack of functional demands which are ordinarily imposed upon the developing nerve centers. Particularly has this been observed in connection with experiments on the development of amphibian limbs.

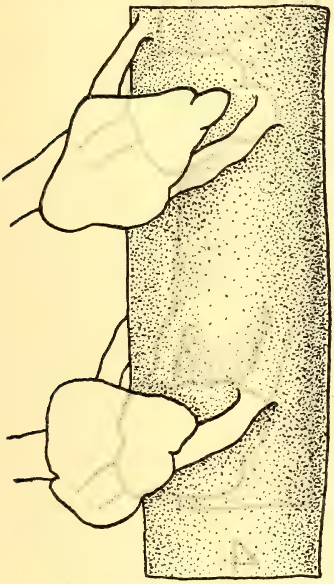
The cumulative evidence favors the idea that *complete* development of the nerve centers will not take place unless under the influence of the functional activity of the end organ. The extent, however, to which function may effect development is a question concerning which the results of investigators stand at variance. Moreover, no experiments have hitherto been carried out to show whether, by increasing the functional demands at the periphery, it is possible to effect a corresponding increased development (hyperplasia) of the nerve centers.

The experiments reported in this paper have shown that by effecting an increase in the peripheral area it was possible to bring about a hyperplasia of the sensory nerve centers. The experiments which have disclosed this fact consisted in transplanting the right anterior limb rudiment of Amblystoma punctatum a given number of body segments posterior to the normal position, whereby it was possible to study the effects of the continued function of limbs so placed upon the development of the sensory and motor nerve centers in that region of the cord supplying the limb with nerves. A brief account of the innervation and the function of limbs so transplanted has previously been published (Detwiler[5]).

Limbs transplanted the distance of more than three segments posterior to the normal position receive the bulk of their nerve supply from segments of the cord just posterior to the normal limb level. An examination of the peripheral nerves of this level shows that those contributing fibers to the functioning transplanted limbs are larger than their counterparts which are not connected with a limb.

In an endeavor to show whether this increase in the number of peripheral neurones was due to a hyperplasia of the motor centers, attempts were made to compare the number of motor nerve cell bodies in both halves of the spinal cord at the level in question. The larvae having been preserved at a relatively young stage (sixty to seventy days after the closure of the medullary folds), it was impossible to differentiate accurately between the nervous and non-nervous cells. Nevertheless, the results obtained from counting approximately eighty thousand cells, in addition to a comparative study of the size of the ventral roots, indicated that a hyperplasia of the motor centers had not taken place.

The evidence, however, of a hyperplasia of the sensory centers was very specific. In case $AS4_{26}$ the right anterior limb, which was transplanted the distance of four segments posterior to the normal limb level, was innervated by the fifth, sixth and seventh segments of the cord—the normal intact left limb receiving its innervation from the third, fourth and fifth segments.[6] The right sixth and seventh spinal ganglia (connected with the transplanted limb) were found to be considerably larger than the corresponding ganglia on the left side (not connected with a limb), the comparative sizes being illustrated in figures 3 and 4. The results of counting the sensory nerve cells in the ganglia on the two sides showed clearly that the hypertrophy was due to a marked hyperplasia of the cells. The removal of the limb from its normal position, likewise, resulted in a marked reduction in the sizes of the third and fourth ganglia which normally supply it with sensory nerves (figs. 1 and 2).



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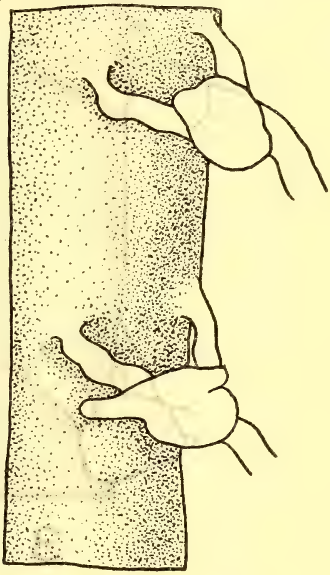

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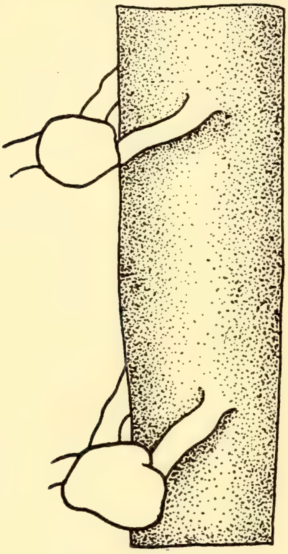
FIG. 1. Drawing of a reconstruction model showing the third and fourth left spinal ganglia (connected with the normal intact limb). × 50

FIG. 2. Drawing of a reconstruction model of the third and fourth spinal ganglia showing great reduction in size as a result of excision of the limb. × 50

By counting the cell bodies in the third and fourth ganglia on the left (those connected with the normal intact limb) and comparing with the number counted in the corresponding ganglia on the right side, where the peripheral area was greatly diminished as a result of the limb excision, a cell ratio between the normal and hypoplastic development was obtained. Further, by counting the number of cells in the sixth and seventh ganglia on the left (normal) side and comparing with the number found in the right sixth and seventh ganglia, which contributed innervation to the transplanted limb, it was likewise possible to obtain a ratio between the normal and the hyperplastic development. A cell count was also made of the right and left ninth ganglia, which supplied homologous and undisturbed peripheral areas, as a control for the hypoplastic and hyperplastic cell estimates.



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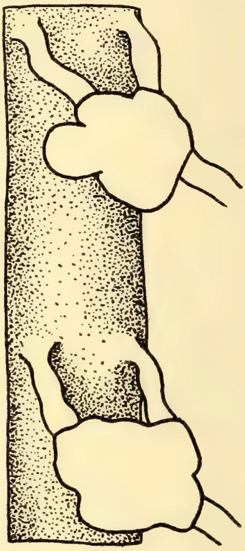

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Fig. 3. Drawing of a reconstruction model showing the sixth and seventh spinal ganglia which have no connection with a limb

Fig 4. Drawing of a reconstruction model showing hypertrophied sixth and seventh spinal ganglia supplying afferent innervation to a transplanted limb

Having determined the cell ratios, the weight ratios were obtained by weighing and comparing the weights of unassembled paper models of the ganglia. The results of both computations (cell and weight) are summarized in the accompanying table. The extensive hypoplastic development of the right third and fourth ganglia resulting from the limb excision is readily seen, the total number of cells in each ganglion being less than half the number present in the corresponding left ganglia with the limb intact. This is also emphasized by comparing the weights of the ganglia, the weight of the right third and fourth being less than half that of the corresponding left ganglia. The cell and weight ratios as seen in the table show an interesting approximation. The hyperplastic development of the right sixth and seventh ganglia connected with the transplanted limb is seen by comparing the number of cells in these structures with the number present in the corresponding left ganglia. A ratio closely approximating that of the cell count is shown by comparison of the weights.

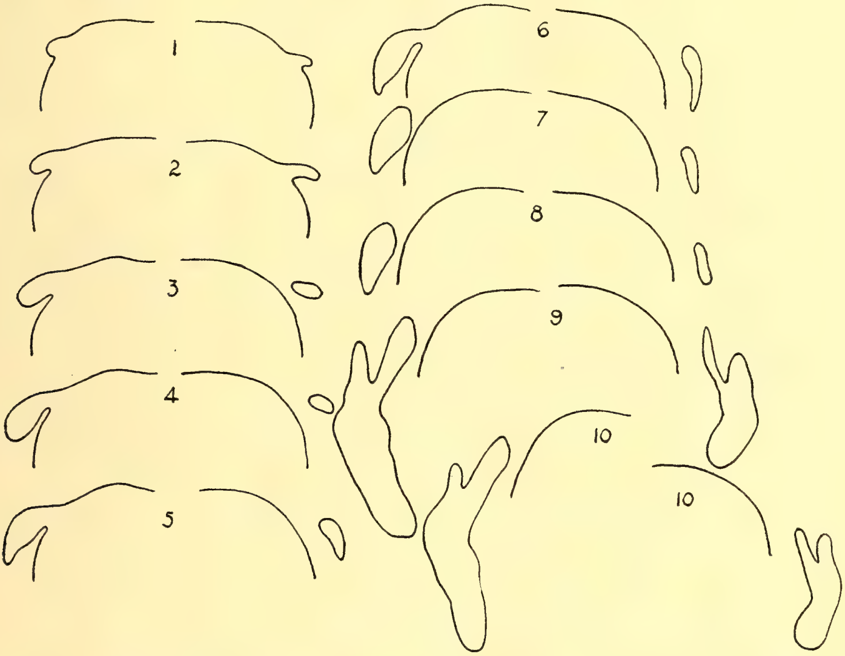

FIG. 5. Camera lucida drawing, showing serial sections of the sensory roots of the right and left third spinal nerve. The right root is greatly reduced in size as a result of excision of the right limb with which it is normally connected. × 40

Although a cell count of the fifth ganglia is included in the table, the results have no comparative value since both supply nerves to a limb. The cell content of the right ganglion connected with the transplanted limb is seen to be slightly greater than that of the left ganglion connected with the normal intact limb. The cell and weight ratios of the ninth ganglion which were used for a control show only an inappreciable difference which is well within the limit of error.

The number of cells in a given ganglion was estimated by counting the nuclei. The diameter of the nuclei in many cases being greater than the thickness of the section (10 μ), a considerable number of cells must have been counted twice. The percentage error being the same, however, in all ganglia, regardless of size, a constant factor is introduced which has no appreciable effect on the cell ratios.

Hyperplasia of the sensory centers was also demonstrated by an increase in the size of the posterior (sensory) roots. The latter which were connected with the transplanted limb on the right side were found to be considerably larger than those on the left side, not connected with a limb (fig. 6). The hypoplasia of the sensory centers resulting from extirpating the limb was likewise shown by a great reduction in the size of the sensory roots, normally connected with the limb (fig. 5).

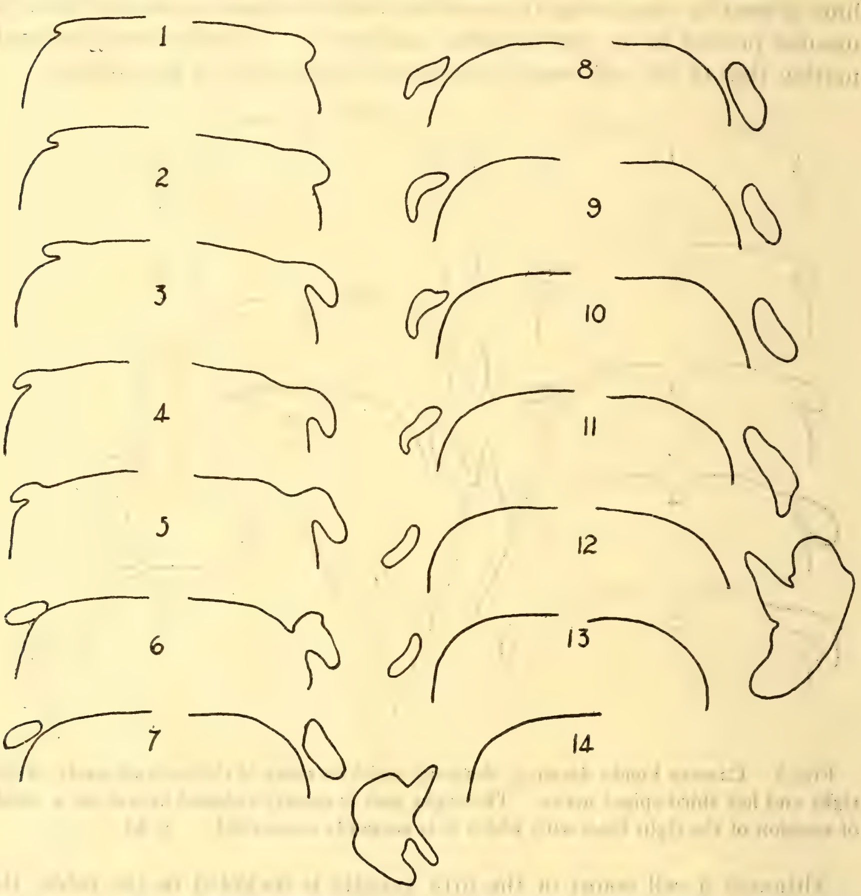

FIG. 6. Camera lucida drawing, showing serial sections of the sensory roots of the right and left seventh spinal nerve. The right root being connected with the transplanted limb is considerably larger than the left, which has no connection with a limb. × 40

The results reported in this paper, as well as confirmatory evidence obtained from the study of other cases not reported ($AS5_{25}$, $AS5_{26}$, $AS5_{27}$, $AS5_{30}$), clearly show that excessive peripheral loading with a subsequent increase in the functional demands will result in a corresponding hyperplasia of the sensory nerve centers. Results so far accumulated suggest the possibility that motor regulation is accomplished by division of the peripheral axones. It is hoped that further experimentation will disclose a number of additional facts concerning the effect of peripheral overloading on the development of nerve centers.

Showing the Effects of Removal of the Limb and the Functual Activity of the Transplanted Limb upon the Development of Peripheral Afferent Neurones

GANGLION CONNECTED WITH NORMAL INTACT LEFT LIMB			GANGLION WITH RIGHT LIMB REMOVED			GANGLION CONNECTED WITH RIGHT LIMB TRANSPLANTED TO A HETEROTOPIC POSITION			GANGLION NOT CONNECTED WITH EITHER LIMB			RATIOS	
No. of spinal ganglion	No. of cells counted	Weight of the model in grams	No. of spinal ganglion	No. of cells counted	Weight of the model in grams	No. of spinal ganglion	No. of cells counted	Weight of the model in grams	No. of spinal ganglion	No. of cells counted	Weight of the model in grams	Cell No.	Weight
3	1725	30.4841	3	850	12.9748							0.492	0.425
4	1430	26.1604	4	685	9.7311							0.479	0.372
5	955	16.8798				5	1195	22.2939				1.25	1.32
						6	1171	26.5422	6	693	12.6600	1.69	2.09
						7	1084	21.7634	7	720	11.7366	1.51	1.85
									9*	754	15.5456		
									9**	773	17.2328	1.02	1.10

* Left ganglion. ** Right ganglion.

---

[1] Braus, H., *Morph. Jahr.*, **35,** 1906.

[2] Shorey, M. L., *J. Exper. Zoöl.*, **17,** 1909.

[3] Dürken, B., *Zs. wiss. Zoöl.*, **99,** 1911.

[4] Burr, H. S., *J. Exper. Zoöl.*, **20,** 1916.

[5] Detwiler, S. R., these Proceedings, **5,** 1919.

[6] The innervation to the normal and the transplanted limb in Case $AS4_{26}$ is figured in the paper referred to above.

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CONTENTS

Page

Chemistry.—An Apparent High Pressure Due to Adsorption, the Heat of Adsorption, and the Density of Gas-Mask Charcoals .... *By William D. Harkins and D. T. Ewing* 49

Anthropology.—Central American Calendars and the Gregorian Day .... *By Herbert J. Spinden* 56

Physics.—The Torsional Magnetic Energy Absorption of an Iron Conductor .... *By Carl Barus* 59

Physiology.—The Adjustment to the Barometer of the Hematorespiratory Functions in Man .... *By Yandell Henderson* 62

Physics.—A Study of Absorption Spectra with the Electric Furnace ...... *By Arthur S. King* 63

Physics.—A Study of the Effect of a Magnetic Field on Electric Furnace Spectra .... *By Arthur S. King* 65

Genetics.—Selective Fertilization in Pollen Mixtures .... *By D. F. Jones* 66

Mathematics.—Groups Generated by Two Operators, s_1, s_2, Which Satisfy the Conditions $s_1^m = s_2^n$, $(s_1s_2)^k = I$, $s_1s_2 = s_2s_1$ .... *By G. A. Miller* 70

Genetics.—Model of the Linkage System of Eleven Second Chromosome-genes of Drosophila .... *By W. E. Castle* 73

Zoölogy.—The Development of Connective Tissue in the Amphibian Embryo .... *By George A. Baitsell* 77

Pathology.—Calcium and Magnesium Metabolism in Certain Diseases .... *By Frank P. Underhill, James A. Honeij and L. Jean Bogert* 79

Astronomy.—The Larger Opportunities for Research on the Relations of Solar and Terrestrial Radiation .... *By C. G. Abbot* 82

Zoölogy.—On the Hyperplasia of Nerve Centers Resulting from Excessive Peripheral Loading .... *By S. R. Detwiler* 96

VOLUME 6 MARCH, 1920 NUMBER 3

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ENCEPHALITIS AND POLIOMYELITIS

By Simon Flexner

Rockefeller Institute for Medical Research, New York

Read before the Academy, November 10, 1919

In 1916 there arose in Vienna and possibly in other places in the Austrian Empire, cases of a peculiar disease a prominent symptom of which is lethargy. Because of this feature, which amounts at times to a condition of profound and prolonged "sleepiness," the disease receives the popular name of sleeping sickness. And yet it was quickly recognized as being wholly distinct from the well-known African sleeping sickness, the inciting microörganism of which is a trypanosome.

The structural or histologic basis of the Austrian disease is an inflammation, chiefly located in the gray matter of the base of the brain, or technically an encephalitis. Because of the peculiar symptoms and of the lesions, von Economo of Vienna who first described the disease gave it the name of lethargic encephalitis.

About two years later, that is, in 1918, outbreaks of the same disease were reported from England and France. A little later the disease appeared in the United States, and published reports now indicate that it has a wide distribution over the world.

The total number of cases which have been observed and reported now run into the hundreds. Indeed, some writers prefer to call the disease "epidemic encephalitis." Up to the present time no great epidemic outbreak has been reported. However, the conditions with reference to the disease are more or less disquieting.

It is interesting to note that when the disease was first recognized in Vienna, it was supposed to be a form of food (sausage) poisoning; and the early cases in England were attributed to botulism. This mistake is quite explicable: among the prominent symptoms are paralyses of the ocular and facial muscles, which arise also in food poisoning. Because of war conditions, both the countries mentioned were using preserved foods to an unusual degree; hence the inference.

Because of the accompanying paralysis and subsequently because of certain peculiar characteristics of the histological changes in the brain, a discussion arose whether the disease was not merely a peculiar form of poliomyelitis or infantile paralysis, which has prevailed epidemically and fitfully in Europe and America for a decade or longer.

It is on this last point that I wish to make a few remarks. At the Rockefeller Institute we have had what may be regarded as a wide experience with poliomyelitis during ten or twelve years; and since the winter of 1919 and up to the present time we have had referred to us material from numbers of cases of the form of encephalitis we are considering.

On the basis of the studies we have carried out with the latter, it is quite safe to say that the diseases—lethargic or epidemic encephalitis and epidemic poliomyelitis—are distinct. A striking difference relates to the transmissibility of the one to lower animals (monkeys) and not of the other. No special difficulty is encountered in communicating poliomyelitis from man to monkey; we have not succeeded in transferring the encephalitis to animals. Moreover, the actual histological changes or lesions in the two diseases show striking differences, both of distribution in the central nervous organs and in minute structure. Lethargic encephalitis is surely an infectious disease, but its inciting microbe is still undiscovered.

The question arises whether encephalitis has been observed before, or is to be considered as being a new disease, in the sense that it had never before been recognized and described. In this connection it may be said that a brief account exists of an outbreak of a so-called "sleeping sickness" in the neighborhood of Tübingen in Germany in the year 1712; and what appears to be a similar malady more widely distributed in Austria, Italy, and Switzerland in 1890. There is a certain amount of mystery attached to the latter outbreak, partly because of the odd name "nona" it received, which has not been satisfactorily traced etymologically. It has been suggested that the term is a mere corruption of "coma," and partly because the chief accounts of it are given in the lay rather than the medical press of the time.

It is, however, of some significance that the first appearance of the present epidemic and the location of the 1890 outbreak coincide territorially. It is, of course, not absolutely possible to state that the disease originated this time in Vienna; under war conditions its occurrence in outlying rural districts might well have been overlooked. But there is nevertheless basis for the view that I shall propose tentatively, namely, that an endemic focus of a peculiar and specific form of encephalitis exists in Southeastern Europe, from which the present wide occurrence of the disease took origin. Should this view come to be established, we shall have learned of a new and previously unsuspected focus of an endemic-epidemic disease of world importance.

A STATISTICAL METHOD FOR STUDYING THE RADIATIONS FROM RADIOACTIVE SUBSTANCES AND THE X-RAYS AND ITS APPLICATION TO SOME γ-RAY PROBLEMS

By Alois F. Kovarik

Sloan Laboratory, Yale University

Communicated by H. A. Bumstead. Read before the Academy, November 10, 1919

Statistical methods of studying X-rays and the radiations from radioactive substances are important partly because they make it possible, to some extent at least, to investigate the individual rays. The scintillation method of counting α-particles for example brought decisive evidence of the atomic structure of matter. It also made it possible to obtain directly the charge on an α-particle. Furthermore, the experiments on the scattering of α-particles gave Rutherford some definite knowledge which led to some of the most fruitful suggestions about atomic structure.

The electrical method of counting α-particles as used by Rutherford and Geiger was adapted by Geiger,[1] after some changes, to the statistical study of both α-particles and β-particles. Kovarik and McKeehan[2] applied the method to the investigation of the absorption and scattering of β-particles getting direct information about the β-particles, whereas the previous information was all deduced from ionization experiments which to be accurate demands a complete knowledge of the variation of the speed and of ionization with speed of a β-particle passing through matter. By this method they also obtained evidence[3] supporting the single scattering theory of Rutherford and got results in agreement with those deduced from Wilson's photographs of the tracks of β-particles produced by the condensation method—another important statistical method. They also investigated the magnetic spectra[4] of the β-particles from radium B and C and from radium D and E. In their first report they called attention to the fact that the γ-ray effects were also counted and that correction had to be made for these.

A more recent development[5] in the method makes it possible for each ray, upon entering the counting chamber, to cause an audible sound or to make an automatic record on paper. The counting chamber generally surrounds a specially prepared point. The chamber and the point are at a high difference of potential—near the sparking potential. When any of the rays enter the counting chamber, ionization is produced and on account of the intense field between the point and the chamber, ionization by collision results. This magnifies the effect initially produced by the ray and this is further magnified by an audion bulb operating a sensitive relay which operates a local circuit by means of which a chronograph pen makes a record of the fact that an α-particle, a β-particle, a γ-ray pulse or an X-ray pulse has entered the counting chamber.

Using this method it is possible to investigate some important problems pertaining to the nature of γ-rays and X-rays, and also questions bearing on energy absorption and emission by an atom. For example it is important to know whether a spherical wave passing over matter will cause a simultaneous ejection of electrons at different parts of the same wave front. To test this two independent counting chambers were placed at equal distances from the source of γ-rays but at various different relative positions. One chamber registered the γ-ray effect audibly and the other optically. It was found that in no case were the effects produced simultaneously. When the chambers were placed one behind the other the effects were also non-simultaneous.

By varying the distance between one chamber and the source a confirmation of the inverse square law was obtained. Using a chamber of some definite material with thin walls and increasing this small thickness of the walls the registered number of effects increased. When material of higher atomic weight was used the effect also increased.

The first experiments mentioned can best be explained by assuming some corpuscular or ether-string theory and applying the law of probability to the distribution of the radiations, but in view of the facts coming from crystal experiments it seems necessary to consider some spreading pulse theory. If we assume the pulses to spread out as spherical surfaces we must consider absorption of energy by an atom and also a possible trigger action—an action not in great favor in recent years. The fact that the counting chambers did not record the γ-ray effects simultaneously means that the electron or electrons ejected were not ejected simultaneously from the metal or air of the chamber; but since the electrons ejected have, presumably, equal velocities and, therefore, equal energies, the experimental result indicates that the individual electrons do not acquire the necessary quanta of energy at the same time. This suggests a possible storing up of energy. There are, however, reasons to believe that the energy stored up cannot be equal to the energy represented by the ejected electrons, and it would follow that some if not most of the energy of the ejected electrons must come from the interior of the atom and that the γ-ray energy absorbed is only sufficient to perform the trigger action. Furthermore, it seems very probable that the particular electron to be ejected may have previously acquired, in some way, energy nearly sufficient to throw it out of balance which the γ-ray energy may suffice in doing; and it also seems probable that the more penetrating the γ-rays, i. e., the higher the value of ν, the deeper is the electron which will be ejected by the γ-ray.

During the progress of these experiments, which were to include also the determination of the number of γ-ray pulses per second, per gram of radium, I received reprints of work done on this problem by R. W. Lawson and V. F. Hess[6] at the Radium Institute in Vienna (where Mr. Lawson, although a civilian prisoner during the war, was permitted to continue his work). Their counting chamber was a Rutherford and Geiger's spherical chamber with gas at reduced pressure. They obtained 2.92×10^{10} γ-rays from radium B and C per second per gram of radium. The material of their counting chamber was copper. In view of my results with chambers of different materials I decided to continue my experiments on this point especially as my results indicated that Lawson and Hess's value is probably low. My results at present indicate a value of 7×10^{10} γ-rays from radium B and C per second per gram of radium.

[1] Geiger, H., *Verh. D. Phys. Ges.*, **15,** 1913 (534).

[2] Kovarik, A. F., and McKeehan, L. W., *Physik. Z.*, **15,** 1914 (434).

[3] Kovarik, A. F., and McKeehan, L. W., *Physic. Rev.*, N. S., **6,** 1915 (426).

[4] Kovarik, A. F., and McKeehan, L. W., *Ibid.*, **8,** 1916 (574).

[5] Kovarik, A. F., and McKeehan, L. W., *Ibid.*, **13,** 1919 (272).

[6] Hess, V. F., and Lawson, R. W., *Sitzb. Akad. Wien*, **125,** 1916 (585).

ON THE NATURE OF THE HEAT PRODUCTION IN A SYSTEM OF PLATINUM BLACK, ALCOHOL, AND AIR

BY L. B. LOEB

NATIONAL RESEARCH FELLOW, RYERSON PHYSICAL LABORATORY, UNIVERSITY OF CHICAGO

Communicated by R. A. Millikan, January 16, 1920

When a small quantity of platinum black is introduced into an atmosphere of air saturated with ethyl or methyl alcohol vapor a violent reaction takes place on the surface of the platinum causing it to become incandescent.

The nature of this reaction has been explained in two ways. It is held by some that owing to the vast surface and poor heat conductivity of platinum black the adsorption of the alcohol vapor liberates sufficient heat to raise the temperature of the platinum considerably. This rise in temperature is supposed to accelerate the rate of oxidation of the alcohol vapor in the neighborhood of the platinum thus further raising its temperature until the substance becomes incandescent.*

On the other hand it is held by others that the heat produced at the surface of the platinum black is due primarily to the oxidation of the alcohol at the surface of the platinum. The adsorption of the alcohol is in this case considered as contributing relatively little heat. The surface of the platinum is, however, supposed to exert a "catalytic" action on the alcohol vapor and oxygen increasing the speed of reaction to many times that in air at ordinary temperatures.

The writer's attention was called to the two explanations by Prof. Millikan and it occurred to the writer that a decision between the two explanations might easily be obtained experimentally. If a thermocouple be covered with platinum black it is an easy matter to determine the relative rise in temperature of the platinum black due to the adsorption of alcohol vapor in the presence and in the absence of air. Such experiments were performed by the writer. As the results were so decisive and as no reference to similar experiments could be found in the literature, a brief statement of the observations may be of interest.

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* This explanation is ascribed to Wilhelm Ostwald by Prof. P. Sabatier, in a course of lectures given students of the A. E. F., at Toulouse, France, in 1918.

About 0.41 gram of platinum black, prepared from $PtCl_4$ by precipitation with aluminium from an acid solution, was placed in a small beaker in a vacuum desiccator. The base of the desiccator contained a small quantity of H_2SO_4 which served to dry the platinum black. The beaker was surrounded as completely as possible with cotton wool to cut down convection currents. The junction of a "Chromel-Alumel" thermocouple of No. 18 wire welded by copper was buried in the heap of platinum black. The leads passed through a rubber stopper to the circuit of a galvanometer which was closed through a key.

The sensibility of the galvanometer used had several values lying between a deflection of 1 cm. scale reading for 4.9° C. change in temperature to 1 cm. scale reading for 0.36° C. change in temperature. Greater sensibility could not be used as the temperature changes due to compression and exhaustion of the air in the experiments were in the neighborhood of one degree. By means of a two-way stopcock the desiccator could alternately be exhausted by an air pump or filled with a mixture of CO_2 and CH_3OH, a mixture of air and CH_3OH, or by CO_2 alone. The gases were saturated with CH_3OH by bubbling through a small quantity of the liquid in the bottom of a bottle.

The following is the record of a typical experiment. After having been exposed to air and alcohol, the chamber was exhausted to about 2 cm. Hg pressure and washed out with CO_2 gas at least three times. The galvanometer deflection after each filling registered about 4 cm. corresponding to a rise of about 1.44° C. This was due to the heat of compression of the CO_2 on filling, as the pressure of the CO_2 in the generator was above that of the air. Exhaustion of the gas in the desiccator also caused a drop of temperature of about 0.4° C.

Alcohol vapor plus CO_2 was then admitted and the deflection amounted to 5.5 cm. or to about 1.98°. The rise due to adsorption of the alcohol, or due to *adsorption* and *oxidation* of adsorbed alcohol by the remnants of the adsorbed O_2 left on the platinum, therefore, amounted to about 0.54° C. The mean of a number of results gave the value of the rise produced due to adsorption as about 0.36° C.

If the gas admitted after exhaustion was a mixture of air and CH_3OH vapor, instead of CO_2 and CH_3OH, the resulting deflection was on the average 24 cm. at the end of about a minute. The sensibility of the galvanometer was less than before so that this corresponded to a rise of 34° C. Different sensibilities gave about the same value for the rise in temperature. In this case the deflection was not instantaneous but the temperature rose more gradually, reaching a maximum in about 45 seconds. After this it slowly fell as the alcohol and air were consumed in the immediate neighborhood of the platinum.

When air alone was admitted to the apparatus no appreciable rise in temperature was detected.

It is obvious that the heat liberated on the surface of platinum black due to adsorption alone either of air or of alcohol vapor is small compared to that liberated in the process of oxidation of the alcohol at the surface of the platinum. In fact, adsorption gives at most only 1% of the heat produced in these experiments.

It will be of interest to know whether this conclusion is also supported by other facts.

On the basis of a recent theory of Langmuir[1] that the layer of molecules of a gas *adsorbed* on a solid surface is never more than one molecule thick, it is possible with certain assumptions to calculate the rise in temperature of the platinum black. If one considers that the heat of adsorption is equal to the heat of condensation, if one neglects heat losses due to radiation, conduction, and convection, and if one assumes a definite radius for the particles of the platinum black the calculation is quite simple.

Let us assume that the platinum black is made up of aggregates of spheres of platinum whose diameter is 100 $\mu\,\mu$, or 1×10^{-4} cm. This is about the size of the colloidal gold particles of a gold suspension investigated by Zsigmondy.[2] Such an assumption is justifiable as the particles of platinum black used are certainly much larger than this.

Using 0.41 gram of platinum black we get

The number particles of diameter 1×10^{-4} cm. in 0.41 gram platinum black $= 3.75 \times 10^{10}$.

The total surface of all the particles $= 1.18 \times 10^{3}$ sq. cm.

On Langmuir's theory there should be $= 0.85 \times 10^{15}$ molecules of CH_3OH per square cm. surface in an adsorbed layer one molecule thick.

Total number of molecules of CH_3OH condensed on the surface of the platinum black $= 1 \times 10^{18}$ molecules.

Heat generated by these molecules on condensation equals heat of evaporation of the 10^{18} molecules $= 1.53 \times 10^{-2}$ calories.

The 0.41 gram of platinum will be raised 1.16° C. at 0° C. by this adsorption.

If the amount adsorbed were oxidized the temperature would be raised 19.8° C.

It is obvious that the relative values calculated on these assumptions are in approximate agreement with the values experimentally obtained. The surface of the platinum black is in reality much smaller than was assumed here. Furthermore the heat liberated by oxidation is continuously liberated as long as alcohol and oxygen are present, while in the case of adsorption the heat is all liberated in the short span of time during which the adsorbed layer is forming. It is therefore obvious that in spite of the fact that the heat of adsorption may be greater than that of condensation the rise in temperature observed would be mainly due to the oxidation of the alcohol as was found by experiment to be the case.

In view of what has preceded it is to be concluded that the rise in temperature of the platinum black due to adsorption of the alcohol vapor is not the initiator of the reactions which heat the platinum to incandescence. For the heat generated by adsorption is negligible compared to that produced by oxidation and would not be capable of increasing the speed of reaction appreciably.

It is then on the basis of an increased rate of oxidation of the alcoho at the surface of the platinum ("catalytic" action) coupled with the physical properties of the platinum black that one must explain the rise in temperature. In this, one is justified to some extent, for it has long been known that surfaces of platinum accelerate certain chemical reactions, both in gases and in solution. Recently, Langmuir[1] has shown that at ordinary temperatures and at reduced pressures platinum foil when "activated" will cause oxygen and carbon monoxide to unite. In this case there seems to be some close relation between this action of platinum and adsorption, but it is not due to any heating effect.

[1] Langmuir, I., *J. Amer. Chem. Soc., Easton, Pa.*, **40**, 1918 (1361).

[2] Zsigmondy, R., *Zur Erkenntnis der Kolloide*, Jena, 1905 (104).

LEPTOSPIRA ICTEROIDES AND YELLOW FEVER

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Communicated by S. Flexner. Read before the Academy, November 10, 1919

Although yellow fever is being rapidly controlled by measures directed against the intermediate host (the Stegomyia mosquito), which acts as the vector, yet the nature of its inciting microbe has remained unknown. Hence yellow fever is a striking instance of the fact that given precise knowledge of the mode of infection of a microbic disease effective practical measures may be evolved for its control, even though the inciting microbe remain undiscovered.

Recently a fresh opportunity was afforded for the investigation of the etiology of yellow fever. The International Health Board of the Rockefeller Foundation sent a commission to Guayaquil, Ecuador, in June, 1918, to survey the field preparatory to the application of measures of control. Guayaquil has long been a recognized endemic focus of yellow fever, from which periodic extension has taken place to Central America.

I was attached to this commission as bacteriologist, and my attention was particularly directed, although not wholly confined to, the possibility that the inciting microbe belonged to the group of spiral microorganisms which in the last ten or twelve years have come to be regarded as playing a very important part in human pathology. There was already known, indeed, a particular spiral organism, *Leptospira icterohemorrhagiae*, which produces a severe and sometimes fatal jaundice in man, and which has a wide distribution in nature—far wider, indeed, than has yellow fever. This spiral microbe inhabits the rat, from which animal it finds its way at times to human beings, who then develop the symptoms and organic lesions of the disease called infectious jaundice.

I have given much attention to the spiral group of microörganisms for about ten years. In this period I have been enabled to devise new methods of cultivation, which have proven successful for species not before cultivated in successive generations, and which have also led to the discovery of new species. Hence in going to Guayaquil I took with me a laboratory outfit complete for this line of study.

Fortunately, I was successful in detecting in certain cases of yellow fever by culture methods and by guinea pig inoculation a particular spiral organism which I have since named *Leptospira icteroides*. The guinea pigs (and, as later studies showed, also puppies) successfully inoculated with the blood of yellow fever patients or with the cultures develop symptoms and lesions closely approximating those occurring in man and giving the clinical picture of the disease as usually observed. The outstanding signs are jaundice, hemorrhage into the lungs and stomach (this latter in man leads to the black vomit, so called), and albumin and casts in the urine. At autopsy, in the guinea pig as in man, the liver, kidneys, and other internal organs prove to be severely degenerated. The spiral organisms are, of course, recoverable from the inoculated guinea pigs, and with these organisms the disease is transmissible through an indefinite series of animals.

Moreover, guinea pigs have been successfully infected with the spiral organisms by means of Stegomyia mosquitoes, the vector in nature of the inciting microbe from man to man, and Stegomyias fed on infected guinea pigs are capable of transmitting the active microbe to still other guinea pigs, which develop the symptoms and lesions described.

Finally, immunological studies have brought out important points of relationship (for example, to the Leptospira of infectious jaundice) and indicated the possibility of developing a "vaccine," and even a curative serum. But until the finding of *Leptospira icteroides* is confirmed by the investigation of cases of yellow fever in still other places, its standing as the inciting agent of yellow fever will have to be regarded as not yet certainly established.

HUMAN RETINAL ADAPTATION

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Communicated by J. Loeb, January 2, 1920

Although the magnitude and the course of the dark adaptation of the human eye are well known, no fundamental interpretation has been made of the results. The data are simple. In the dark the eye becomes increasingly sensitive to light as the time goes on, until after about an hour it reaches a maximum of irritability. The measure of this changing sensitivity is given in terms of the least intensity which is just perceptible to the eye. The data which have been accumulated in this way are plentiful and accurate.[1]

The lack of theoretical significance of these data is caused by two conditions. The first is that the term sensitivity, even as measured in units of intensity, has no ultimate material basis. The fundamental question is: what objective changes in the makeup of the retina determine the variations in its sensitivity? This cannot at present be answered from experiments on the eye. However, even if this situation were satisfactorily elucidated, a second limitation bars the way to a final interpretation. This second condition hinges on the photochemical effect of the light. Since the data of retinal dark adaptation are given in terms of intensity, it is necessary to have some quantitative estimate of the photolytic action of light at the various intensities used for stimulation. This condition again cannot be fulfilled from the reported experiments with the eye. Indeed, in the present state of our knowledge it is difficult to devise experiments for the purpose of investigating these two conditions.

Fortunately the physicochemical analysis of the photoreception of lower forms has reached the point where these questions may be answered with considerable accuracy and a fair degree of confidence.[2] With regard to the first limitation of retinal interpretation, the experiments with *Mya* and *Ciona* are unequivocal. A photosensitive substance S is decomposed by the light into its two precursors P and A. This reaction is reversible, the regeneration of photosensitive material S from its two precursors P and A following the course of a bimolecular reaction. Thus the equation of the reaction is

$$S \underset{\text{“dark”}}{\overset{\text{light}}{\rightleftharpoons}} P + A.$$

The important point in this mechanism is that the sensitivity of the sense organ at any moment is dependent on the concentration of precursor substances P and A. Quantitatively expressed this means that the amount of fresh precursors that must be formed by the light before a response is elicited is always a constant fraction of the amount of precursors already present in the sense organ. Conversely, this amount of fresh precursors is a direct measure of the concentration of residual precursors. If this concentration is high, much fresh precursors are required, and much light energy must be used to produce the minimum stimulating effect. If the concentration of residual precursors is low, the reverse will be true. In short, the sensitivity is thus defined in terms depending on the physical constitution of the sense organ.

Similarly with the second condition which inhibits the interpretation of retinal adaptation. Experiments to be published in the *Journal of General Physiology* show a simple relation between the intensity of the stimulating light and its photochemical effect in the photosensory process. If E is the photolytic effect as measured in units of P and A formed, then the results are that

$$E = lnI$$

where I is the intensity of the stimulating light. The photochemical effect of the light is, therefore, a logarithmic function of its intensity. Here again the meaning of the minimum intensity is given in terms of the ultimate composition of the sensory mechanism.

If the initial phase of the photic reception of the human retina in dim light is similar to the mechanism in these lower forms, the application of the two principles just described should yield an analysis of the results obtained in dark adaptation. The sensitivity of the eye given in terms of the minimum intensity of the stimulating light can now be replaced by a direct statement of the photochemical effect of this light. Instead of the intensity we may use the logarithm of the intensity to indicate the extent of this photolytic activity. This now shows that during dark adaptation of the eye less and less photochemical decomposition is required to produce a visual effect. Assuming a photosensitive substance in the retina, this means that as the time goes on less and less of this photosensitive material must be decomposed in order to produce a stimulus. The exact amount of this decomposition can now be calculated.

If in addition it is true for the eye that the amount of freshly formed decomposition product is always a constant fraction of the residual decomposition product present in the retina, the significance of these facts becomes clear. During dark adaptation less and less fresh decomposition product must be formed. Therefore, the quantity of residual decomposition product present in the retina must also become less and less, and at the same rate as that of the fresh decomposition product. The ratio between the two being constant, the curve of dark adaptation should represent the rate of disappearance of the residual decomposition product.

The many experiments reported on the retinal adaptation of the eye, when analyzed in this way, show a uniformly consistent course for the disappearance of the decomposition product. The curve which represents all of the published data is the isotherm of a bimolecular reaction

$$k = \frac{1}{at} \cdot \frac{x}{a - x}$$

in which k has an average value of approximately 0.016. Because of the refined technic used in the experiments the agreement between the individual observations and the theoretical curve is frequently diagrammatic in its precision. An example is given in figure 1. The observations are taken from an experiment of Nagel's made more than ten years ago.

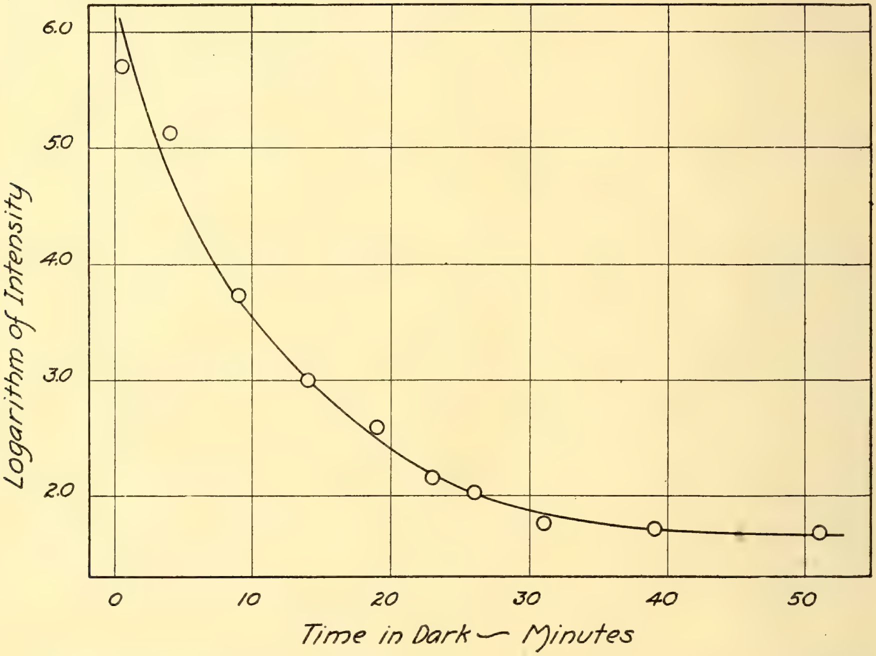

FIG. 1. Dark adaptation of the human eye. The points are single observations, whereas the curve is the isotherm of a bimolecular reaction. Actually the ordinates are the logarithms of the minimum intensities at different periods in the dark; theoretically they represent the number of units of decomposition products still present in the retina.

The meaning of the representation of the course of retinal dark adaptation by the curve of a bimolecular reaction is obvious. *Two* substances are decreasing in concentration according to the ordinary velocities of chemical reactions. These two substances are, therefore, combining to form a third substance.

The simplest interpretation of these facts is that the substance synthesized during dark adaptation is the photosensitive substance, and that the two materials which form this photosensitive substance are its precursors as well as its decomposition products. The initial process in visual reception in dim light must, therefore, depend on a reversible photochemical reaction in which a photosensitive substance is broken down into *two* decomposition products. During dark adaptation of the retina these two decomposition products then unite to form again the sensitive substance from which they were produced. The actual course of dark adaptation depends on the change in concentration of these two reacting substances.

In all essentials, then, the mechanism underlying the initial phase of retinal sensitivity in dim light is the same as that which forms the basis of the initial process of photoreception in such forms as *Mya* and *Ciona.*

[1] W. Nagel, in Helmholtz' *Handbuch der physiologischen Optik,* **2,** 1911.

[2] S. Hecht, *Journal of General Physiology,* **1** and **2,** 1918–20.

A KINEMATICAL INTERPRETATION OF ELECTROMAGNETISM

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Communicated by H. A. Bumstead. Read before the Academy, November 10, 1919

The concept of lines of force was introduced by Faraday as an aid in mapping out magnetic fields—these lines having everywhere the direction in which a small north pole would be urged by the field. Later it occurred to him that the characteristics of an electric field might be represented in similar manner by curves everywhere tangent to the electric intensity. Faraday limited his use of lines of force to static fields, and in cases where the field was produced by a number of charged particles, to the resultant field.

The electromagnetic theory of light, however, made it necessary to suppose that electric and magnetic lines of force in a wave front move with the velocity of light in a direction at right angles to their plane. Furthermore, the discovery of the electron made it natural to assume that when one of these small particles is in motion, its field is carried along with it. Wiechert and Stokes have explained X-rays as kinks in the lines of force emanating from such a particle—these kinks being propagated outward with the velocity of light whenever there is a change in the electron's state of motion.

The object of this paper is to show that the laws of electromagnetism may be explained exactly and in their entirety as kinematical relations between the moving elements which constitute lines of force, provided use is made of the space and time transformations of the principle of relativity in place of the more familiar but less philosophical Galilean transformations.[1] Each charged particle must be considered to have its own field, extending out to infinity in all directions, the resultant field at any point being a combination of those fields which extend to the point in question.

The electrodynamic equations are five in number: four of these specify the electric and magnetic fields in terms of the distribution of charge and current throughout space, and the fifth specifies the force on charged matter due to the strength of the electric and magnetic fields in which it is moving. First will be taken up the kinematical interpretation of the two equations —Coulomb's law and Ampere's law—which do not involve the space time transformations, and hence are not dependent on the principle of relativity.

In the representation to be employed, each charged particle e is to be considered as a source of moving elements projected outward uniformly in all directions with the velocity of light c. A line of force is a curve drawn through the moving elements which have been projected in a given direction. Perhaps a better picture of the method by which the field is produced may be obtained by imagining a very large number of guns to be stationed at e, pointing outward in all directions. Suppose each gun to be provided with a long, perfectly elastic cord on which shot are strung. Let these shot be fired from the gun at regular but very short intervals with the velocity of light. Then the shot constitute the moving elements emanating from the charge, and the elastic cord connecting them marks the line of force diverging in the particular direction under consideration.

The number of lines of force diverging from a charge de will be supposed to be very large, no matter how small de may be. For convenience in making numerical calculations, these lines will be grouped into bundles or tubes of M lines each, where M is a large number so chosen as to make the number of tubes N diverging from any charge equal numerically to the magnitude of the charge. This convention is tantamount to measuring electric charges in Heaviside-Lorentz rational units.

If the electric intensity $\mathbf{E}$ is defined as the number dN of tubes per unit cross section, the component of the intensity normal to the small surface $d\sigma$ is

$$\mathbf{E}_{d\sigma} \equiv \frac{dN}{d\sigma^2}\, d\sigma.$$

According to Gauss' theorem, the average divergence of $\mathbf{E}$ over a small volume $d\tau$ surrounded by the closed surface σ is given by

$$\begin{aligned} \nabla\cdot\mathbf{E}d\tau &= \int_\sigma \mathbf{E}\cdot d\sigma \\ &= \int_\sigma dN. \end{aligned}$$

Now the part of this sum due to charges outside the region $d\tau$ vanishes, while the part due to the charge de inside this region is equal to de itself. Hence, if ρ is the density of charge at the point in question,

$$\nabla\cdot\mathbf{E} = \rho. \qquad (1)$$

This is Coulomb's law.

To obtain Ampere's law, consider the increase in electric intensity at *O* (fig. 1) taking place in the time *dt*. During this time the lines of force at *P* will have moved up to *O*. If these lines are closer together at *P* than at *O*, **E** will change by an amount

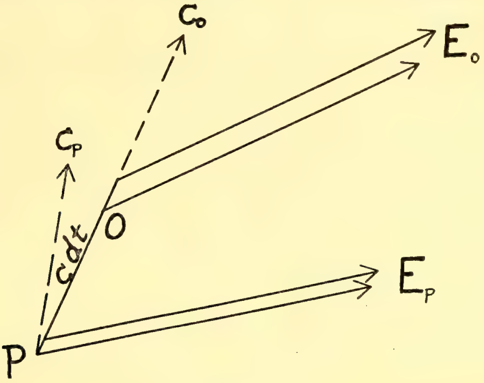

FIG 1

$$-\mathbf{c}\cdot\nabla\mathbf{E}dt.$$

Furthermore **E** will suffer a change if the velocity $\mathbf{c}_P$ of the moving elements at *P* has a different direction from that at *O*, or if these lines twist as they move upward. The increase in **E** due to these two causes is

$$(-\mathbf{E}\nabla\cdot\mathbf{c} + \mathbf{E}\cdot\nabla\mathbf{c})dt.$$

Therefore, in all,

$$\begin{aligned}\dot{\mathbf{E}} &= -\mathbf{c}\cdot\nabla\mathbf{E} - \mathbf{E}\nabla\cdot\mathbf{c} + \mathbf{E}\cdot\nabla\mathbf{c} \\ &= -\mathbf{c}\nabla\cdot\mathbf{E} + \nabla\times(\mathbf{c}\times\mathbf{E}).\end{aligned}$$

So far, no account has been taken of the new lines of force produced by the emission of moving elements in the neighborhood of a charged particle.

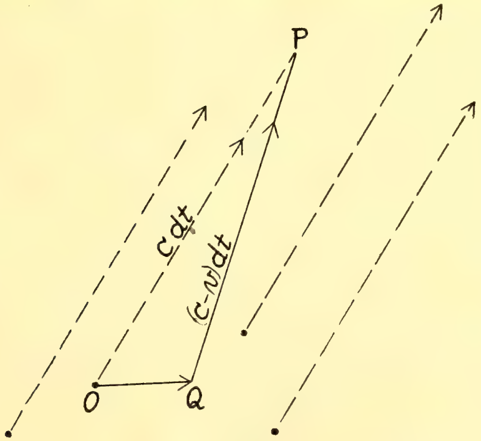

FIG. 2

Let the dots in figure 2 represent sources emitting lines of force in the direction of the broken lines, and let these sources have a velocity **v** to the right. Then the rate of change of the electric intensity at P due to the formation of new lines is

$$(\mathbf{c} - \mathbf{v})\nabla\cdot\mathbf{E}.$$

Adding this to the expression for $\dot{\mathbf{E}}$ obtained above, and using Coulomb's law to eliminate $\nabla\cdot\mathbf{E}$, it is seen that

$$\nabla \times (\mathbf{c} \times \mathbf{E}) = \dot{\mathbf{E}} + \rho\mathbf{v}.$$

The magnetic intensity $\mathbf{H}$ is defined by

$$\mathbf{H} \equiv \frac{1}{c}\mathbf{c} \times \mathbf{E}.$$

Therefore

$$\nabla \times \mathbf{H} = \frac{1}{c}(\dot{\mathbf{E}} + \rho\mathbf{v}). \qquad (2)$$

This is Ampere's law for the field due to a single charged particle. As it is linear in $\mathbf{E}$ and $\mathbf{H}$, the same law holds for the resultant of the simple fields due to any number of charged particles.

Next consider a charged particle e (fig. 3) at rest relative to the observer

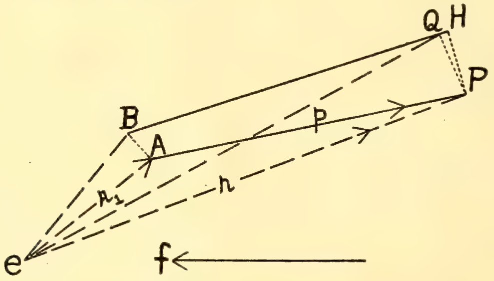

FIG. 3

but having an acceleration f to the left. Let eP be the path of a moving element emitted from the charge at the time zero, and eA that of a moving element belonging to the same line of force, but emitted at the time dt. As the velocity of the charge has changed in this time, these paths will make different angles with the acceleration. The line of force at P at time r/c will have the direction of AP, or $\mathbf{p}$. Evidently

$$\begin{aligned} \mathbf{r} &= \mathbf{c}t, \\ \mathbf{r}_1 &= \mathbf{c}t - \mathbf{c}dt + td\mathbf{c}, \\ \mathbf{p} &= \mathbf{c}dt - \frac{r}{c}d\mathbf{c}, \end{aligned}$$

and the electric intensity at P at the time r/c is given by

$$\begin{aligned} \mathbf{E} &= \frac{e}{4\pi r^2}\frac{\mathbf{p}}{p\cos QPH} \\ &= \frac{e}{4\pi r^2 c}\left\{\mathbf{c} - \frac{r}{c}\frac{d\mathbf{c}}{dt}\right\}. \end{aligned}$$

A short calculation gives

$$\frac{d\mathbf{c}}{dt} = -\frac{1}{c^2}(\mathbf{f} \times \mathbf{c}) \times \mathbf{c}.$$

Therefore,

$$\mathbf{E} = \frac{e}{4\pi r^2 c}\left\{\mathbf{c} + \frac{r}{c^3}(\mathbf{f} \times \mathbf{c}) \times \mathbf{c}\right\}. \tag{3}$$

So far no use has been made of the relativity transformations. In order to get the electric intensity due to a moving charge, recourse must be had to these transformations, resulting in the following expression,

$$\mathbf{E} = \frac{e}{4\pi r^2 k^2 c\left(1 - \frac{\mathbf{c}\cdot\mathbf{v}}{c^2}\right)^3}\left\{\mathbf{c} - \mathbf{v} + \frac{rk^2}{c^3}\{\mathbf{f} \times (\mathbf{c} - \mathbf{v})\} \times \mathbf{c}\right\}, \tag{4}$$

where

$$k \equiv \frac{1}{\sqrt{1 - \frac{v^2}{c^2}}}.$$

From the definition of magnetic intensity, it follows that

$$\mathbf{H} = \frac{e}{4\pi r^2 k^2 c\left(1 - \frac{\mathbf{c}\cdot\mathbf{v}}{c^2}\right)^3}\left\{-\frac{\mathbf{c} \times \mathbf{v}}{c} + \frac{rk^2}{c^4}\mathbf{c} \times (\{\mathbf{f} \times (\mathbf{c} - \mathbf{v})\} \times \mathbf{c})\right\}. \tag{5}$$

For fields specified by these expressions, it is easily shown that

$$\nabla \mathbf{H} = 0, \tag{6}$$

and

$$\nabla \times \mathbf{E} = -\frac{1}{c}\dot{\mathbf{H}}. \tag{7}$$

These are the two remaining field equations, the second being the mathematical expression of Faraday's law of current induction for the case where a current is induced by varying the magnetic flux through a stationary circuit. As these laws are linear in **E** and **H**, they hold as well for the resultant of the simple fields due to a number of charged particles as for each of these fields individually.

The last of the electromagnetic equations gives the force on a charge moving through electric and magnetic fields. Let unprimed letters in figure 4 refer to quantities as measured in a so-called stationary system, and primed letters to the same quantities as measured in a system moving to the right with velocity **v**. On account of the dependence of time on position required by the relativity transformations, the line of force *PE* in the stationary system has the different direction *PE'* in the moving system. If the dynamical equation for a charge e situated in the moving system is assumed to be

$$e\mathbf{E}' = \frac{d}{dt'}(m\mathbf{v}'),$$

as measured in the units of this system, the relativity transformations give

$$e\left(\mathbf{E} + \frac{1}{c}\,\mathbf{v}\times\mathbf{H}\right) = \frac{d}{dt}\left(\frac{m}{\sqrt{1-\frac{v^2}{c^2}}}\,\mathbf{v}\right) \tag{8}$$

in the stationary system. This is the fifth and last of the electromagnetic equations. If the relativity transformations had not been made use of in deriving it, the term involving the magnetic intensity would have been absent. But this term is the one that accounts for the current induced in a wire moving through a steady magnetic field, and for the torque in a coil carrying a current which is placed in such a field. Therefore, the action of every generator and every motor used by industry in this age of electricity is incontrovertible evidence of the truth of the principle of relativity.

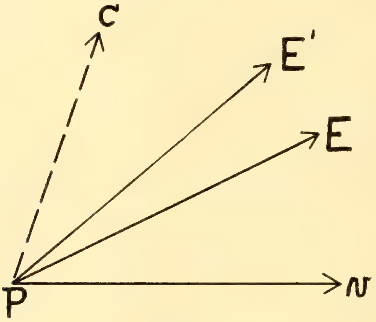

FIG. 4

In deriving expression (3) for the electric intensity due to a point charge, it has been tacitly assumed that the field does not rotate. If the sources of moving elements constituting the charge—the guns in the analogy previously used—are supposed to rotate with angular velocity ω, the rate of change of $\mathbf{c}$ at the time r/c becomes

$$\frac{d\mathbf{c}}{dt} = -\frac{1}{c^2}\left[(\mathbf{f}\times\mathbf{c})\times\mathbf{c} + \left\{(\omega\times\mathbf{c})\times\mathbf{c}\right\}\times\mathbf{c}\right],$$

for a charge which is at rest relative to the observer at the time zero. This introduces into the expression (3) for $\mathbf{E}$ the additional term

$$\frac{e}{4\pi r^2 c}\left\{\frac{r}{c^3}\left\{(\omega\times\mathbf{c})\times\mathbf{c}\right\}\times\mathbf{c}\right\},$$

and $\mathbf{E}$ and $\mathbf{H}$ for a moving charge become

$$\mathbf{E} = \frac{e}{4\pi r^2 k^2 c\left(1 - \dfrac{\mathbf{c}\cdot\mathbf{v}}{c^2}\right)^3}\left\{\mathbf{c} - \mathbf{v} + \frac{rk^2}{c^3}\,\{\mathbf{f}\times(\mathbf{c}-\mathbf{v})\}\times\mathbf{c} + \frac{r}{c^3}\,(\{\boldsymbol{\omega}\times(\mathbf{c}-\mathbf{v})\}\times\mathbf{c})\times\mathbf{c}\right\}, \quad (9)$$

$$\mathbf{H} = \frac{e}{4\pi r^2 k^2 c\left(1 - \dfrac{\mathbf{c}\cdot\mathbf{v}}{c^2}\right)^3}\left\{-\frac{\mathbf{c}\times\mathbf{v}}{c} + \frac{rk^2}{c^4}\,\mathbf{c}\times(\{\mathbf{f}\times(\mathbf{c}-\mathbf{v})\}\times\mathbf{c}) + \frac{r}{c^2}\,\{\boldsymbol{\omega}\times(\mathbf{c}-\mathbf{v})\}\times\mathbf{c}\right\}. \quad (10)$$

The field equations (1) and (2) representing Coulomb's law and Ampere's law are unchanged for this more general type of field, but (5) and (6) become

$$\nabla\cdot\mathbf{H} = \epsilon, \quad (11)$$

$$\nabla\times\mathbf{E} = -\frac{1}{c}\,(\dot{\mathbf{H}} + \epsilon\mathbf{u}), \quad (12)$$

where the density of magnetic charge ϵ is

$$\epsilon \equiv \frac{e}{2\pi r^2 k^2 c^2}\,\frac{\dfrac{\boldsymbol{\omega}\cdot\mathbf{c}}{k^2} - \boldsymbol{\omega}\cdot\mathbf{v}\left(1 - \dfrac{\mathbf{c}\cdot\mathbf{v}}{c^2}\right)}{\left(1 - \dfrac{\mathbf{c}\cdot\mathbf{v}}{c^2}\right)^4}, \quad (13)$$

and its velocity u is in general equal to the velocity c of light. The energy equation for fields of this character takes the form

$$\frac{d}{dt}\left\{\frac{1}{2}\,(E^2 + H^2)\right\} + c\nabla\cdot(\mathbf{E}\times\mathbf{H}) + \mathbf{E}\cdot\mathbf{v}\rho + \mathbf{H}\cdot\mathbf{u}\epsilon = 0, \quad (14)$$

and the energy radiated by a charge e due to rotation of the field is

$$R = \frac{e^2\omega}{3c}$$

for each revolution. In terms of the frequency ν this expression has the form

$$R = \frac{2\pi e^2}{3c}\,\nu.$$

If the value of the electronic charge is substituted for e, to wit

$$e = 4.77\,(10)^{-10}\sqrt{4\pi}$$

Heaviside-Lorentz units, it is found that

$$R = \frac{1}{32}\,6.4(10)^{-27}\,\nu \text{ erg sec.}$$

$$= \frac{1}{32}\,h\nu,$$

where h is Planck's constant. This numerical coincidence suggests that quantum phenomena may be in some way connected with rotating electric fields, and that

$$h \equiv \frac{64\pi e^2}{3c}.$$

Summary.—The equations of electrodynamics are shown to be simple kinematical relations between the moving elements which constitute lines of force. If the sources of these moving elements are supposed to rotate, free magnetic charges are produced, and the four equations of the electromagnetic field assume a symmetrical form. The energy radiated due to the rotation is found to amount almost exactly to $^1/_{32}\, h\nu$ for each revolution.

[1] The reader who is interested in following through in more detail the analysis involved in deriving these relations is referred to the author's paper on "Relativity and the Ether," *Amer. J. Sci.*, *New Haven*, **38,** 1914 (169).

THE LAWS OF ELASTICO-VISCOUS FLOW. II

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Communicated January 19, 1920

In a paper of Harold Jeffreys entitled "The Viscosity of the Earth,"[1] the author makes use of a formula which combines the laws of Larmor and of Maxwell.

$$n\left(S + \tau_2 \frac{ds}{dt}\right) = F + \frac{1}{\tau_1}\int F dt.$$

The integral implies a permanent set which as the author indicates would be inconsistent with the "accepted theories of tidal friction and variation of latitude. Hence τ_1 must be practically infinite." The formula is thus reduced to the expression $F = n_1(S + \tau_2\, ds/dt)$.

Experiments made on a great variety of materials show, however, that this expression must be seriously modified to represent the facts.

Thus it has been shown[2] that the displacement produced by a stress P is given by the expression[3] $S = C_1Pe^{h_1P} + C_2Pe^{h_2P}(1 - e^{-\alpha\sqrt{t}}) + C_3Pe^{h_3}t^p$. The last term produces permanent set, so for the present may be omitted. Putting

$$C_1 = \frac{1}{n_1} \text{ and } C_2 = \frac{1}{n_2} \quad \alpha = \frac{1}{\tau} \quad \theta = t^{1/2}$$

this becomes

$$S = \frac{P}{n_1}\left(A_1 + \frac{n_1A_2}{n_2}\left(1 - e^{-\frac{\theta}{\tau}}\right)\right)$$

whence

$$n_1\left(S - \tau\frac{ds}{d\theta}\right) = P\left(A_1 + \frac{n_1}{n_2}A_2\right).$$

For small stresses $A_1 = A_2 = 1$ and if n_2 equal n_1 this expression takes a form resembling that given by Jeffreys.

It is important to note, however, that this formula is based on the assumption that the viscosity is "external"—that is, it acts as though the viscous resistance were due to an absolute velocity ds/dt. But this is by no means evident; and indeed the probability is that a considerable part if not the major part of the viscous resistance may be "internal"—that is, due to the relative motion of parts. Thus if an element consist of two parts y and z, y being coupled to the next adjacent element by an elastic coupling n_1 and with z by an elastic coupling n_2 together with a viscous coupling ϵ_2 while ϵ_1' and ϵ_2' represent the "external" viscosities, the equations of motion will be

$$\rho_1 \ddot{z} = \epsilon_2(\dot{z} - \dot{y}) + n_2(z - y) + \epsilon_1' \dot{z}$$
$$\rho_2 \ddot{y} = \epsilon_2(\dot{z} - \dot{y}) + n_2(z - y) + \epsilon_2' \dot{y} + n_1 \frac{d^2y}{dx^2}.$$

If ρ_2, ϵ_1' and ϵ_2' be considered negligible, the solution, for not too rapid extinction, is

$$z = ae^{-\beta x} \cos p(t - vx)$$

in which

$$v^2 = \frac{n_1}{\rho} \frac{(n_2 - \rho p^2)^2 + p^2\epsilon^2}{n_2(n_2 - \rho p^2) + p^2\epsilon^2}$$
$$\beta = \frac{\rho^{3/2} p^4 \epsilon}{2\sqrt{n}[n_2(n_2 - \rho p^2) + p^2\epsilon^2]}.$$

If $p\epsilon$ is large compared with n_2

$$v^2 = \frac{n}{\rho}\left(1 + \frac{\rho^2 p^2}{\epsilon^2}\right)$$
$$\beta = \frac{\rho p^2}{2\epsilon v}.$$

So that in this case the higher the viscosity the less rapid the decay of the oscillations—quite the reverse of the conclusions on the former assumption. But the appearance of $\Theta = t^{1/2}$ is a more serious matter, making the use of the formula much more difficult.

The operator which should replace n is, therefore,

$$n_1 \frac{1 + 2\tau\sqrt{t}\,\frac{ds}{dt}}{1 + \frac{n_1}{n_2} A_2}.$$

But the application of this formula to such a problem as the earth's viscosity is still further complicated by the fact that all the constants are functions of the pressure and of the temperature in the earth's interior. Even though more or less probable assumptions may be made regarding the value of temperature and pressure as functions of the distance from the center, we know but little regarding the effect of these factors on either rigidity or viscosity.

1. It was found that the temperature effect may be represented with considerable accuracy by the expression

$$A = Ee^{(K + bP)\Theta}$$

in which P is the applied stress, Θ the temperature and E, K and b constants.

For room temperature the values of $b\Theta = h$ are given in table IV.

If we take $h = 0.2$ as fairly representative

$$S_2 = Pe^{.2P}(1 - e^{-\sqrt{t}}). \quad \text{(The unit } P = 100 \text{ gm.)}$$

so that G the couple $= Pr$ gm. cm. Thus we get for the displacement after a sufficiently long time

$$S = Pe^{.2P} \text{ and } \frac{1}{n_2} = \frac{S}{P}$$

TABLE I

P	S/P
0	1.0
1	1.2
10	7.0
50	20000.0

showing the very great increase in importance of the elastico-viscous term for large stresses. The same is also true for the purely viscous term.

The following table gives the ratio n_2/n_1 for twenty-two materials showing that there are certainly two elasticities—one of which is not accompanied by viscosity and the second is thus affected. In every case excepting that of sealing wax, where the ratio is unity, the second elasticity is much greater than the first, and in some cases enormously greater.

TABLE II

	$\frac{n_2}{n_1}$		$\frac{n_2}{n_1}$
Tin	60	Slate	150
Zinc	70	Shale	50
Marble	45	Soapstone	35
Limestone	30	Lead	40
Ebonite	8	Cadmium	65
Iron	2200	Gold	150
Steel	12000	Magnesium	250
Copper	8	Bapelite	7
Aluminum	4400	Ivory	50
Talc	200	Silver	80
Glass	100	Sealing Wax	1

The introduction of $t^{1/2}$ instead of t itself is a step so radical that it may be well to give an illustration in its justification.

For this purpose it is desirable to choose a material in which the elastico-viscous effect is well marked. This is notably the case for vulcanite, which has the added advantage of the relatively small importance of the third or purely viscous term. This illustration is, perhaps, the most striking in showing the appropriateness of $t^{1/2}$ instead of t; but all the materials investigated give similar results.

Following is a table of results for R_o, the return at the time t after releasing the stress.[4] $^{R}\sqrt{t}$ gives the result of calculation from

$$R = 890(1 - e^{-.57\sqrt{t}})$$

R_t gives values calculated from

$$R = 840(1 - e^{-.4t}).$$

The differences between calculated and observed values under Δ_1 and Δ_2 show that the former expression is very near the truth, while the latter is entirely inadequate.

TABLE III

t	R_o	R_t	Δ_1	R_t	Δ_2
1	380	387	7	277	—103
2	490	492	2	462	—28
4	600	605	5	672	+72
9	730	729	—1	820	+90
16	800	802	2	838	+38
25	840	841	1	840	00
30	853	851	—2	840	—13
00	890	890	0	840	—50

While the term involving a permanent set may not have any application to the problem of the Earth tides, yet it may not be amiss to draw attention to the fact that in some cases and especially at temperatures approaching the melting-point this term becomes the most important of all. The temperature coefficient in this case enters in the form $\Theta/T - \Theta$; giving as it should perfect fluidity at T, the melting-point.

In the former article the expression given for this viscous term is $S_3 = (Ft)^{\rho}$ in which $F = C_3Pe^{h_3P}$ and ρ is stated to be approximately one-half.

From more recent data the average value of ρ is 0.41; and if from the nineteen substances examined, four be excluded the average is 0.35 which makes it much nearer one-third than one-half.

The expression for the viscous terms should be $S_3 = (Ft)^{1/3}$ if the stress (P) is constant. If P is a function of time

$$S_3 = (\int F dt)^{1/3}.$$

Thus if P be given a constant value for a time t^0 and then changed to P_1 the corresponding value of the viscous term would be

$$S_3 = (F_o t_o + F_1 t)^{1/3}.$$

If the first stress be considerable and act for a long time, the effect of the second stress is negligible.

Following is a provisional table of the constants which appear in the formula for the torsional strain at room temperature.

$S = A_1 + A_2(1 - e^{-\alpha\sqrt{t}}) + A_3 t^p + A_4$ in which $A = CPe^{hP}$, P is the weight acting on a pulley of radius 5 cm. The unit of weight being 100 grams and the unit of time one minute. The specimen is a cylindrical rod 7.5 cm. long and 4 mm. in diameter.

The term A_4, which may be termed the "lost motion" should probably be considered as a part of the viscous term—but with a very small exponent r so that the whole viscous term may be represented by

$$S_3 = C_3 P e^{h_3 P}(t^p + Bt^r).$$

TABLE IV

	C_1	C_2	C_3	C_4	h_1	h_2	h_3	h_4	α	p
Tin slowly cooled.......	660	20.0	3.0	1.0	0.10	1.0	3.5	4.5	0.8	0.6
Tin quickly cooled......	640	8.0	0.6	0.3	0.00	1.8	3.8	4.6	0.8	0.6
Zinc slowly cooled......	312	3.0	3.4	2.0	0.00	0.4	0.3	0.8	1.0	0.5
Zinc quickly cooled.....	300	8.4	3.0	0.00	0.00	0.4	0.6	1.8	1.0	0.5
Marble...............	840	18.0	40.0	34.0	0.14	1.8	0.8	0.3	1.1	0.2
Limestone.............	600	20.0	9.0	11.0	0.10	0.3	0.3	0.8	1.2	0.3
Ebonite, first determ....	13×10^3	1600	600?	0?	0.00	0.1	0.0		0.6	0.5
Ebonite, second determ..	13×10^3	1800.0	750.0	0.0	0.00	0.1	0.2		0.9	0.7
Soft Iron Annealed.....	145	0.06	0.00	0.15	0.00	0.1		0.1	1.0	0.2
Soft Iron unannealed....	155	0.08	0.00	0.15	0.04	0.1		0.1	1.0	0.2
Tool steel, annealed.....	144	0.01	0.00	0.10	0.00	0.2		0.1	0.4	0.3
Tool steel, glass hard....	143	0.14	0.26	0.07	0.00	0.0	.03	0.06	0.4	0.4
Copper, annealed.......	250	1.3	0.00	0.75	0.06	0.1	1.8	1.2	0.5	0.3
Aluminum, annealed....	440	0.1	0.00	0.9	0.00	1.0	2.5	1.4	1.0	0.3
Talc, parallel cleavage ..	1340	4.0	20.0	110.00	0.60	3.0	1.4	1.2	1.0	0.3
Slate, parallel cleavage..	382	2.0	0.4	0.6	0.01	0.3	0.7	0.6	1.2	0.3
Slate, perp. to cleavage..	400	4.0	1.6	1.0	0.03	0.3	0.00	0.8	1.5	0.6
Sealing wax............	3×10^4	2×10^4	?	?	0.00	0.0	00.00	.00	0.7	?
Glass (plate)...........	446	3.0	0.00	4.0	0.00	0.0	00.00	.00	0.5	
Glass (lead)............	425	5.6	0.00	0.1	0.00	0.0	00.00	.15	1.4	
Shale, parallel cleavage..	400	8.4	1.0	1.2	0.02	0.16	00.35	.6	1.2	0.4
Lead..................	1500	40.0	7.0	4.0	0.00	0.2	1.9	3.6	1.1	0.4
Cadmium..............	460	70.0	5.4	2.4	0.00	0.2	0.6	0.8	1.0	0.5
Gold..................	450	3.0	0.2	1.0	0.00	0.4	0.2	0.3	1.0	0.3
Magnesium............	2300	9.4	8.0	50.0	0.00	0.25	0.4	0.6	1.2	0.3
Bakellite..............	7400	960.0	14.0	0.00	0.00	0.12	0.2		1.2	0.8
Ivory.................	10^4	320.0	3.0	0.00	0.00	0.10	0.2		0.8	0.6
Silver.................	480	6.0	2.0	0.4	0.00	0.06	0.1	0.2	1.0	0.3

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[1] *Mon. Not. Roy. Astron. Soc.*, **77**, No. 5.

[2] These Proceedings, **3**, No. 5.

[3] The strains in these experiments were torsional, thus involving only the rigidity

constant n. In the formula as given in the paper referred to the coefficients C and the exponents h are functions of the temperature. The stress P is constant and ρ is approximately one-half.

[4] It was found by experiment that for stresses not too great, the "direct" curve (on applying the stress) and the "return" curve (on releasing) were the same; or rather if the former is S and the latter R, then $S - R = Ct$.

NOTE ON THE SIMPLE DEVICE FOR INCREASING A PHOTOGRAPHIC POWER OF LARGE TELESCOPES

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Communicated by G. E. Hale, February 5, 1920

Among the many problems in sidereal astronomy that demand great telescopic power the three following may be cited as particularly significant for inquiries relating to the origin and evolution of stellar and galactic systems: (1) The total number of stars and limiting faintest magnitude in globular clusters; (2) the frequency and distribution of dwarf stars in clusters and in the sky at large; (3) the limits of the galactic system in certain directions. To these stellar problems we should add the highly interesting questions connected with the faint extra-galactic nebulae, and note that important contributions toward their solution seem to be only a little beyond our present telescopic power. Thus the best available photographs of globular clusters suggest that we are approaching the faintest magnitudes, and that, if we could only extend our studies two or three magnitudes farther, one phase of the problem of dwarf stars could probably be solved.

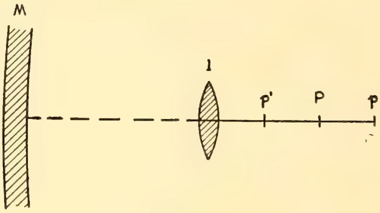
The longest exposures with the 60-inch reflector have yielded apparent photographic magnitudes between 20 and 21. The Hooker telescope, it is believed, will gain about a magnitude over the 60-inch provided the focal images are not much larger; but in the case of nebulae, since the ratio of focal length to aperture is the same for the two reflectors, no gain is to be expected except in scale, which is of course important for revealing new structure and purposes of measurement, but will not bring fainter structure to view. Small nuclei will also be shown in stronger contrast with the 100 inch telescope.

The investigation of faint stars and nebulae is of sufficient importance to justify every attempt to increase beyond normal limits the working range of great reflectors. Since these instruments are universally adapted to photographic observation, the problem is one of increasing the brightness of the image or increasing the sensitivity of the photographic plate.

In the device described below the principle employed is to increase the brightness of the image by reducing its area. This is accomplished by placing a short focus lens in the converging beam at an appropriate distance in front of the photographic plate. The immediate result is to reduce the effective focal length of the telescope, thus giving high speed and reducing the scale on the photograph.

In studies where the important thing is scale, an intensifying lens does not help; further, if a large field is desired, the intensifiers now available are unsatisfactory. But for the problems mentioned above, where scale and field are of second importance to limiting magnitude, the intensifying device is efficiently and inexpensively adaptable.

For example, by using such a device at the primary focus of the 100-inch, so as to reduce the scale to that of the 60-inch, the exposure time needed to give a required density in the photograph of a nebula should be less than one-half that otherwise necessary, even after allowing for a reasonable loss of light in the lens system of the intensifier.



Several lenses have been experimented with successfully in combination with various telescopes during the last six months. I am indebted to Mr. Benioff, who was associated with me in the early plans and experiments, for assistance in adapting the different intensifiers.

Let

F_1 be the focal length of the objective (mirror) of linear aperature A, $\overline{MP}$
f be the focal length of the intensifier of linear aperture a, $\overline{Ip}$
f' be the apparent focal length of the intensifier in the converging beam, $\overline{Ip'}$
F be the equivalent focal length of the combination,
d be the distance between the objective and intensifier, $\overline{MI}$
$D = F_1 - d$ be the distance from the focus of the objective to the intensifier, $\overline{IP}$
$R = F/F_1$ be the reduction of focal length and linear scale of the field.

From the relation giving the focal length of a combination of lenses we derive,

$$F = \frac{F_1 f}{F_1 + f - d} = \frac{F_1 f}{f + D}, \tag{1}$$

$$R = \frac{f}{f + D}. \tag{2}$$

Equation (2) shows that for a given intensifier the reduction depends only on the distance of the intensifying element from the normal focal point, P, of the telescope. Since the reduction is independent of the apertures and the focal length of the objective (mirror), being a function only of the focal length of the intensifier and its position in the cone of light, a given intensifier will reduce the focal lengths of all telescopes in the same ratio when placed at a fixed distance in front of the normal focal plane. The one now in use at Mount Wilson is suitable without structural alteration for work at both the primary and secondary foci of the 60-inch and 100-inch reflectors, and with the 10-inch photographic refractor of 45 inches focus. Conditions analogous to those above hold for amplifying lenses.

An interesting corollary of equation (2) is that no advantage accrues from having an intensifier of large aperture, unless, for the sake of accommodating a larger field, the increase in size can be made without increasing its focal length. Difficulties would obviously arise if the focal length of the intensifier were so short or so long that the distance of this secondary lens from the normal focus interfered with the photographic operations. Cf. equation (4) below.

The distances $\overline{IP} = D$ and $\overline{p'P} = D - f'$ are important in the actual manipulation of an intensifier. In the formula for conjugate foci,

$$\frac{1}{f} = \frac{1}{u} + \frac{1}{v},$$

we may put $u = f'$, $v = -D$, so that

$$\frac{1}{f'} = \frac{1}{f} + \frac{1}{D}, \tag{3}$$

and from (2)

$$f' = RD, \quad D - f' = D(1 - R). \tag{4}$$

That is, for any desired reduction of the effective focal length, the distance of the modified focus from the ordinary focal plane of the telescope is $1 - R$ times the distance of the intensifier from that plane. Equations (2) and (4) define the arrangement of a given intensifier and the photographic plate.

As an illustration, let us suppose that in working with the 100-inch Hooker telescope the reduction desired is $R = {}^1/_3$, and that the focal length of the intensifier employed is $f = 3$ inches; then $D = 6$ inches, $D - f' = 4$ inches. The primary focus of the Hooker reflector is $F_1 = 42.3$ feet. From (1) the equivalent focal length is 14.1 feet, and we have, in effect, a 100-inch reflector with a focal ratio of 1.7 instead of 5. If we assume that not more than 25% of the light is lost in the intensifier, the theoretical gain of the telescope (limiting faintness) is a little more than two magnitudes.

For $R = {}^1/_2$ the same intensifier should give a gain over normal photographic power of approximately 1.25 magnitudes, and leaves the scale very slightly inferior to that of the 60-inch reflector.

The most efficient intensifier for use with telescopes built for usual photographic purposes will be one that is adaptable to a large range of values of R; it should probably have a focal length between two and five inches and the largest aperture possible, and should not be wasteful of light through reflections from numerous glass-air surfaces. If an extended object, rather than a point source, is to be photographed, the flatness of the intensifier field is important; or, better still, its focal properties should be such as to improve the field of the primary.

Putting $S = F/A$ and $s = f/a$ for the ratios of focal length to aperture for the objective (mirror) and intensifier, respectively, we find the minimum equivalent focal length, which is obtained when the cone of light from a source on the axis exactly fills the intensifier, is

$$F_{\text{min}} = \frac{F_1}{1 + S/s};$$

also

$$D_{\text{max}} = Sa.$$

For the Mount Wilson reflectors $S = 5$, and the greatest reduction of scale, for full accommodation of the convergent light, is

$$R_{\text{min}} = \frac{1}{1 + 5/s}.$$

An intensifying lens with focal ratio 1.0 would, at the limit, increase the theoretical photographic limit by nearly four magnitudes for a source on the axis.

Intensifying devices are particularly suited to reflecting telescopes because of the freedom from chromatic aberrations; also they should be of especial value when used with instruments of largest aperture and longest focal length because in studies of faint nebulae the large scale allows the minification necessary for increased speed. Influences of bad seeing, or any other defects in the photographic images, are obviously minimized with the intensifier—a fact that may make it possible to use large reflectors under conditions otherwise impracticable.

The lens now in use is a Dallmeyer No. 2 Kinematograph of 3 inches focal length and focal ratio 1.9. It has a fine field, but the many glass-air reflections considerably diminish the light. The lens is mounted so that the reduction can be readily changed, with a range of R between $^5/_7$ and $^2/_7$ at the primary focus of the 100-inch reflector. For $R = {}^1/_2$ the workable field is about 6′ in diameter—quite sufficient for most clusters and nebulae; for $R = {}^1/_3$ it is one third as large. A comparative discussion of results obtained will be made in a later communication. The preliminary observations, which include successive exposures on the same plate with and without the intensifier, show that, when a reasonable allowance is made for loss of light in the lens, the predicted results are fully obtained, at least for nebulae. For instance, a three minute photograph of the spiral Messier 77, with a reduction of $^3/_8$, shows much fainter nebulosity than a ten minute exposure without the intensifier; and in the star cluster Messier 3 a reduction of $^8/_{15}$ gives a gain of nearly a magnitude for exposures of equal length.

ANTHROPOMETRY OF THE SIOUAN TRIBES

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The material is the result of investigations made on the occasion of the World's Columbian Exposition in Chicago in 1893 under the supervision of Professor Franz Boas. The measurements and observations were made by Franz Boas, F. C. Smith, J. W. Cooke, G. A. Kaven, Z. T. Daniels, E. F. Wilson, C. A. Helvin, F. C. Kenyon and G. M. West. The series contains male and female children and adults and includes a large series of half-bloods as well as full-bloods. The total series deals with 1431 individuals which makes it particularly valuable from a statistical point of view. Of this 1431, 594 were male adult full-bloods, 77 were male adult half-bloods, 181 were female adult full-bloods and 19 were female adult half-bloods. The remainder of the series are children.

Individuals from the following bands or tribes representing sub-divisions of the closely allied Siouan Indians were included in the series: Santee, Wahpeton, Sisseton, Yankton, Yanktonai, Cut Head, Teton, Brulé, Loafer, Oglala, Waziahziah, Sans Arc, Blackfoot Sioux, Minneconjou, Two Kettle, Hunkpapa and Assiniboine. In the larger adult male series each of these bands was represented on the average by about 30 men. The material was first studied by local bands, but so close an agreement was found in all the principal measurements and characteristics that it seemed preferable to include them all in one large series. The result is a fairly homogeneous group with a variability slightly below civilized groups in most characteristics.

Fourteen measurements were taken from which were calculated eight indices. Detailed observations were also made on the hair, skin, eyes, nose, ears, etc.

The series is of interest from two points of view: first, from the point of view of accurately describing and defining the Siouan type and showing its relationship to those American Indian tribes already described, and secondly from the point of view of racial intermixture since we have in this series individuals representing the results of the intermixture of two widely separated races of mankind.

On the first point, we may say that the Sioux with an average male stature of 1724 mm., are among the very tallest of the American Indians who range in stature from the 1530 mm. of the Guaranis of South America to the 1750 mm. of the Maricopa and Cheyenne. In head form they are mesocephalic with an index of 79.6. The face is very wide and high, 149.1 and 124.6 mm., respectively. On the average the face is nearly as wide as the head. The average proportion is 96.1%. The nasal bridge is rather high. The hair, of course, is straight and black, the eyes dark brown, and the skin shows varying shades of brown. The beard, moustache and body hair is poorly developed.

Comparative material for estimating the affinities of the Siouan tribes is not abundant. In nearly every measurement and index the averages for our series are in very close agreement with the Chippewa (Ojibway) series of Hrdlicka. Less detailed results of Boas suggest a uniformity of type with a majority of the Plains tribes (Blackfoot, Cheyenne, Arapaho, Crow, Pawnee) and less certainly with the Micmac, Abenaki, Delaware, Iroquois, Ottawa, and Menomini.

The half-bloods in the series are for the most part the results of the intermarriage of French, Scotch, Irish, and English men with Indian women. In the study of skin, hair, and eye color the tests were not sufficiently sensitive to bring out any clear cut and certain differences between the full-bloods and half-bloods, if such differences exist. In all of these characters the half-bloods seem to stand very close to the full-bloods. In regard to the amount of hair on the face (beard and moustache) the half-bloods stand intermediate between the Indians and whites.

The anthropometric characters bring out two points of interest: First, that in general body form and proportions the Sioux Indians are not very different from the whites with whom they have mixed. There are practically no differences between the full-bloods and half-bloods in absolute or relative shoulder height, shoulder width, sitting height, arm length, arm reach, and very small differences in the cephalic, facial, and nasal indices. Second, by far the most noticeable and consistent differences are differences in absolute size. The half-bloods are taller than the full-bloods. On the other hand, the full-bloods have the more massive heads, faces, and noses. While the relation of these diameters, as expressed by indices, are very much alike, the absolute diameters are different. The most marked difference is in the width of face. The full-bloods have a much wider face than the half-bloods or whites. The height of the face of full-bloods is also greater and the area of the face much larger than in the half-bloods. It seems that in all those characters in which the Indian differs most markedly from the whites, the half-bloods stand nearer to the Indians than to the whites.

In our present investigation we found the half-bloods more variable than the full-bloods in stature, shoulder height, sitting height, head length, face width, cephalo-facial index, facial index and nasal index. In the remaining twelve of the twenty-one observations the full-bloods were slightly more variable. But in nearly every seriation the distribution among the half-bloods was more irregular.

In our correlations we found the closest relationships to exist between diameters in the same axis such as stature and arm reach, stature and arm length, stature and sitting height, and width of head and width of face. A fair degree of correlation exists between gross diameters in opposite axes such as stature and width of shoulder. Other diameters in different axes, such as length and width of head, height and width of face, and height and width of nose show a lower degree of correlation. The differences in degree of correlation of two diameters between full-bloods and half-bloods are not very great. On the whole, it does not seem as if intermixture had seriously affected the degree of correlation. In the instance of face width and head width the correlation seems to be increased in the half-bloods.

The distribution of face width among the one-fourth Indians, two-fourths Indians, one-half Indians, and three-fourths Indians indicates that the inheritance is alternating. The mixed individual tends to inherit either the Indian or European type of face, intermediate types being of relatively rare occurrence. A high degree of correlation between width of face and width of head was found in the mixed group. Individuals with a narrow head had a tendency to inherit also the narrow face and vice versa. A summary of the anthropometric results will be found in table I and of the correlations in table II.

A detailed report will be published in the *Anthropological Papers of the American Museum of Natural History*, vol. 23, part 3.

TABLE I

Summary of the Anthropometric Results

Male Adult Full-Blood and Half-Blood Sioux Indians

CHARACTER	AVERAGE		STANDARD DEVIATION (σ)		COEFFICIENT OF VARIABILITY (V)	
	Full-bloods	Half-bloods	Full-bloods	Half-bloods	Full-bloods	Half-bloods
Number of Cases	540	77	540	77	540	77
Stature	172.4	173.5	5.64	6.81	3.27	3.92
Shoulder Height	142.7	142.3	5.03	6.07	3.52	4.26
Shoulder Width	38.8	38.9	1.92	1.89	4.94	4.83
Index Shoulder Width	22.5	22.4	1.10	1.01	4.88	4.51
Sitting Height	88.5	89.6	3.50	4.39	3.95	4.89
Index Sitting Height	51.4	51.6	1.68	1.94	3.26	3.76
Arm Reach	181.4	182.2	7.03	6.99	3.87	3.83
Index Arm Reach	105.2	105.0	2.41	2.19	2.29	2.09
Arm Length	77.0	77.3	3.57	3.28	4.64	4.24
Index Arm Length	44.6	44.6	1.47	1.26	3.29	2.82
Head Length	194.9	194.4	6.16	7.12	3.16	3.66
Head Width	155.1	154.3	5.39	5.04	3.47	3.26
Cephalic Index	79.6	79.4	3.20	2.64	4.03	3.33
Face Width	149.1	143.4	5.45	5.49	3.65	3.83
Cephalo-Facial Index	96.1	92.9	3.22	3.23	3.35	3.48
Anatomical Face Height	124.6	121.5	6.39	6.36	5.12	5.23
Anatomical Face Index	83.6	84.8	4.84	5.28	5.78	6.22
Nasal Height	58.3	54.9	3.94	3.55	6.75	6.48
Nasal Width	39.9	37.6	3.22	3.04	8.07	8.08
Nasal Index	68.8	69.2	7.05	7.08	7.05	7.08

TABLE 2
CORRELATIONS IN THE MALE ADULT SERIES

CORRELATED MEASUREMENTS	COEFFICIENTS OF CORRELATION	
	540 Full-bloods	77 Half-bloods
Stature and Sitting Height	0.61	0.65
Stature and Width of Shoulder	0.35	0.48
Stature and Arm Reach	0.81	0.85
Stature and Length of Arm	0.70	0.76
Length and Width of Head	0.27	0.54
Width of Head and Width of Face	0.55	0.51
Anatomical Height and Width of Face	0.16	0.08
Height and Width of Nose	0.05	0.02

TRANSMISSION OF EYE-DEFECTS INDUCED IN RABBITS BY MEANS OF LENS-SENSITIZED FOWL-SERUM

By M. F. Guyer and E. A. Smith

Zoölogical Laboratory, University of Wisconsin

Communicated by L. Hektoen, January 19, 1920

As the work progresses it is becoming increasingly evident that the discoveries made in the field of serology all have their broader biological aspects, and that they afford new methods of attacking certain fundamental biological problems. Not the least of these is a possible method of breaching the wall which has gradually come to surround the long-standing problem of provoking specific modifications in the germ-cell through the direct action of external agencies, or indirectly, through changes produced in the parental body. For if external influences *can* be transmitted to the germ-cell, the one obvious means of conveyance in higher animals is the blood, and when one considers the protean possibilities which modern work has revealed in the blood, it is certainly a rational quest to seek in this medium a possible means of altering the germ. To set the problem more specifically, if a serum of one species of animal can be so sensitized to a given tissue or tissues of another species that it will become toxic or lytic for the tissue in question, may it not be that there is sufficient constitutional identity between the mature substance of the tissue and at least some of its material antecedents in the germ, that the latter may also be influenced specifically by the sensitized serum? Or may not changes in its own tissues originate antibodies in the blood serum of a given individual which will not only react with the tissue elements themselves but with their correlatives in the germ cells?

In an attempt to find answers to these and kindred questions we have been engaged in series of experiments which have extended over a period of four years and which are still in progress. To state briefly some of our results, we have been able occasionally to produce what appear to be specific antenatal lens-defects in rabbits and mice by means of fowl-serum sensitized to rabbit-lens and mouse-lens respectively. And, what is probably of more interest, breeding experiments with the affected rabbits show that the defect, once established, may be transmitted to subsequent generations.

Since most of the work was done with rabbits the following account is concerned with them alone. Chickens were employed as the source of the antibodies. To produce these, the fresh lenses of rabbits were pulped thoroughly in a mortar and diluted with normal salt solution. By means of a hypodermic syringe about four cubic centimeters of the emulsion was injected into the peritoneal cavity of the fowl. In later experiments some of the fowls were injected intravenously as well as intraperitoneally. Ordinarily the fowls were treated with such lens-emulsions at intervals of a week for four or five weeks and were then allowed to rest a week of ten days before bleeding.

In the mean time rabbits had been bred so as to have the young advanced to about the tenth day of pregnancy. From four to seven cubic centimeters of the sensitized fowl-serum (sometimes diluted with normal saline) were injected into the pregnant rabbits at intervals of two or three days from about the tenth to the twentieth days of pregnancy. Some rabbits died from the treatment, many young were killed in utero, but a goodly number also survived. Among the latter were occasional individuals with opaque (sometimes liquid) lenses and eyes otherwise defective (reduced size, cleft iris, persistent hyaloid artery, bluish or silvery color instead of the characteristic red of the albino eye, and occasionally almost complete disappearance of the eyeball). Taking into account the method of embryological development of the eye, however, these defects are probably all attributable to the early injury of the lens.

Out of sixty-one surviving young from mothers treated during pregnancy with lens-sensitized serum, four had one or both eyes conspicuously defective and five others had eyes which, though less noticeably modified, were nevertheless abnormal. Others also may have had their eyes affected, but if so, the condition was not observable from the exterior. As a matter of fact, liquid lenses were found in a few young ones which were dissected in the earliest experiments. This condition was not detectable in the living animal. Of the controls, from mothers treated with pure (i. e., unsensitized) fowl-serum twelve living young were obtained, and from others treated with serum sensitized to rabbit-tissue other than lens, thirty-six young were secured, or a total of forty-eight which survived long enough to show the condition of their eyes. In not one of these forty-eight controls was there evidence of eye-defect. The experiments indicate, therefore, that the effect of the lens-sensitized serum is specific.

As already mentioned, once the defect is secured, it may be transmitted to subsequent generations through breeding. So far we have succeeded in passing it to the sixth generation. There seems to be no reason why it will not go on indefinitely since the imperfection tends to become worse in succeeding generations and also to occur in a proportionally greater number of young. The transmission was not infrequently of an irregular unilateral type, sometimes only the right, at others only the left eye showing the defect. In later generations there was an increasing number of young which had both eyes affected.

To meet the objection that we were not getting instances of true inheritance in each generation but merely placental transmissions of antibodies or kindred substances from the blood stream of the mother, it was obviously necessary to establish the descent through the male line alone. To do this females from strains of rabbits unrelated to our defective-eyed stock were mated to defective-eyed males. The first generation produced in this way were invariably normal-eyed, but when females of this generation in turn were mated to defective-eyed males the defect reappeared in some of the progeny somewhat after the manner of an extracted Mendelian recessive. Inasmuch as the defect can thus be made to reappear in the descendants of a male with abnormal eyes when he is mated to a female from unrelated and untreated stock, it is obvious that it could only have been conveyed through the germ-cells of the male, and that it may, therefore, be pronounced an example of true inheritance.

The detailed study, with charts, pedigrees, drawings and photographs will appear shortly in the *Journal of Experimental Zoölogy.*

ON THE MECHANISM OF FEVER REDUCTION BY DRUGS

By Henry G. Barbour and Julian B. Herrmann

Department of Pharmacology, Yale University, School of Medicine

Communicated by L. B. Mendel, Read before the Academy, November 10, 1919

Correlation of antipyretic drug action with the carbohydrate metabolism has been suggested by one of us[1] as a result of two findings in fever patients. In the first place dextrose by mouth has been found frequently to exert a mild antipyretic action; on the other hand, acetyl-salicylic acid or antipyrine under similar conditions increase the respiratory quotient, thus indicating a relatively augmented carbohydrate combustion. These facts point strongly to a mobilization of sugar by such drugs.

We have, therefore, investigated the effects of antipyretics upon normal and fevered dogs with particular attention to the concentration of dextrose in the blood. Mild fever was produced by the subcutaneous injection upon the preceding day of a suspension of killed colon bacilli. Doses of from 0.2 to 0.5 gram. per kilo of the following drugs were given: sodium salicylate, quinine hydrochloride, antipyrine (all subcutaneously) and acetyl-salicylic acid (per os). In fevered dogs a fall in body temperature of about 1° C. resulted; on the other hand a slight increase was the only observed change in the temperature of normal animals.

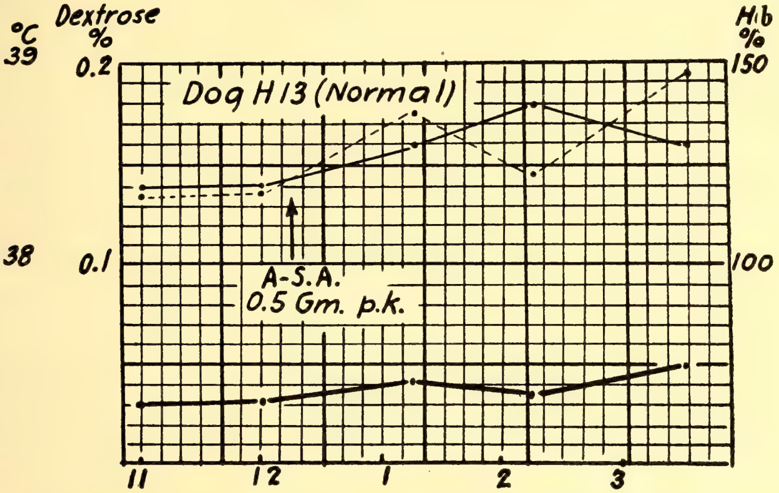

FIG. 1. Effects of acetyl-salicylic acid (0.5 gm. per kilo) on normal dog. Rectal temperature: light line; blood dextrose per cent: broken line; hemoglobin per cent: heavy line.

Blood Dextrose.—Both series of dogs were subjected to investigation of the blood sugar concentration by the Benedict-Lewis method. In all animals irrespective of the fevered condition there resulted a moderate hyperglycaemia, the extent of which may be judged from the following summary:

DEXTROSE CONCENTRATION IN THE BLOOD

	BEFORE ANTIPYRETIC %	MAXIMUM AFTER ANTIPYRETIC %
Average of 13 normal dogs	0.137	0.186
Average of 10 fevered dogs	0.139	0.218

Blood Volume.—The hemoglobin concentration was followed in most of these experiments simultaneously with the blood sugar, employing the colorimetric method of Cohen and Smith.[2] In the normal dogs was found a rise in the hemoglobin percentage accompanying the slight temperature increase. The thickening of the blood thus indicated was insignificant in comparison with the observed dextrose increase, the latter being thus proven absolute rather than merely relative.

Fevered dogs, on the other hand, showed a distinctly increased blood volume as indicated by the diminished hemoglobin percentage accompanying the antipyretic effect. Dilution of the blood of fever patients by acetyl-salicylic acid and by antipyrine has also been observed by one of us.

The above described effects as exemplified in normal and fevered dogs are illustrated in figures I and II, respectively. The following conclusions have been drawn from this work:

(1) A variety of antipyretic drugs increase the blood sugar in both normal and fevered dogs.

(2) In the latter this effect is accompanied by a dilution of the blood (indicated by diminished hemoglobin percentage) and a fall in temperature, neither of which occur in healthy animals.

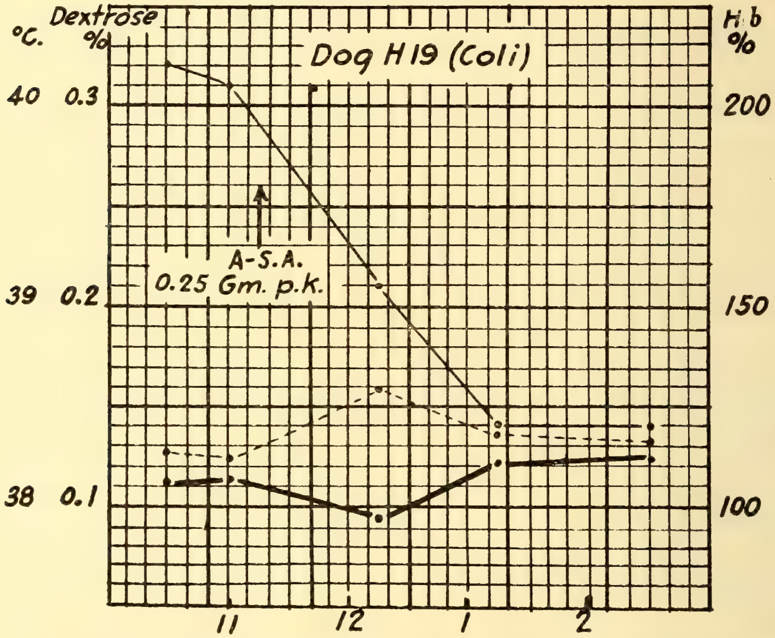

FIG. 2. Effects of acetyl-salicylic acid (0.25 gm. per kilo) on dog treated on preceding day with suspension of killed colon bacilli. (See Fig. 1.)

(3) *Theory of the mechanism of fever reduction by drugs.*—Antipyretic drugs increase the blood content of dextrose, a substance itself often exhibiting temperature-reducing properties when introduced from without. Moderate hyperglycaemia favors dilution of the blood.[3] In fevered animals antipyretics actually produce such a plethora, the hyperglycaemia probably contributing largely to this effect. Plethora promotes dissipation of heat both by radiation (peripheral vaso-dilation) and water evaporation from the surface of the body. The occurrence of plethora with its resulting anti-pyretic effect is apparently limited to fevered animals. This fact should probably be attributed not so much to a greater degree of hyperglycaemia as to the relative water retention by the tissues which is said to accompany febrile conditions.[4]

[1] Barbour, H. G., "Antipyretics" (Papers I and III), *Arch. Int. Med.*, Dec., 1919; and *Proc. Soc. Exp. Biol. Med.*, **16**, 1919 (136).

[2] Cohen, B. and Smith, A. H., *J. Biol. Chem.*, **39**, 1919 (489).

[3] Cf. Fisher, G. and Wishart, M. B., *J. Biol. Chem.*, **13**, 1912 (49).

[4] A part of the expenses of the work herein reported has been defrayed from the Francis E. Loomis Research Fund of the Yale University School of Medicine. More extensive reports will be sent to the *Journal of Pharmacology & Experimental Therapeutics* and to the *Archives of Internal Medicine*.

INFERENCES FROM THE HYPOTHESIS OF DUAL ELECTRIC CONDUCTION; THE THOMSON EFFECT

BY EDWIN H. HALL

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Communicated January 29, 1920

At the Washington meeting of the National Academy of Sciences in April, 1919, I presented two papers that have not yet been published. One was on the *Effect of Pressure on Electric Resistance and on Peltier Heat in Metals*, the other on *Thermal Conduction in Metals*, both being written from the standpoint of Dual Electric Conduction.

The first named of these two papers contained implicitly the following propositions:

1*a*. Increase of pressure should, by bringing the atoms and the metal ions closer together, increase k_a, the associated-electron conductivity, and decrease k_f, the free-electron conductivity. We might, then, expect the total conductivity, k, to increase under pressure in metals having a relatively small value of $(k_f \div k_a)$ and to decrease in metals having a relatively large value of this ratio.

1*b*. As antimony and bismuth have exceptionally small values of k, they probably have exceptionally large values of $(k_f \div k_a)$, and this may account for the fact that, among twenty metals examined by Bridgman, these two were the only ones to show a decrease of conductivity under an increase of pressure.

2*a*. If the ratio $(k_f \div k_a)$ is greater in metal B than in metal A, ionization must occur at the junction of the two metals when a current flows from A to B, and re-association must occur there when the current flows from B to A. As ionization is doubtless accompanied by absorption of heat and re-association by evolution of heat, we have here an action which may play a very important part, if not the chief part, in the Peltier effect.

2*b*. The exceptionally large value of $(k_f \div k_a)$ that probably exists in bismuth may account for the fact that heat is absorbed when a negative current goes into this metal from any other.

2*c*. As increase of pressure probably decreases the ratio $(k_f \div k_a)$, we should expect an absorption of heat where a negative current flows from a metal under high pressure to the same metal uncompressed. If we call this effect of compression *plus* and the opposite effect *minus*, we find that, among eighteen metals examined by Bridgman under high pressures, thirteen showed *plus* effects only, none showed *minus* effects only, but five showed mixed effects, *minus* at 0° C. and *plus* at 100° C.

3*a*. According to the theory under discussion the Thomson effect should disappear if $(k_f \div k_a)$ became zero. Accordingly increase of pressure, causing a decrease of this ratio, should diminish the Thomson effect. Among eighteen metals examined by Bridgman under pressure, nine showed a decrease throughout the whole range of observation, one showed an increase through the whole range of observation, one showed zero change everywhere, the other seven showed mixed effects.

The following may be taken as a summary of the second paper:

1. Thermal conduction in a metal may be due to the convective action of a circulating electric current, free electrons moving down the temperature gradient and associated electrons moving up, with ionization at the hot end of the metal, involving absorption of heat, and re-association at the cold end, involving the emission of heat.

2. A quantitative test of this thesis, made necessarily with various assumptions, indicated ionizing potentials of the same order of magnitude as those observed in the ionization of metal vapors; but it seemed doubtful whether values so large as those here indicated are consistent with the magnitudes of the Thomson effect. It is to be noted that ionization within the solid body of a metal may well require less energy than ionization of the vapor.

It seemed desirable to study the Thomson effect more fully from the standpoint of dual electric conduction before proceeding farther with the theory of thermal conduction. The results of an examination of the Thomson effect are now ready for publication.

THE THOMSON EFFECT

In what follows:

n = the no. of free electrons per cu. cm. of a metal.

v = the no. of cu. cm. of metal containing 1 gm. of free electrons.

m = mass of electron, and G = no. of electrons in 1 gm. of electrons.

Then $nv = G = 1 \div m$. (1)

p = press. of free electrons, in dynes per sq. cm. of cross-section of the metal.

R = the gas constant for one molecule, or for one electron, $= 1.37 \times 10^{-16}$.

Then $p = nRT$, (2)

and, for 1 gm. of free electrons, $pv = nvRT = RT \div m$. (3)

P = electro-mag. pot. due to purely electric forces within the metal.

P_a = electro-mag. pot. due to differential attraction of the unequally heated metal for the associated electrons.

P_f = corres. pot. due to differential attraction of the metal for the free electrons.

k_a = part of specific electric conductivity due to associated electrons.
k_f = part of specific electric conductivity due to free electrons.
k = $k_a + k_f$ = total electric conductivity.
e = the electron charge in electro-mag. measure = 1.6×10^{-20}.
λ = the latent heat of ionization per $(1 \div e)$ electrons, in ergs.
λ' = the latent heat of ionization per electron, in ergs.

Hypothesis (*A*) is, that the mere mechanical tendency of the free electrons is toward uniformity of pressure throughout the unequally heated metal.

Hypothesis (*B*), alternative with (*A*), is that the mechanical tendency of the free electrons is toward the state of equilibrium produced by thermal effusion; that is,

$$p \div T^{1/2} = \text{constant}. \quad (4)$$

Let C of figure (1) be the cold end and H the hot end of a metal bar forming part of a circuit in which an electric current is maintained by thermo-electric action, the resistance of some part of the circuit being so great that the conditions existing in CH are very little different from those of equilibrium. In this case the Joule heat generated in CH can be neglected in comparison with the Thomson heat there generated or absorbed.

Contrary to custom, the direction of the stream of electrons through the metal will, in this paper, be taken as the direction of the current, and accordingly σ, the Thomson heat at any temperature T, will be defined as the heat absorbed by the electromagnetic unit quantity of electricity, $(1 \div e)$ electrons, in going through the metal from a place of temperature $(T - 0.5)$ degree to a place of $(T + 0.5)$ degree. This definition will make σ negative for copper and positive for iron. The value of σ will be expressed in ergs.

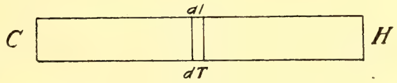

FIG. 1

When the unit quantity of electricity, $(1 \div e)$ electrons, $(m \div e)$ gm., goes through the slice dl of the bar CH, from the isothermal surface T to the isothermal surface $T + dT$, the fraction $(k_f \div k)$ of it consists of free electrons and the part $(k_a \div k)$ of associated electrons. We have now to take note of the changes of energy, of various kinds, undergone by these two parts of the current. We shall list the various forms of energy here considered under five general heads: (1) bulk potential energy, or pv potential energy, to which the free electrons only are subject; (2) kinetic energy of the electrons, which we shall regard as negligible in the associated electrons and equal to that of monatomic gas molecules in the free electrons; (3) electric-charge potential energy, the P energy, to which both the free and associated electrons are alike subject; (4) the P_f potential energy, which affects free electrons only; (5) the P_a potential energy, which affects the associated electrons only. We must take account also of the gain of energy involved in the ionization which may occur in the current from T to $T + dT$, due to the increase in the ratio $(k_f \div k)$ with rise of temperature.

Under hypothesis (*A*): All changes due to change of $(k_f \div k)$ being considered last, we have as the change of pv energy, (see equation (3))

$$(k_f \div k)(m \div e)d(pv) = (k_f \div k)(R \div e)dT. \qquad (a)$$

The gain of thermal kinetic energy by the free electrons is

$$(k_f \div k)(1 \div e)\,.\,\frac{3}{2}RdT. \qquad (b)$$

The gain of P potential energy is dP. (*c*)
The gain of P_f potential energy is $(k_f \div k)dP_f$. (*d*)
The gain of P_a potential energy is $(k_a \div k)dP_a$ (*e*)
The gain of energy through ionization is
$\lambda d(k_f \div k) = (1 \div e)\lambda' d(k_f \div k)$. (*f*)

The sum of all these quantities is the Thomson-effect heat absorbed between T and $T + dT$; that is

$$\sigma dT = (a) + (b) + (c) + (d) + (e) + (f) \qquad (5)$$

From this we get

$$\sigma = \frac{k_f}{k}\cdot\frac{5}{2}\frac{R}{e} + \left[\frac{k_f}{k}\cdot\frac{dP_f}{dT} + \frac{k_a}{k}\cdot\frac{dP_a}{dT} + \frac{dP}{dT}\right] + \frac{\lambda'}{e}\cdot\frac{d(k_f \div k)}{dT}. \qquad (6)$$

From the conditions of equilibrium, under hypothesis (*A*), in a detached bar like *CH* we have[1]

$$\frac{k_f}{k}\cdot\frac{1}{e}\left(\mathrm{R} + \frac{RT}{n}\cdot\frac{dn}{dT}\right) + \left[\frac{k_f}{k}\cdot\frac{dP_f}{dT} + \frac{k_a}{k}\cdot\frac{dP_a}{dT} + \frac{dP}{dT}\right] = 0 \qquad (7)$$

Subtracting (7) from (6) we get

$$\sigma = \frac{k_f}{k}\cdot\frac{3}{2}\frac{R}{e} - \frac{k_f}{k}\cdot\frac{RT}{ne}\cdot\frac{dn}{dT} + \frac{\lambda'}{e}\cdot\frac{d(k_f \div k)}{dT}. \qquad (8)$$

Under hypothesis (*B*) we get $0.5R$ instead of the first R in (7), and this gives $2R$ instead of $1.5R$ in the first term of (8). Hypothesis (*B*) makes no other change in (7) or (8).

I shall now try to put equation (8) into a form suitable for dealing with the values of σ found by Bridgman[2] in his experiments on a large number of metals. In this undertaking I assume that for present purposes the following equations hold above 0° centigrade:

$$n = zT^q, \qquad (9)$$

and

$$(k_f \div k) = C + C_1t + C_2t^2, \qquad (10)$$

where z, q, C, C_1 and C_2 are constants, and t is temperature on the ordinary centigrade scale.

I shall, moreover, assuming that the total heat of ionization per electron is made up of a part $\lambda'_{\circ}$, due to the overcoming of atomic attraction, a part $1.5RT$ for the kinetic energy gained, and a part RT for the pv potential energy gained, write

$$\lambda' = \lambda'_{\circ} + 2.5RT = \lambda'_{\circ} + 2.5R(273 + t). \qquad (11)$$

Keeping to hypothesis (A), and so using equation (8) for σ, I get by substitution according to eqs. (9), (10) and (11),

$$\sigma = K + (K_1 + K_2 t)T, \qquad (12)$$

where K, K_1 and K_2 are constants,[3] defined by the equations

$$K = \frac{R}{e}\left[C(1.5-q) + C_1\left(\frac{\lambda'_{\circ}}{R} - 273(1.5-q)\right) - 273C_2\left(\frac{2\lambda'_{\circ}}{R} - 273(1.5-q)\right)\right] \qquad (13)$$

$$K_1 = \frac{R}{e}\left[C_1(4-q) + C_2\left(\frac{2\lambda'_{\circ}}{R} - 273(1.5-q)\right)\right] \qquad (14)$$

$$K_2 = \frac{R}{e} \cdot C_2(6.5-q). \qquad (15)$$

I have put σ into the form shown by equation (12) in order to make my expression for it correspond as nearly as may be to that used by Bridgman to set forth the results of his experiments. He writes, in substance,

$$\sigma = (A + Bt)T, \qquad (16)$$

where A and B are constants, the latter being zero in many metals.

Bridgman finds nothing corresponding to my constant K, and I have spent much labor in attempting to get rid of this constant; but no reasonable assumption that I can make eliminates it from my general expression for σ. On the other hand, equation (13) shows that K is the sum of many terms, some positive, some negative, and there is nothing to show that it may not be very small, too small to appear in such experiments as those of Bridgman. Accordingly, in dealing with his observations I put K, and so the second member of (13), equal to zero. This gives me an equation of which I make frequent use in the form

$$C = \left[-C_1\left(\frac{\lambda'_{\circ}}{R} - 273(1.5-q)\right) + 273C_2\left(\frac{2\lambda'_{\circ}}{R} - 273(1.5-q)\right)\right] \div (1.5-q). \qquad (17)$$

As to the A and B of equation (16), I take these to be, respectively, the K_1 and the K_2 of my equations, and, as Bridgman gives the value of A and B for every case dealt with, I have the K, K_1 and K_2, of equations (13), (14) and (15), replaced by definite numerical terms.

These three equations now contain the five unknowns, C, C_1, C_2, $\lambda'_{\circ}$, and q. Accordingly, I must assume values for two of these quantities in order to evaluate the other three. As a rule, I have assumed values of

q and λ'_o, different ones in succession for each metal, and have then worked out the corresponding values of the other quantities, following the order C_2, C_1, C.

Values of q and λ'_o that would lead to values for C greater than 1 or less than 0 are of course rejected, as such values of C would be meaningless, but negative values of C_1 and C_2 are not to be regarded as impossible. I was at first inclined to the opinion that the ratio $(k_f \div k)$ would always increase with rise of temperature, but this is not a logical necessity, in the present state of our knowledge, and it appears from what follows that the ratio in question is quite as likely to decrease as to increase in the temperature ascent from 0° to 100°. An interesting relation between this conclusion and the observations of Bridgman on change of resistance under pressure, at various temperatures, will be shown in this paper.

Cases in which K_2, or B, is 0: In twelve of the seventeen metals for which Bridgman gives the value of σ the B of equation (16) is zero. Such cases are very easy to deal with. We have $K_2 = 0$, and so, from equation (15), $C_2 = 0$, unless q has the improbably large value 6.5. If C_2 is 0, we have, from equation (14),

$$C_1 = K_1 \text{ (or } A) \frac{e}{R} \div (4 - q). \tag{18}$$

Substituting for C_1 in equation (17), we get

$$C = \left[-K_1 \cdot \frac{e}{R} \left(\frac{\lambda'_o}{R} - 273(1.5 - q) \right) \div (4 - q) \right] \div (1.5 - q). \tag{19}$$

For any given value of q this becomes

$$C = K'\lambda'_o - K'' \tag{20}$$

where K' and K'' are new constants, the values of which depend on q.

This equation shows that, for a fixed value of q, we can represent the relation of C to λ'_o by means of a straight line drawn on the $(C\text{-}\lambda'_o)$ plane. Such a line is useful for purposes of interpolation and extrapolation.

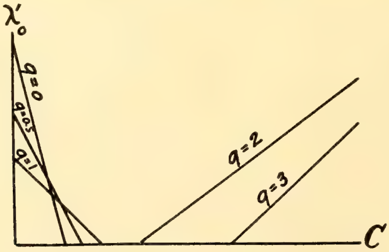

FIG. 2

It is to be noted that the metals for which K_2 is 0 fall into two groups, for one of which K_1 is positive, while for the other it is negative.

Figure 2 shows the general character of the set of q-constant lines for the first group, and figure 3 does the same for the second group.

For both of these groups 1.5 is a critical value for q. Examination of equation (19) shows that, when $q = 1.5$, C becomes infinite unless λ'_o at the same time becomes 0; and if λ'_o becomes 0 while $q = 1.5$, C becomes indeterminate. In both figure 2 and figure 3, therefore, $q = 1.5$ would imply a line coincident with the C axis.

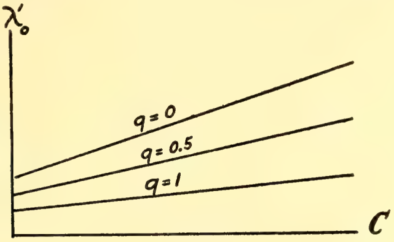

FIG. 3

If q in equation (19) has a value between 1.5 and 4, C will have the same sign as K_1, which is positive for the first group and negative for the second group. Accordingly, since negative values of C are meaningless, $1.5<q<4$ is possible for the first group but not for the second group.

Cases in which K_2 is not 0. There are in Bridgman's list five metals for which K_2, or B, is not zero. These are aluminium, gold, iron, molybdenum, and thallium. Finding the value of C, for a given combination of q and λ'_o, in these metals, is a somewhat roundabout, though not difficult, process. The value of C_2 is found by use of equation (15), then the value of C_1 by use of (14), then the value of C by use of (17). The q-constant lines on the (C-λ'_o) plane are no longer straight, as they are in figures 2 and 3. Figure 4 shows their general shape for $q = 0$, $q = 0.5$ and $q = 1$, in the range from $C = 0$ to $C = 0.20$. It is to be noted that, though K_1 is positive in iron and thallium while negative in aluminium, gold, and molybdenum, the lines in question are of the same general shape and arrangement for all five metals. The value of K_2 is positive for all.

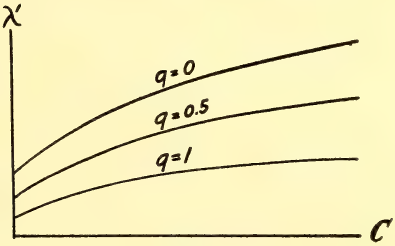

FIG. 4

Examination of equation (17) shows that, when $q = 1.5$, C is ∞ or is indeterminate. In the latter case, we may have either $\lambda'_o = 0$ or $C_1 = 546C_2$.

When q is made larger than 1.5 but smaller than 6.5, negative values of C result, in all five of the metals, so far as I have examined the matter.

The following tables represent my results for all of the elementary metals for which Bridgman gave the values of σ in the paper already referred to. A dash (—) in place of a number indicates that the number would be negative, and that a negative value in this place is regarded as impossible.

The values of $(k_f \div k)$ at 100° are found by use of equation (10).

The δ_o of these tables is the "ionizing potential" needed for dealing with the attraction which an electron must overcome in the process of ionization. If this ionizing potential is 1 volt, for example, the *internal* work of ionization is about 11700 R ergs per electron. The *external* work of ionization, to provide the kinetic energy and the pv energy of the gaseous state, is 2.5 RT ergs per electron. This at 0° would require an ionizing potential of about 0.058 volt, which must be added to the δ_o of the tables in order to get the total ionizing potential in volts.

The general significance of these tables can be illustrated as follows, with numbers taken from table 1: If in cobalt the value of C is 30%, the Thomson effect heat, as found by Bridgman for this metal, can be accounted for either by

taking $q = 2$, $\delta_o = 0.02$, $C_1 = 457 \times 10^{-6}$, $C_2 = 0$,

or by

taking $q = 3$, $\delta_o = 0.01$, $C_1 = 915 \times 10^{-6}$, $C_2 = 0$.

In the first case $(k_f \div k)$ will be 0.346 at 100°, while in the second case it will be 0.392. If the value of C is 40%, there are corresponding values of q, δ_o, etc., that will account for σ. A like statement would hold for any value of C between 30% and 40%, and for numberless other values of this ratio.

First Group: Metals for which $K_1 > 0$ and $K_2 = 0$

TABLE 1: COBALT.

$K_1 = 7.8$, $K_2 = 0$, $C_2 = 0$

C, or $(k_f \div k)$, at 0°	$C_1 = 228 \times 10^{-6}$			$C_1 = 261 \times 10^{-6}$			$C_1 = 305 \times 10^{-6}$			$C_1 = 457 \times 10^{-6}$			$C_1 = 915 \times 10^{-6}$		
	q	δ_o	$(k_f \div k)$ at 100°	q	δ_o	$(k_f \div k)$ at 100°	q	δ_o	$(k_f \div k)$ at 100°	q	δ_o	$(k_f \div k)$ at 100°	q	δ_o	$(k_f \div k)$ at 100°
0.01	0	.03	.033	.5	.02	.036	1	.01	.041	2	—		3	—	
0.08[1]	0	—		.5	—		1	.00	.111	2	—		3	—	
0.20	0	—		.5	—		1	—		2	.01	.246	3	—	
0.30	0	—		.5	—		1	—		2	.02	.346	3	.01	.392
0.40	0	—		.5	—		1	—		2	.03	.446	3	.02	.492

[1] If there is any combination of q and δ_o that will make $C = 0.10$ for cobalt, the q must be very near 1.5.

TABLE 2: NICKEL.

$K_1 = 3.56, K_2 = 0, C_2 = 0$

C, or $(kf \div k)$, at 0°	$C_1 = 104 \times 10^{-6}$			$C_1 = 119 \times 10^{-6}$			$C_1 = 139 \times 10^{-6}$			$C_1 = 208 \times 10^{-6}$			$C_1 = 416 \times 10^{-6}$		
	q	δ_0	$(kf \div k)$ at 100°	q	δ_0	$(kf \div k)$ at 100°	q	δ_0	$(kf \div k)$ at 100°	q	δ_0	$(kf \div k)$ at 100°	q	δ_0	$(kf \div k)$ at 100°
0.01	0	.02	.020	.5	.016	.022	1	.01	.024	2	—		3	—	
0.10	0	—		.5		—	1	—		2	.010	.121	3	—	
0.20	0	—		.5		—	1	—		2	.033	.221	3	.03	.242
0.30	0	—		.5		—	1	—		2	.056	.321	3	.06	.342
0.40	0	—		.5		—	1	—		2	.079	.421	3	.09	.442

PALLADIUM

$K_1 = 3.52, K_2 = 0, C_2 = 0$

Palladium is so like nickel in its Thomson effect that table 2 will serve for it.

TABLE 3: PLATINUM.

$K_1 = 2.67, K_2 = 0, C_2 = 0$

C, or $(kf \div k)$, at 0°	$C_1 = 78 \times 10^{-6}$			$C_1 = 89 \times 10^{-6}$			$C_1 = 104 \times 10^{-6}$			$C_1 = 156 \times 10^{-6}$			$C_1 = 312 \times 10^{-6}$		
	q	δ_0	$(kf \div k)$ at 100°	q	δ_0	$(kf \div k)$ at 100°	q	δ_0	$(kf \div k)$ at 100°	q	δ_0	$(kf \div k)$ at 100°	q	δ_0	$(kf \div k)$ at 100°
0.01	0	.02	.018	.5	.01	.019	1	.01	0.20	2	—		3	—	
0.10	0	—		.5	—		1	—		2	.02	.116	3	.01	.131
0.20	0	—		.5	—		1	—		2	.05	.216	3	.05	.231
0.30	0	—		.5	—		1	—		2	.08	.316	3	.09	.331
0.40	0	—		.5	—		1	—		2	.11	.416	3	.13	.431

TABLE 4: TIN.

$K_1 = 0.134, K_2 = 0, C_2 = 0$

C, or $(kf \div k)$, at 0°	$C_1 = 392 \times 10^{-8}$			$C_1 = 448 \times 10^{-8}$			$C_1 = 520 \times 10^{-8}$			$C_1 = 780 \times 10^{-8}$			$C_1 = 1560 \times 10^{-8}$		
	q	δ_0	$(kf \div k)$ at 100°	q	δ_0	$(kf \div k)$ at 100°	q	δ_0	$(kf \div k)$ at 100°	q	δ_0	$(kf \div k)$ at 100°	q	δ_0	$(kf \div k)$ at 100°
0.01	0	—		.5	—		1	—		2	0.05	.011	3	0.05	.012
0.10	0	—		.5	—		1	—		2	0.52	.101	3	0.79	.102
0.20	0	—		.5	—		1	—		2	1.04	.201	3	1.62	.202
0.30	0	—		.5	—		1	—		2	1.56	.301	3	2.45	.302
0.40	0	—		.5	—		1	—		2	2.08	.401	3	3.28	.402

TABLE 5: ZINC.

$K_1 = 0.99, K_2 = 0, C_2 = 0$

C, or $(k_f \div k)$, at 0°	$C_1 = 29 \times 10^{-6}$			$C_1 = 33 \times 10^{-6}$			$C_1 = 38 \times 10^{-6}$			$C_1 = 58 \times 10^{-6}$			$C_1 = 116 \times 10^{-6}$		
	q	δ_0	$(k_f \div k)$ at 100°	q	δ_0	$(k_f \div k)$ at 100°	q	δ_0	$(k_f \div k)$ at 100°	q	δ_0	$(k_f \div k)$ at 100°	q	δ_0	$(k_f \div k)$ at 100°
0.01	0	—		.5	—		1	.0	.014	2	—		3	—	
0.10	0	—		.5	—		1	—		2	.06	.106	3	.07	.112
0.20	0	—		.5	—		1	—		2	.13	.206	3	.18	.212
0.30	0	—		.5	—		1	—		2	.20	.306	3	.30	.312
0.40	0	—		.5	—		1	—		2	.28	.406	3	.41	.412

Second Group: Metals for which $K_1 < 0$ and $K_2 = 0$

TABLE 6: BISMUTH.

$K_1 = —3.2, K_2 = 0, C_2 = 0$

C, or $(k_f \div k)$, at 0°	$C_1 = —94 \times 10^{-6}$			$C_1 = —107 \times 10^{-6}$			$C_1 = —125 \times 10^{-6}$		
	q	δ_0	$(k_f \div k)$ at 100°	q	δ_0	$(k_f \div k)$ at 100°	q	δ_0	$(k_f \div k)$ at 100°
0.01	0	0.05	0.001	0.5	0.03	0.001	1	0.01	0.000
0.10	0	0.17	0.091	0.5	0.10	0.089	1	0.04	0.087
0.20	0	0.31	0.191	0.5	0.18	0.189	1	0.08	0.187
0.40	0	0.58	0.391	0.5	0.34	0.389	1	0.14	0.387
0.60	0	0.85	0.591	0.5	0.50	0.589	1	0.21	0.587

TABLE 7: CADMIUM.

$K_1 = —32.4, K_2 = 0, C_2 = 0$

C, or $(k_f \div k)$, at 0°	$C_1 = —94 \times 10^{-6}$			$C_1 = —107 \times 10^{-6}$			$C_1 = —125 \times 10^{-6}$		
	q	δ_0	$(k_f \div k)$ at 100°	q	δ_0	$(k_f \div k)$ at 100°	q	δ_0	$(k_f \div k)$ at 100°
0.01	0	0.036		0.5	0.024	—	1	0.012	—
0.10	0	0.049	0.006	0.5	0.032	—	1	0.015	—
0.20	0	0.062	0.106	0.5	0.040	0.093	1	0.019	0.075
0.40	0	0.090	0.306	0.5	0.055	0.293	1	0.026	0.275

TABLE 8: COPPER.

$K_1 = —0.966, K_2 = 0, C_2 = 0$

C, or $(k_f \div k)$, at 0°	$C_1 = —28 \times 10^{-6}$			$C_1 = —35 \times 10^{-6}$			$C_1 = —38 \times 10^{-6}$		
	q	δ_0	$(k_f \div k)$ at 100°	q	δ_0	$(k_f \div k)$ at 100°	q	δ_0	$(k_f \div k)$ at 100
0.01	0	0.08	0.007	0.5	0.05	0.006	1	0.02	0.006
0.10	0	0.49	0.097	0.5	0.26	0.096	1	0.12	0.096
0.20	0	0.95	0.197	0.5	0.48	0.196	1	0.24	0.196
0.40	0	1.87	0.397	0.5	0.92	0.396	1	0.48	0.396

TABLE 9: MAGNESIUM.[1]

$K_1 = -0.008,\ K_2 = 0,\ C_2 = 0$

C, or $(k_f \div k)$, at 0°	$C_1 = -23 \times 10^{-6}$			$C_1 = -27 \times 10^{-6}$			$C_1 = -31 \times 10^{-6}$		
	q	δ_o	$(k_f \div k)$ at 100°	q	δ_o	$(k_f \div k)$ at 100°	q	δ_o	$(k_f \div k)$ at 100°
0.001	0	0.51	0.001	0.5	0.34	0.001	1	0.15	0.001
0.01	0	5.5	0.01	0.5	3.2	0.01	1	1.4	0.01
0.05	0	27.5	0.05	0.5	16.0	0.05	1	7.0	0.05

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[1] The Thomson effect in magnesium was very small, and the values of δ_o indicated for this metal are exceptionally large. Bridgman, however, describes the specimen used as of commercial quality, and thinks it was impure.

TABLE 10: SILVER.

$K_1 = -0.864,\ K_2 = 0,\ C_2 = 0$

C, or $(k_f \div k)$, at 0°	$C = -25 \times 10^{-6}$			$C_1 = -29 \times 10^{-6}$			$C_1 = -34 \times 10^{-6}$		
	q	δ_o	$(k_f \div k)$ at 100°	q	δ_o	$(k_f \div k)$ at 100°	q	δ_o	$(k_f \div k)$ at 100°
0.01	0	0.09	0.007	0.5	0.05	0.007	1	0.02	0.007
0.10	0	0.54	0.097	0.5	0.32	0.097	1	0.14	0.097
0.20	0	1.04	0.197	0.5	0.61	0.197	1	0.26	0.197
0.40	0	2.04	0.397	0.5	1.19	0.397	1	0.50	0.397

TABLE 11: TUNGSTEN.

$K_1 = -3.41,\ K_2 = 0,\ C_2 = 0$

C, or $(k_f \div k)$, at 0°	$C_1 = -100 \times 10^{-6}$			$C_1 = -114 \times 10^{-6}$			$C_1 = -130 \times 10^{-6}$		
	q	δ_o	$(k_f \div k)$ at 100°	q	δ_o	$(k_f \div k)$ at 100°	q	δ_o	$(k_f \div k)$ at 100°
0.01	0	0.05	0.000	0.5	0.03	0.000	1	0.02	0.000
0.10	0	0.16	0.090	0.5	0.10	0.089	1	0.05	0.087
0.20	0	0.29	0.190	0.5	0.17	0.189	1	0.08	0.187
0.40	0	0.55	0.390	0.5	0.32	0.389	1	0.14	0.387

Third Group: Metals for which $K_1 > 0$ and $K_2 > 0$

TABLE 12: IRON.

$K_1 = 1.78,\ K_2 = 0.0516$

C, or $(k_f \div k)$, at 100°	$C_2 = 93 \times 10^{-8}$				$C_2 = 100 \times 10^{-8}$				$C_2 = 110 \times 10^{-8}$			
	q	δ_0	$C_1 \times 10^{-6}$	$(k_f \div k)$ at 100°	q	δ_0	$C_1 \times 10^{-6}$	$(k_f \div k)$ at 100°	q	δ_0	$C_1 \times 10^{-6}$	$(k_f \div k)$ at 100°
0.001	0	.015	66	.017	.5	.010	70	.018	1	.005	77	.020
0.01	0	.019	44	.024	.5	.012	57	.026	1	.006	68	.028
0.05	0	.031	—21	.057	.5	.019	10	.061	1	.009	43	.065
0.10	0	.042	—81	.101	.5	.027	—43	.106	1	.013	9	.112
0.15	0	.052	—135	.146	.5	.034	—90	.151	1	.017	—26	.158
0.20	0	.060	—178	.192	.5	.039	—123	.198	1	.020	—52	.205
0.25	0	.068	—221	.237	.5	.045	—163	.244	1	.022	—69	.254

TABLE 13: THALLIUN.

$K_1 = 0.268,\ K_2 = 0.00336$

C, or $(k_f \div k)$, at 0°	$C_2 = 6 \times 10^{-8}$				$C_2 = 6.5 \times 10^{-8}$				$C_2 = 7.1 \times 10^{-8}$			
	q	δ_0	$C_1 \times 10^{-6}$	$(k_f \div k)$ at 100°	q	δ_0	$C_1 \times 10^{-6}$	$(k_f \div k)$ at 100°	q	δ_0	$C_1 \times 10^{-6}$	$(k_f \div k)$ at 100°
0.001	0	.020	7	.002	.5	.010	10	.003	1	.006	10	.003
0.01	0	.048	—3	.011	.5	.034	—1	.011	1	.016	5	.011
0.05	0	.130	—32	.047	.5	.090	—25	.048	1	.050	—14	.049
0.10	0	.186	—51	.096	.5	.130	—42	.096	1	.080	—31	.098
0.15	0	.227	—65	.144	.5	.160	—55	.145	1	.096	—40	.147
0.20	0	.262	—78	.193	.5	.186	—66	.194	1	.105	—45	.196
0.30	0	.324	—99	.291	.5	.220	—81	.293	1	.135	—62	.295

Fourth Group: Metals for which $K_1 < 0$ and $R_2 > 0$

TABLE 14: ALUMINUM.

$K_1 = —0.016,\ K_2 = 0.006$

C, or $(k_f \div k)$, at 0°	$C_2 = 11 \times 10^{-8}$				$C_2 = 12 \times 10^{-8}$				$C_2 = 13 \times 10^{-8}$			
	q	δ_0	$C_1 \times 10^{-6}$	$(k_f \div k)$ at 100°	q	δ_0	$C_1 \times 10^{-6}$	$(k_f \div k)$ at 100°	q	δ_0	$C_1 \times 10^{-6}$	$(k_f \div k)$ at 100°
0.001	0	.020	—1	.002	.5	.012	—1	.002	1	.006	—1	.002
0.01	0	.036	—11	.010	.5	.022	—9	.010	1	.012	—7	.011
0.05	0	.086	—41	.047	.5	.058	—37	.048	1	.032	—30	.048
0.10	0	.125	—64	.095	.5	.088	—61	.095	1	.047	—46	.097
0.20	0	.180	—97	.191	.5	.126	—91	.192	1	.068	—69	.194
0.30	0	.226	—125	.289	.5	.155	—114	.290	1	.088	—91	.292

TABLE 15: GOLD.

$K_1 = -0.934, K_2 = 0.001$

C, or $(kf \div k)$, at 0°	$C_2 = 1.8 \times 10^{-8}$				$C_2 = 2.0 \times 10^{-8}$				$C_2 = 2.1 \times 10^{-8}$			
	q	δ_0	$C_1 \times 10^{-6}$	$(kf \div k)$ at 100°	q	δ_0	$C_1 \times 10^{-6}$	$(kf \div k)$ at 100°	q	δ_0	$C_1 \times 10^{-6}$	$(kf \div k)$ at 100°
0.001	0	.032	—29	—	.5	.024	—33	—	1	.011	—38	—
0.01	0	.051	—31	.007	.5	.040	—35	.007	1	.018	—39	.006
0.05	0	.122	—38	.046	.5	.096	—42	.046	1	.050	—44	.046
0.10	0	.188	—45	.096	.5	.158	—50	.095	1	.083	—50	.095
0.20	0	.292	—56	.195	.5	.256	—62	.194	1	.136	—58	.194
0.25	0	.334	—60	.244	.5	.294	—77	.293	1	.158	—64	.244

TABLE 16: MOLYBDENUM.

$K_1 = -4.334, K_2 = 0.015$

C, or $(kf \div k)$, at 0°	$C_2 = 27 \times 10^{-8}$				$C_2 = 29 \times 10^{-8}$				$C_2 = 32 \times 10^{-8}$			
	q	δ_0	$C_1 \times 10^{-6}$	$(kf \div k)$ at 100°	q	δ_0	$C_1 \times 10^{-6}$	$(kf \div k)$ at 100°	q	δ_0	$C_1 \times 10^{-6}$	$(kf \div k)$ at 100°
0.001	0	.025	—138		.5	.017	—155		1	.008	—175	
0.01	0	.030	—146		.5	.020	—161		1	.010	—180	
0.05	0	.047	—173	.035	.5	.030	—180	.035	1	.015	—192	.044
0.10	0	.064	—197	.083	.5	.042	—204	.083	1	.020	—205	.083
0.20	0	.094	—220	.181	.5	.061	—240	.179	1	.030	—230	.180

Comments

Lead does not appear in these tables, for the reason that σ for this metal is so small that we have no formula for it. Zero value for σ could be accounted for, according to the principles of this paper, in either of two ways:

First, as equation (8) shows, $(k_f \div k)$ might be zero.

Second, if in equation (8) we make $(kf \div k)$ a constant, of whatever value,[4] and is zero if $n \propto T^{1.5}$

These tables, like the experimental values of σ by means of which they are made, have no great pretension to accuracy. For example, it is doubtful whether much confidence can be placed in the small value of K_2, very nearly 0.001, found by Bridgman for gold. Yet the effect of neglecting this value is considerable. Thus, if we call $K_2 = 0$ for gold, we get

TABLE 15 *bis*: GOLD.

$K_1 = -0.934$, K_2 taken to be 0, and $C_2 = 0$

C, or $(k_f \div k)$, at 0°	$C_1 = -27 \times 10^{-6}$			$C_1 = -31 \times 10^{-6}$			$C = -36 \times 10^{-6}$		
	q	δ_0	$(k_f \div k)$ at 100°	q	δ_0	$(k_f \div k)$ at 100°	q	δ_0	$(k_f \div k)$ at 100°
0.01	0	0.08	0.007	0.5	0.05	0.007	1	0.02	0.006
0.10	0	0.51	0.097	0.5	0.30	0.097	1	0.13	0.096
0.20	0	0.99	0.197	0.5	0.58	0.197	1	0.25	0.196
0.40	0	1.95	0.397	0.5	1.14	0.397	1	0.49	0.396

Comparison of this with table 15 shows that ignoring the value of K_2, small as it is, makes a good deal of difference in the values of δ_0, though comparative little in the values of $(k_f \div k)$ at 100°, as calculated by means of equation (10).

Uncertain as the values of $(k_f \div k)$ are, there is little room for doubt in most cases, if my theory is substantially sound, as to the direction of change of these values when the temperature is raised. Accordingly I have divided all the metals represented by the preceding tables into two groups, in the first of which $(k_f \div k)$ is greater at 100° than at 0°, while in the second group the ratio in question is greater at 0°.

For most of these metals Bridgman has determined the pressure-coefficient of resistance at 0° and 100°. For magnesium the value at 0° only was found; for bismuth the highest temperature used was 75°. For all of the metals here considered except bismuth the coefficient in question is negative—that is, the resistance decreases with increase of pressure—but for bismuth it is positive. I shall make use of what Bridgman calls the *average* pressure-coefficient, the average value of the coefficient through a range of pressure from 0 kgm. to 12000 kgm. per square centimeter. I shall let π_0 represent the value of this coefficient at 0°, and π_{100} the value at 100°.

In accordance with what has been said in the opening paragraphs of this paper we should, other things being equal, expect π to decrease, numerically, with increase of $(k_f \div k)$ in metals for which π is negative, and to increase with increase of $(k_f \div k)$ in metals for which π is positive. Accordingly we might expect $(\pi_{100} - \pi_0) \div \pi_0$ to be, in general, a negative quantity for metals in which $(k_f \div k)$ increases with rise of temperature, and a positive quantity for metals in which $(k_f \div k)$ decreases with rise of temperature—bismuth of course requiring exceptional consideration. The table given below enables us to test the validity of this expectation.

TABLE 17

(*A*)

METALS IN WHICH $(kf \div k)$ INCREASES WITH RISE OF TEMPERATURE

	π_0	π_{100}	$(\pi_{100} - \pi_0) \div \pi_0$
Co	-0.0_6873	-0.0_6726	-16.9%
Ni	-0.0_5147	-0.0_5158	$+7.5\%$
Pd	-0.0_5190	-0.0_5186	-2.1%
Pt	-0.0_5187	-0.0_5184	-1.6%
St	-0.0_5920	-0.0_5951	$+3.4\%$
Zn	-0.0_5470	-0.0_5454	-3.4%
		Mean	-2.2%

(*B*)

METALS IN WHICH $(kf \div k)$ DECREASES WITH INCREASE OF TEMPERATURE

	π_0	π_{100}	$(\pi_{100} - \pi_0) \div \pi_0$
Ag	-0.0_5333	-0.0_5336	$+0.9\%$
Al	-0.0_5382	-0.0_5377	-1.3%
An	-0.0_5287	-0.0_5292	$+1.7\%$
Bi[1]	$+0.0_4223$	$+0.0_4202$	$+9.4\%$
Cd	-0.0_5894	-0.0_5927	$+3.7\%$
Cu	-0.0_5183	-0.0_5177	-3.3%
Mg[2]	-0.0_555		
Mo	-0.0_5129	-0.0_5127	-1.6%
Ta	-0.0_4115	-0.0_4123	$+7.0\%$
W	-0.0_5123	-0.0_5126	$+2.4\%$
		Mean	$+2.1\%$

[1] For bismuth, because π is positive and the highest temperature for π was 75°, we use $(\pi_0 - \pi_{75}) \div \pi_0$.

[2] Bridgman did not find π_{100} for magnesium.

Iron does not appear in this table, for the reason that, as table 12 shows, it should go into Section (*A*) under some conditions, but into Section (*B*) under other conditions. Its value of $(\pi_{100} - \pi_0) \div \pi_0$ is about 4%. This value, if put into Section (*A*), would make the mean value there -1.3%; put into Section (*B*), it would make the mean there $+ 2.3\%$. If $(kf \div k)$ for iron is greater than 10% at 0°*C*., which seems likely, iron belongs to Section (*B*).

Thallium and aluminium should go into Section (*A*) if $(k_f \div k)$ in them is less than 1% at 0°*C;* but this is improbable.

It is to be observed that for each section of table 17 the average value of $(\pi_{100} - \pi_0) \div \pi_0$ comes out with the sign it should have according to the predictions of the dual theory of electric conduction, as used in this paper, a *minus* sign for Section (*A*) and a *plus* sign for Section (*B*). This can hardly be pure accident.

Under Hypothesis (*B*)

All of the equations and all the tables of this paper, thus far, are based upon or are consistent with "hypothesis (*A*)." If hypothesis (*B*) is adopted instead, equation (8) is changed in the manner already described and the result in equations (13) to (19) is to replace 1.5 by 2, 4 by 4.5, and 6.5 by 7, in the parenthesis $(1.5 - q)$, $(4 - q)$ and $(6.5 - q)$.

The resulting tables, for cobalt and bismuth, the only metals for which the calculation has been made, are given in Tables 18 and 19 (below).

By comparison of these two tables with the corresponding ones obtained by the use of hypothesis (*A*) we see that no radical difference in the results comes from the substitution of one hypothesis for the other. Hypothesis (*B*) seems to give, other things being equal, somewhat larger values of δ_0 than hypothesis (*A*).

TABLE 18: COBALT.

$K = 7.8, K_2 = 0, C_2 = 0$

C, or $(k_f \div k)$, at 0°	$C_1 = 202 \times 10^{-6}$			$C_1 = 230 \times 10^{-6}$			$C_1 = 260 \times 10^{-6}$			$C_1 = 303 \times 10^{-6}$		
	q	δ_0	$(k_f \div k)$ at 100°	q	δ_0	$(k_f \div k)$ at 100°	q	δ_0	$(k_f \div k)$ at 100°	q	δ_0	$(k_f \div k)$ at 100°
0.01	0	.038	.030	0.5	.029	.033		.020	.036	1.5	.010	.040
0.05	0	.004	.070	0.5	.007	.073		.007	.076	1.5	.005	.080
0.10	0	—		0.5	—			—		1.5	—	

C, or $(k_f \div k)$, at 0°	$C_1 = 455 \times 10^{-6}$			$C_1 = 607 \times 10^{-6}$		
	q	δ_0	$(k_f \div k)$ at 100°	q	δ_0	$(k_f \div k)$ at 100°
0.10	2.5	—		3	—	
0.15	2.5	0.002	0.196	3	—	
0.20	2.5	0.007	0.246	3	0.005	0.261
0.30	2.5	0.016	0.346	3	0.019	0.361
0.40	2.5	0.026	0.446	3	0.032	0.461

TABLE 19: BISMUTH.

$K_1 = -3.2, K_2 = 0, C_2 = 0$

C, or $(k_f \div k)$, at 0_o	$C_1 = -83 \times 10^{-6}$			$C_1 = -93 \times 10^{-6}$			$C_1 = -106 \times 10^{-6}$		
	q	δ_0	$(k_f \div k)$ at 100°	q	δ_0	$(k_f \div k)$ at 100°	q	δ_0	$(k_f \div k)$ at 100°
0.01	0	0.07	0.002	0.5	0.05	0.001	1.5	0.03	0.000
0.10	0	0.25	0.092	0.5	0.17	0.091	1.5	0.10	0.089
0.20	0	0.46	0.192	0.5	0.32	0.191	1.5	0.18	0.189
0.40	0	0.87	0.392	0.5	0.58	0.391	1.5	0.34	0.389
0.60	0	1.28	0.592	0.5	0.84	0.591	1.5	0.50	0.589

In a paper already well advanced I shall undertake to show how far the data obtained in the present paper enable us to go in the way of explaining thermal conduction in the metals here dealt with.

---

[1] Eq. (7) is obtained from eq. (1) of my paper in the *Proceedings of the National Academy of Sciences* for April, 1918, by substituting for μ from eq. (2) of that paper (which equation should have n instead of m) and then making obvious changes.

[2] *Proceedings of the Amer. Acad. of Sciences*, Vol. **53**, No. 4, 1918, pp. 269–386. These values are not regarded by Bridgman as accurate, having been obtained as second derivatives of e. m. f. values, the quantities measured; but taken as a whole they seem to be the best available data for the present purpose.

[3] This assumes that λ'_0 is constant.

[4] When $(k_f \div k)$ is infinite, we have the conditions discussed in my paper "Thermo-electric Diagrams on the P-V Plane," *Proc. Amer. Acad., Boston*, February, 1918.

NOTE ON GEOMETRICAL PRODUCTS

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Communicated by E. H. Moore, January 28, 1920

Geometrical products may be divided into two general classes: those definable in a space of any number of dimensions and those definable only in a space of a definite number of dimensions. To the first class belong the progressive (outer) product of Grassmann and the inner products of Grassmann, Gibbs[1] and Lewis.[2] To the second class belong the regressive product of Grassmann and the cross product of Gibbs (as defined in the Gibbs-Wilson Vector Analysis) and the quaternion multiplications. In this paper we show that there is a series of geometrical products independent of dimensions that may be in a sense considered intermediate between the progressive product and the inner product.

In terms of the units[3] the progressive product is expressed by identities of the form $k_{12}k_{34} = k_{1234}$, the product containing all the units multiplied together. If there are any common units as in $k_{12}k_{14} = 0$, the product is zero. The inner product of Lewis is expressed by identities of the form $k_3.k_{123} = k_{12}$, the common units being cancelled (with certain conventions as to algebraic sign). Unless one factor is entirely contained in the other the result is zero.

If π_1 and π_2 are products of units whose first m factors are the same and in the same order we define as the product $[\pi_1\pi_2]_m$ of index m the result of cancelling those m common units and taking the outer product of the remaining units in the order written. Thus

$$[k_{12}k_{13}]_1 = k_{23}, \quad [k_{123}k_{124}]_2 = k_{34}.$$

If π_1 and π_2 have m units in common but these are not the first m units, π_1 and π_2 are rewritten (with change of sign if necessary as required by the outer multiplication) in such a way that the first m factors are the same, and in the same order, the product is then formed as above, negative signs introduced being included.[4] Thus

$$[k_{12}k_{23}]_1 = -k_{13}, \quad [k_{234}k_{134}]_2 = k_{21} = -k_{12}.$$

If there are either more or less than m units in common, the product is zero. Thus

$$[k_{123}k_{124}]_1 = k[_{123}k_{124}]_3 = 0.$$

The inner product is obtained when the index is equal to the dimension of the lowest factor. Thus

$$[k_1k_{12}]_1 = k_1.k_{12} = -k_2.$$

The outer product occurs when the index is zero. Thus $[k_{12}k_{34}]_0 = k_{1234}$.

To obtain the product of M and N, two homogeneous linear functions of units, they are multiplied distributively, the product of units being found as above. We shall now show that the product of two such quantities is invariant, i. e., that it is independent of the system of units in terms of which it is expressed. We do this by showing that $[MN]_m$ can be expressed in terms of invariant operations on M, N and the identical transformation[5] (dyadic) I.

If $k_1, k_2, \ldots k_n$ are the unit one-vectors, the 1, 1 idemfactor is

$$I_1 = k_1k_1 + k_2k_2 + \ldots + k_nk_n.$$

Using the dot to express inner multiplication we then have

$$\begin{aligned} k_{12}.I_1 &= k_1k_2 - k_2k_1 = -I_1.k_{12}, \\ k_{123}.I_1 &= k_{23}k_1 - k_{13}k_2 + k_{12}k_3, \\ I_1.k_{123} &= k_1k_{23} - k_2k_{13} + k_3k_{12}, \end{aligned}$$

and similar expressions for products with units of higher order. Using these values we find

$$[k_{12}k_{13}]_1 = [(k_{12}.I_1).(I_1.k_{13})]_0,$$

where the subscript 0 indicates that the antecedent and consequent of each dyad in the brackets are multipled according to the (outer) multiplication of index zero and the sum of terms then taken. Similarly we find

$$[k_{123}k_{145}]_1 = [(k_{123}.I_1).(I_1.k_{145})]_0,$$

and generally

$$[\pi_1\pi_2]_1 = [(\pi_1.I_1).(I_1.\pi_2)]_0$$

where π_1 and π_2 are any product of units. Since the operations are all distributive we have then

$$[MN]_1 = [(M.I_1).(I_1.N)_0].$$

Since the dot multiplication, the outer (zero) multiplication and I_1 are invariant it follows that $[MN]_1$ is invariant.

For the product of index 2, we use the 2, 2 idemfactor $I_2 = \Sigma k_{ij}k_{ij}$ with $i<j$. We then find

$$[k_{123}k_{124}]_2 = [(k_{123}.I_2).(I_2.k_{124})]_0$$

and generally

$$[\pi_1\pi_2]_2 = [(\pi_1.I_2).(I_2.\pi_2)]_0,$$

or since the operations are distributive

$$[MN]_2 = [(M.I_2).(I_2.\mathrm{N})]_0$$

which shows the invariance as before.

In general the product of index m is

$$[MN]_m = [(M.I_m).(I_m.N)]_0$$

where

$$I_m = \Sigma k_{12\ldots m}\, k_{12\ldots m}.$$

Let M_p be a simple space of dimension p and N_q a simple space of dimension q.

Theorem I. In order that $[M_pN_q]_m = 0$ it is necessary and sufficient that

$$[M_p(R_m.N_q)]_0 = 0. \qquad (1)$$

where R_m is an arbitrary simple space of dimension m in M_p.

To prove this take the units $k_1, k_2, \ldots k_n$ so that $k_1, k_2, \ldots k_p$ lie in M_p and $R_m = k_{12 \ldots m}$. Then

$$[M_p N_q]_m = \Sigma[(M_p . k_{12 \ldots m})(k_{12 \ldots m} . N_q)]. \qquad (2)$$

In the summation terms containing $k_{p+1}, k_{p+2}, \ldots k_n$ may be omitted since the product with M_p would be zero. The outer product of R_m and (2) is

$$[R_m(M_p N_q)_m]_0 = [R_m(M_p . k_{12 \ldots m})(k_{12 \ldots m} . N_q)]_0 = 0, \qquad (3)$$

all other terms vanishing because they contain repeated factors. Now

$$[R_m(M_p . k_{12 \ldots m})]_0 = [k_{12 \ldots m}(M_p . k_{12 \ldots m})]_0 = M_p.$$

Hence (3) is equivalent to

$$[M_p(R_m . N_q)]_0 = 0$$

which was to be proved.

Theorem II. A necessary and sufficient condition that

$$[M_p N_q]_m = 0 \qquad (4)$$

is that either M_p and N_q intersect in a space of dimensions $\overset{=}{>} m + 1$ or M_p contain a space of dimension $p - m + 1$ completely perpendicular to N_q and N_q contain a space of dimensions $q - m + 1$ completely perpendicular to M_p.

If (4) is satisfied, by (1), either $R_m . N_q = 0$ or the outer product of M_p and $R_m . N_q$ is zero. In the first case R_m contains a perpendicular to N_q and consequently M_p must contain a space perpendicular to N_q which is cut by every R_m. For this it is necessary and sufficient that M_p contain a space of dimensions $p - m + 1$ perpendicular to N_q. In the second case the intersection of M_p and N_q must contain a vector perpendicular to any R_m in M_p. The intersection must then be of such a dimension that it cuts all spaces in M_p of dimension $p - m$. It must then be of dimension $m + 1$.

Theorem III. If

$$[M_p N_q]_m = 0, \ [M_p N_q]_{m+1} \pm 0, \qquad (5)$$

M_p and N_q will intersect in a space of dimension $m + 1$ and so all the products of M_p and N_q of index equal to or less than m will be zero; and, if

$$[M_p N_q]_n = 0, \ [M_p N_q]_{n-1} \pm 0, \qquad (6)$$

M_p will contain a space of dimension $p - m + 1$ completely perpendicular to N_q and so all products of index equal to or greater than n will be zero.

In fact if relations (5) are satisfied, the vanishing of $[M_p N_q]_m$ must be due to M_p and N_q intersecting in a space of dimension $m + 1$; for if there were in M_p a space of dimension $p - m + 1$ perpendicular to N_q, the product $[M_p N_q]_{m+1}$ would also vanish. Hence, in this case all the products of index less than m will also vanish. If (6) is satisfied it must be due to M_p containing a space of dimension $p - m + 1$ completely perpendicular to N_q, for if M_p and N_q intersect in a space of order $m + 1$

the product $[M_pN_q]_{n-1}$ will also be zero, hence, all the products of index less than n will vanish.

It is clear from the theorem just proved that all the products $[M_pN_q]_r$ for which $m<r<n$ will be different from zero and all the products for which $r \overline{<} m$ or $r \overline{>} n$ will be zero.

Theorem IV. If equations (5) are satisfied, $[M_pN_q]_{m+1}$ is the outer product of the parts of M_pN_q completely perpendicular to the intersection.

In this case M_p and N_q intersect in a space of $m + 1$ dimensions. Let $k_1, k_2, \ldots k_{m+1}$ be unit vectors in that space. Then

$$M_p = k_1k_2\ldots k_{m+1}P$$

and

$$N = k_1k_2\ldots k_{m+1}Q$$

where P is the part of M_p perpendicular to $k_1k_1\ldots k_{m+1}$ and Q is the part of N_q completely perpendicular to $k_1k_1\ldots k_{m+1}$. Hence

$$[M_pN_q]_{m+1} = [PQ]_0$$

which proves the theorem.

Theorem V. If (6) is satisfied $[M_pN_q]_{n-1}$ is the outer product of the part of M_p perpendicular N_q and the part of N_q perpendicular to M_p.

Let $l_1, l_2, \ldots . l_{p-n+1}$ be the units in M_p perpendicular to N_q and $j_1, j_2, \ldots . j_{q-n+1}$ the units in N_q perpendicular to M_p. These units are then perpendicular to each other and

$$M_p = Pl_1l_2\ldots . l_{p-n+1}$$
$$N_q = Qj_1j_2\ldots . j_{q-n+1}$$

where P (of dimension $n - 1$) is the part of M_p perpendicular to $l_1l_2\ldots l_{p-n+1}$ and Q is the part of N_q perpendicular to $j_1j_2\ldots . j_{q-n+1}$. Since the units $l_1l_2\ldots . l_{p-n+1}, j_1j_2\ldots . j_{q-n+1}$ are all distinct

$$[M_pN_q]_{n-1} = [PQ]_{n-1}[l_1l_2\ldots . l_{p-n+1}\, j_1j_2\ldots . j_{q-n+1}].$$

Since $[PQ]_{n+1}$ is a number this proves the theorem.

---

[1] Gibbs, Wilson, *Vector Analysis.*

[2] *Proc. Amer. Acad. Arts Sci.*, **46,** 1910 (165–181).

[3] The products here discussed occur as partial products in the multiplication of generalized quaternions. From this point of view they have been treated by Clifford (*Amer. J. Math.*, **1**), Joly (*Proc. Royal Irish Acad.*, **5,** (3)) McAuley (*Proc. R. Soc. Edinburgh*, **28**), Shaw (*Bull. Int. Assoc. Adv. Quaternions,* 1913, (24–27)), and others. In victor algebras of the Grassmann type, for example in the usual victor theory of relativity, they have, however, such a different appearance that we have presented them here from that point of view.

[4] In case the index is equal to the order of one of the factors the sign is taken as in the Lewis inner product.

[5] We used this as the definition of the product of index 1 (star product) in a paper "Rotations in Space of Even Dimensions" to appear in *Proc. Amer. Acad. Arts Sci.*

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CONTENTS

Page

PATHOLOGY.—ENCEPHALITIS AND POLIOMYELITIS..........*By Simon Flexner* 103

PHYSICS.—A STATISTICAL METHOD FOR STUDYING THE RADIATIONS FROM RADIOACTIVE SUBSTANCES AND THE X-RAYS AND ITS APPLICATION TO SOME γ-RAY PROBLEMS..........*By Alois F. Kovarik* 105

PHYSICS.—ON THE NATURE OF THE HEAT PRODUCTION IN A SYSTEM OF PLATINUM BLACK, ALCOHOL AND AIR..........*By L. B. Loeb* 107

PATHOLOGY.—LEPTOSPIRA ICTEROIDES AND YELLOW FEVER..*By Hideyo Noguchi* 110

PHYSIOLOGY.—HUMAN RETINAL ADAPTATION..........*By Selig Hecht* 112

PHYSICS.—A KINEMATICAL INTERPRETATION OF ELECTROMAGNETISM..........*By Leigh Page* 115

PHYSICS.—THE LAWS OF ELASTICO-VISCOUS FLOW. II.....*By A. A. Michelson* 122

ASTRONOMY.—A NOTE ON THE SIMPLE DEVICE FOR INCREASING A PHOTOGRAPHIC POWER OF LARGE TELESCOPES..........*By Harlow Shapley* 127

ANTHROPOLOGY.—ANTHROPOMETRY OF THE SIOUAN TRIBES..*By Louis R. Sullivan* 131

GENETICS.—TRANSMISSION OF EYE-DEFECTS INDUCED IN RABBITS BY MEANS OF LENS-SENSITIZED FOWL-SERUM..........*By M. F. Guyer and E. A. Smith* 134

PATHOLOGY.—ON THE MECHANISM OF FEVER REDUCTION BY DRUGS..........*By Henry G. Barbour and Julian B. Herrmann* 136

PHYSICS.—INFERENCES FROM THE HYPOTHESIS OF DUAL ELECTRIC CONDUCTION; THE THOMSON EFFECT..........*By Edwin H. Hall* 139

MATHEMATICS.—NOTE ON GEOMETRICAL PRODUCTS..........*By C. L. E. Moore and H. B. Phillips* 155

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DIFFERENTIATION BY DEFORMATION

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Communicated by J. M. Clarke, February 24, 1920

Introduction.—The deformation of an igneous mass during crystallization, with consequent separation of liquid from crystals, has frequently been suggested as a cause of variation of igneous rocks but the suggestion has apparently never received as detailed consideration as it warrants. That variation would result is obvious, but the full extent of its importance will not be realized unless one has an adequate picture of the far-reaching consequences that may follow from the separation of crystals and mother liquor. In another paper the writer has demonstrated the consequences of such separation in artificial melts and applied the results to natural magmas.[1] Emphasis was there placed upon gravity as a means of separation of crystals from liquid but the chemical consequences are identical whatever the means of separation. In certain respects, however, the consequences of separation by deformation may be distinctive and to these attention is directed in the present paper.[1]

Discontinuous differentiation.—Discontinuous variations, in particular, are not normally to be expected as a result of gravitative accumulation of crystals but discontinuity would appear to be a very likely consequence of deformation. This is especially true at a late stage of crystallization when deformation may cause a crushing of the crystal mesh with consequent closer packing of the crystals and onward movement of the interstitial liquid. At first thought it might seem that the liquid could not move without carrying the crystals with it but it may be pointed out that the break down of a crystal mesh is likely to be progressive and any area breaking down will in such circumstances be bordered by an area not yet affected. The residual liquid is, therefore, free to move into the interstices of the unaffected portion of the mesh and to drive ahead of it the liquid already there which may become a separate intrusive body or a distinctive portion of the same intrusive and of late consolidation. Elsewhere the writer has offered the suggestion that the common association of gabbro and granophyre is frequently capable of interpretation in this manner and perhaps many other associations particularly where the differentiation is markedly discontinuous both chemically and spatially.

Monomineralic types as members of composite intrusives.—Differentiation as a result of settling of crystals and as a result of deformation are by no means mutually exclusive; indeed, it seems probable that in one case, in particular, deformation may step in to bring to completion a task begun by gravitative accumulation, namely, in the case of the production of monomineralic types of excessive purity. For anorthosites, by way of example, it is probable that, besides the sorting of plagioclase crystals by gravitative action, a squeezing out of the interstitial liquid may have contributed to their extreme purity in many cases. If the squeezing continued after a high degree of compaction of crystals had been obtained a clastic structure might be developed and the common protoclastic structure of anorthosite may perhaps be interpreted as evidence that a squeezing out of residual liquid has played an essential part in its production.

Monomineralic types as simple "intrusives."—Peridotite as a sill-like "intrusive" would appear to be capable of production by a somewhat similar combination of crystal settling and deformation. If the intrusive material was originally simply basaltic, if in early stages of crystallization the settling of olivine crystals to the floor was pronounced and if at this time deformation occurred such that in certain places the roof was down-warped the result might be that the whole width of the sill in these places would be occupied by peridotitic material. The down-warping of the roof at this early stage of crystallization might not only remove the supernatant liquid from above the accumulating olivine crystals but might be of sufficient vigor that the interstitial liquid of the peridotite should be largely expressed and a mass of typical dunite result. The mass of peridotite or dunite so formed would have all the ear-marks of an ordinary intrusive sheet and there would be no internal, textural evidence that it had not crystallized from a molten mass of sensibly its own composition though no peridotitic or dunitic liquid ever existed in that region. The formation of this peridotitic differentiate connotes the possibility, indeed the probability, of a complementary granitic differentiate which may be represented as one member of a composite sill in those parts of the sheet that have been widened during the deforming action. The association of peridotitic intrusives with composite intrusives of this type is rather common and merits examination on the part of field-workers with the above suggestion as to origin in mind. In the asbestos region of Quebec the peridotite (now serpentine) masses are described by those best acquainted with them in the field as synclinal sheets.[2] This designation together with the association of diabases, diabase breccia, and granitic intrusives, suggests that the region may furnish a concrete example of the origin of a sheet-like peridotite "intrusive" after the manner described above.

It seems possible that even a dike-like mass of basaltic magma if deformed at an early stage of crystallization, might give rise, in the portions that are constricted by deformation to a peridotitic mass, though probably not as readily as in the case of a sheet. The mutual approach of the walls necessitates a movement of liquid out of the intervening space but close to the walls there would be a certain amount of drag of the crystals resulting in some movement of liquid relative to crystals. The two marginal regions of concentration of crystals might be brought together and even crystal packing and the expressing of liquid might occur if the constriction were carried far enough. Thus might result a dike which, intersected at this particular point, would be simply a peridotite. It would, therefore, seem necessary to include the above action among those to be considered as capable of producing dike "intrusions" of peridotite.

Complementary dikes.—Those minor but rather constant associates of intrusive masses, the complementary dikes, aplitic and lamprophyric, may perhaps be produced by action of the same general nature as that we are now considering. In very late stages of crystallization residual liquid may be drained into clefts in the mass and there suffer differentiation as a result of local pinching and swelling during crystallization. This is particularly likely to have important effects in such small bodies. Complementary units would be formed that might readily have a composition not matched in large intrusive masses.

Primary banding.—A further effect of deformation during crystallization appears to be that which finds its expression in primary banding. Shearing of a crystalline mesh, particularly when it is still weakly knit together and permits passage of liquid through it with great freedom, may develop a feature of the same general nature as a shear zone developed in solid rocks. Lenticular openings may be formed that instantly fill with liquid from the interstices of the mesh and repetition of the action may give rise to a banding, properly oriented with respect to the shearing stress, and showing approximately that contrast between bands that obtained between liquid and crystals at the time of the action. The crystals that became detached during the shearing would naturally become aligned in the liquid filling the lenses so that fluxion structure would be a natural accompaniment. Moreover, in the larger lenses developed a gravitative sorting of these detached crystals would take place under particularly favorable conditions and bands showing the extreme contrast of monomineralic types might thus be formed. The banded gabbros exhibit features that appear to match the results of the postulated action. Other rock types should be subject to similar action, but are probably not as likely to give such obvious contrasts in the bands as are the gabbros.

Alkaline rocks.—In rare instances rocks are found that have empty interstices and to these the term miarolitic is applied. It is usually assumed in explanation that the rock had crystallized in such a way as to leave only gas as interstitial material. This is, however, not the only possibility. It is not difficult to conceive of a distribution of forces in the heterogeneous surroundings of an igneous mass such that locally the liquid might be sucked out of a crystalline mesh, which would acquire a miarolitic texture, temporarily, at least. The consequences of this action would normally differ in no essential particular from those produced by simple squeezing out of the liquid. It is probable, however, that this sucking action could be operative at a very late stage of crystallization when squeezing out of liquid may be impossible or at least very unlikely. The kinds of liquid that form some of the alkaline rocks may perhaps be removed in this manner from quartz-mica rocks at a very late stage of their crystallization and possibly only such action can effect the separation of these liquids. In certain regions of the earth's crust where tangential extension is the dominant expression of the forces acting (Atlantic structures) the development of alkaline rocks might be a prominent feature though the conditions requisite to their formation would undoubtedly occur locally elsewhere.

[1] The Later Stages of the Evolution of the Igneous Rocks, *J. Geol.*, **23**, 1915, Suppl., pp. 1–91.

[2] Howie, Robt., Summary *Rept. Geol. Survey Can.*, 1916, Coleraine Map Sheet insert, p. 228; also Knox, G. K., *Ibid.*, p. 229, et. seq. Reference is made to granitic "batholiths" in the anticlines, p. 232.

THE EVIDENCE FOR THE LINEAR ORDER OF THE GENES

BY T. H. MORGAN, A. H. STURTEVANT AND C. B. BRIDGE

DEPARTMENT OF ZOÖLOGY, COLUMBIA UNIVERSITY

Communicated February 25, 1920

Despite Castle's dictum that we "have failed in two different attempts to establish the linear theory in the case of the three genes yellow, white and bifid," we are bold enough to maintain that the data furnished, and still furnish, the proof called for. We wish to call attention to the fact that in his last paper Castle ignores our proof of the linear order that is furnished by building up the whole chromosome (or even large sections of it) by "distances" so short that no double cross-over classes appear.

Castle asserts that we have rejected "nearly 99 per cent" of our data in the construction of the yellow, white, bifid section of the map. As a matter of fact no data have been omitted. In this case, as always, the order of the loci was determined by experiments that involved all of these loci at once. The order having been established the next step was to determine the relative distance between the loci by the use of all the available data. We have emphasized in our reply to Castle that there are several sources of variability in linkage values such as age, temperature, genetic factors. The variability due to these causes far outweighs that due to random sampling. It is, therefore, inadmissible to compare data from different experiments, however extensive, in establishing the order of the loci, or in testing the validity of the hypothesis of linear arrangement. For such purposes, as we have already pointed out, it is essential to use data in which all the loci are followed at once.

The purpose of the maps is twofold: first, to give the sequence of the loci, and secondly, to indicate, by the relative spacing of the loci, the cross-over values most likely to coincide with the results of future experiments. For the latter purpose it is evident that mean values are needed. These can best be obtained by using all the data. And this, as stated, has been the method used for determining the distances on the maps published. Castle has used all the data for criticizing the sequence of the loci on our maps, and has accused us of using only the three-point data for determining the map-distances. Obviously this is an inversion of the correct relationship.

Even when all the available data are used in constructing the map, these data should themselves be subjected to all the controls that it is possible to apply to them, as everyone familiar with the treatment of rough data will understand. For example: It is well known that under certain conditions, such as crowding, some mutant classes run behind their expected ratios. Under favorable conditions of culture, normal ratios are obtained. Clearly, data of the first kind unless corrected are unsuited for determining distances on the chromosome map; and the larger the amount of such data, the larger would be the discrepancies in a map based upon such data used with normal data. We have shown how the aberrant ratios due to the inviability of given classes can be balanced by the use of converse crosses. Further corrections and weighings are also desirable in order that no one class of data shall unduly prejudice the result. The methods for making such corrections have been briefly outlined in Carnegie Publication No. 237, 1916, and given more fully in Carnegie Publication No. 278, 1919.

Besides environmental disturbances of the kind just described there are factors that are known to modify crossing-over. Obviously such data should be eliminated from material from which a normal chromosome map is to be made. That Castle himself admits the validity of elimination of such data is shown by the fact that he deliberately rejected the data involving lethal 2 which we had shown to cause aberrant linkage relations. If he had used these data, all of his long wires would have been bent. Castle has set up the claim that one of the advantages of his model is that such a bending of a wire in the case of white forked revealed the fact that the experimental value of this interval was too great. The same fact would have been "revealed" on any system by a comparison of the white forked value with the values for white rudimentary and white bar, both of which were based on larger numbers than white forked itself.

Castle's discussion of interference in connection with yellow white bifid (p. 503) shows his failure to understand interference, and hence his criticism misses the mark. It need only be stated that Castle was wrong in supposing that the cross-overs between yellow and white should be subtracted from the total before the white bifid cross-over value is calculated.

If, as Castle states, he cannot conceive of a mechanism "which would tie two genes together in such a way that they will subsequently separate from each other oftener than they will remain together, yet this is what the idea of cross-overs in excess of 50 per cent amounts to," it would seem to follow that he has not really understood the mechanism that we have described, and which he has attacked; for, whether such a mechanism really exists or not, it is nevertheless a conceivable mechanical device that could do just this thing.

We have left to Dr. Metz the opportunity to answer Castle's criticism relating to D. virilis.

To sum up: we believe that we have met all the pertinent criticisms that Castle has brought forward of our methods and conclusions, and that he has failed to meet our criticism of his three dimensional model.

THE ARRANGEMENT OF GENES IN DROSOPHILA VIRILIS

By Charles W. Metz

Station for Experimental Evolution, Carnegie Institution of Washington

Communicated by T. H. Morgan, March 6, 1920

In connection with his general attack on the theory of the linear arrangement of genes Castle[1] has questioned my use of this theory in explaining the genetical results obtained in Drosophila virilis. I did not consider it necessary to reply to this criticism because Castle's general position was shown by Sturtevant, Bridges and Morgan[2] to be untenable as regards Drosophila melanogaster, with which D. virilis agrees in its mode of inheritance. Subsequently, however, Castle has reaffirmed his belief in the superiority of his hypothesis over that of linear arrangement[3] and has apparently misconstrued the silence on my part with respect to Drosophila virilis. This would seem to call for a brief reply.

From my paper on eight sex-linked characters in D. virilis[4] Castle concluded that the genes dealt with could not be arranged in a linear series, and by applying his three dimensional hypothesis to the case he made certain predictions (*b*, p. 36) which he now wishes used as a test of his hypothesis. His general arguments in this case are the same as those he used previously in reference to Drosophila melanogaster, and since they are being treated by those toward whom they were first directed, I will avoid repetition by confining myself to his predictions. These apply to four undetermined cross-over values in D. virilis, namely, magenta-hairy, glazed-rugose, frayed-forked and frayed-glazed.

It should be pointed out first that in making these predictions Castle apparently overlooked the statement in my paper (pp. 113 and 125) that the frayed stock had been lost shortly after it was obtained. The absence of this stock makes it impossible to test the latter two of his predictions. The other two predictions are also such that they cannot be tested at present; the first because of the loss of hairy stock through sterility, and the second because of the sterility of rugose-glazed hybrids.[5] Had it not been for these practical obstacles the values would not have been left undetermined in my original paper.

The matter need not be dropped here, however. It should be noted that, in any event, tests of the predictions would have been futile, for the two hypotheses may be compared just as well by means of known values as by testing unknown ones. In addition, the very nature of the predictions themselves prevents their being used for critical tests, because they are so constructed that, on either view, they must be fulfilled if my data give sufficiently accurate ratios.[6] There is no alternative. This being the case it becomes a question of ascertaining the accuracy of my data[7] or the validity of the assumptions underlying Castle's predictions.

An illustration may be used to amplify this point. Castle's last two predictions apply to the gene for frayed. My data on frayed (pp. 112 and 126) as tabulated by Castle would locate the gene 1.3 units from yellow and 18.6 units from vesiculated. Castle's own calculation of the value yellow-vesticulated is 17.4. This would put the three genes in almost exactly a straight line (1.3 + 17.4 = 18.7, as compared with 18.6). These are the only data available for the direct determination of the location of frayed, but so far as they go they conform to the linear hypothesis. Castle's predictions relate to the cross-over values that should be given by frayed and forked, and by frayed and glazed. The former should be "between 39 and 41" and the latter "between 43 and 46." The predicted values are, of course, to be calculated entirely on the basis of single cross-overs, as all values are determined on his system.

These predictions are evidently based on the cross-over values given by yellow and forked, and yellow and glazed, respectively. By leaving out of account the double cross-overs, Castle calculated the latter values from my data as 40 and 44.5. Since frayed is approximately one unit from yellow it should give within approximately one unit of the same cross-over values as yellow. This principle would apply on either hypothesis. It is evident, then, that by calculating the value frayed-forked in the same manner that yellow-forked was calculated—namely, by using only single cross-overs—the result would have to be within approximately one unit of 40, if crossing over is a consistent process. The same principle applies to the frayed-glazed relation. Both predictions would have to be fulfilled (within the limits of experimental error), under these conditions. But this does not in any way substantiate the three dimensional hypothesis. Predictions in such cases as these can be made and fulfilled on either hypothesis, providing the calculations are made in accordance with the hypothesis (the one including, the other excluding, double cross-overs).

It is not necessary to carry the analysis farther, since neither theory would be benefitted by the fulfillment or non-fulfillment of these predictions. The question at issue is not met in this manner. It is involved in features lying outside the province of the present paper and is being treated by other authors, as mentioned above.

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[1] Castle, W. E., (*a*) "Is the arrangement of the genes in the chromosome linear?" these PROCEEDINGS, **5** 1919 (25–32); (*b*) "The linkage system of eight sex-linked characters in Drosophila virilis (data of Metz)," *Ibid.*, **5** (32–36).

[2] Sturtevant, Bridges and Morgan, "The spatial relations of genes," *Ibid.*, **5**, 1919 (168–173).

[3] Castle, W. E., "Are genes linear or non-linear in arrangement?" *Ibid.*, **5**, 1919 (500–506).

[4] Metz, C. W., "The linkage of eight sex-linked characters in Drosophila virilis," *Genetics*, **3**, 1918 (107–134).

[5] See Metz and Bridges, these PROCEEDINGS, **3**, 1917 (673–678). Before the hairy stock was lost some data on the value hairy-magenta were obtained by Weinstein, as noted by Muller (*Amer. Nat.*, **54**, p. 118), but the numbers were hardly large enough to be conclusive.

[6] Assuming, of course, that crossing-over is a consistent process, and that the experiments are conducted under similar conditions.

[7] My data, in the cases under consideration, include only relatively small numbers and give ratios of only approximate accuracy, as indicated in my paper. This fact alone would vitiate predictions of short distances (such as the first two) even if they were otherwise valid.

THE FUNCTIONS OF INTENSITY AND PHASE IN THE BINAURAL LOCATION OF PURE TONES

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Communicated by R. A. Millikan, February 5, 1920

The permanence of the value of knowledge concerning the physical aspects of audition gives to contributions in this field a greater importance that is at first apparent. There has been much confusion as to the factors involved in the binaural location of sounds. An analysis of the problem of ascertaining the important factors and their relative values show that the solution will be secured most speedily by the separate control of these factors and by using first pure and then complex tones. This report includes experiments covering the frequency range of 100 to 1200 d.v., the results giving the effects of intensity differences and phase differences at the ears, each factor treated first alone and then in combination.

Intensity.—Rayleigh (*Phil. Mag.*, **13**, 1907 (214)) was the first to point out that for frequencies of 128 and 256 d.v., the difference in intensity at the ears could not account for the ability to locate the source of sound. Although there have been many experimental contributions in the localization of sound, the only observers to make quantitative measurements of the effect of intensity were Hovda and the writer (*Psych. Rev.*, **25**, 1918, 3 (242)). They found that, for a frequency of 256 d.v., if the difference of phase at the ears was kept zero but the ratio of intensities varied, there appeared a moving phantom source having a position in front, for an intensity ratio of unity, and rotating horizontally about the observer, its angular displacement from the median plane being proportional to the logarithm of the ratio of the intensities at the two ears, the constant of proportionality varying for the individual and for the frequency. This logarithmic law has now been further tested for frequencies of 512 and 1024 d.v., and has been found to be correct for displacements from the median plane, less than 90°, excepting that for the 1024 d.v. some individuals do not get a moving phantom source of the fused sound. It would thus appear that intensity might have an important influence in localization. But such a conclusion must involve a quantitative test. Assume a source of sound at a distance of five meters from the observer and carried in a circle about him. It is possible, by means of theory (Stewart, *Physic. Rev.*, **33**, 1911 (467)), to compute the ratio of intensities at the ears produced by such a source at different angular displacements from the median plane. But one can, by means of the experiments already mentioned, ascertain what apparent angular displacements would be produced by the computed intensity ratios. Do these computed displacements agree with the actual? If they do, obviously intensity is an important factor in the actual case. But they do not agree quantitatively. For example, if the displacement of the source was 60°, the computed displacements for one observer were as follows: for 256 d.v., 6.1°, for 512 d.v., 13.8°, and for 1024 d.v., 11.2°. For a second observer, and the last two frequencies, the computed angles were 9.1° and 9.1°. A careful examination of all angular displacements up to 90° showed a similar lack of correspondence. Obviously the intensity effect cannot account for the ability to locate sounds. It does not appear to be even an important factor.

Experiments showed that for some people (two out of four observers) the intensity effect disappeared entirely in a certain frequency region. With each of four observers the intensity effect in a relatively large frequency region involved not only a moving phantom source of sound, but also an additional one stationary and in front, or in a position corresponding to the difference of phase.

The logarithmic law of Stewart and Hovda appears not to be merely an extension of Weber's Law, as at first announced.

Phase.—That phase difference affects the localization of a sound source, has been known for a number of years. (Review of literature; Stewart *Physic. Rev.*, **9**, June, 1917 (502).) Hartley (*Physic. Rev.*, **13**, 1919 (373)), has produced evidence in favor of the conclusion that the phase difference of a pure tone at the ears is the controlling factor. The present report presents a considerable addition to this evidence and that of a quantitative character. In the experiments, frequencies from approximately

100 to 1200 d.v. were used. Purity of tone was approximated in a special apparatus, but the results were checked by tones from tuning forks and hence the conclusions are reliable. It was found that, if the phase differences at the two ears were varied, intensities remaining equal and constant, the angular displacement of the phantom sound from the median plane was proportional to the phase difference at the ears. Inasmuch as the theoretical phase differences at the ears were known (Stewart, *Physic. Rev.*, **4**, 1914 (252) and Hartley, *Ibid.*, **13**, 1919 (373)), it became possible to ascertain whether or not the relationship between phase difference and angular displacement was quantitatively the same as in the experiments just mentioned. When the comparison was made, it was found that there was a reasonably close quantitative agreement, i.e., the difference over practically all of the range was, on the average, less than 10%. In fact, the correspondence between results in the theoretical case and in the experimental one was so striking as to leave no doubt as to the fact that *phase* must be the *most important* factor in localization in the region 100 to 1200 d.v.

As can be readily understood, the higher the frequency the less that displacement from the median plane for which the phase difference is 180°. For 1024 d.v., this displacement is 41°. But this is not the maximum angle for which a source can be located by phase. This was shown by the existence of multiple phantoms or images. Thus the conclusion already stated seems worthy of full confidence as applicable for a displacement of less than 90° in front. This does not close the case with pure tones for there is yet to be ascertained what additional factors are of moment in locating a source not in the horizontal plane and not in front of the observer.

That it is impossible to explain the difference of phase effect in terms of intensities, either by bone conduction or otherwise, is proved by the fact that the difference of phase effect was clearly present at 1024 d.v., with two observers who could not at this frequency get the intensity effect at all, i.e., the moving phantom source occasioned by altering intensity ratios was not present. The necessary conclusion is that the ears do perceive phase differences.

The difference of phase effect ceased with increasing frequency at a frequency of 1200 to 1500 d.v. This was tried with four observers. When the effect ceased the phantom sound remained in the median plane with changing phase.

Intensity and phase combined.—Experiments in which phase and intensity differences were simultaneously applied showed that the actual relative importance of either of the two does not change in the combination. Thus the effect of an intensity difference at the ears merely made a relatively slight change in the angular displacement of the phantom source. Consequently our conclusions formed by phase difference alteration, only, are approximately correct for the actual case of location wherein both phase and intensity are altered.

The work thus far clearly shows that with frequencies 100 to 1200 d.v phase is the *chief* factor in localization with pure tones. For the lower frequencies the intensity effect is practically nil, and at all frequencies within the range considered the effect must be small. These conclusions are quite contrary to the view commonly held and appearing in psychological literature and texts. But they are worthy of confidence because based on *quantitative* results, and not on personal judgment. There remains a great amount of experimental work covering an extension of the frequencies and the employment of complex tones. Strictly speaking the localization herein mentioned applies only to 90° either side of the median plane and in front. The removal of this limitation will probably prove relatively simple after the experiments are completed. The above report while covering a limited range, nevertheless does make a definite advance in our understanding of the relative importance of the factors, phase and intensity, in binaural localization, and furnishes a basis upon which the future experiments may depend.

THE MEASUREMENT OF SMALL TIME INTERVALS AND SOME APPLICATIONS, PRINCIPALLY BALLISTIC[1]

By L. T. E. Thompson, C. N. Hickman and N. Riffolt

Ballistic Institute, Clark University

Communicated by A. G. Webster, February 25, 1920

Precision in time calibration is of importance in many classes of research. Several types of apparatus have been developed for this purpose. Some of these, such as the Helmholtz pendulum, permit extremely accurate reproduction of a stated interval but less exact absolute determination. A method proposed and extensively used by A. G. Webster has given satisfactory, absolute determinations of intervals as small as a millionth of a second.[2] The recording apparatus is electrical and the calibration is accomplished by a careful measurement of the height of fall of a projectile.

The Le Boulengé and the Aberdeen chronographs are in general use, the former having a falling projectile and the latter a rotating drum. With any apparatus involving a rotating drum, it is necessary to maintain the speed of the motor driving it within certain limits and to secure a dependable calibration, usually placed on the record as it is being made. Various instruments have been devised to accomplish this purpose.[3] A tuning fork producing a photographic or mechanically inscribed record of a sinuous form is perhaps the commonest example.

Means for applying discontinuously a record of a fork or other vibrator have been used. For example, a very successful method was recently developed at the Bureau of Standards by Duncan and Curtis in which straight lines were produced across the photographic film by the interruption of light passing through slits attached to the tuning fork.

In the ballistic investigations which have been conducted at Clark University during the past two years, a great many of the records obtained have required such a time scale; for example the pressure-time curves for a gun, the curves for the vibration of the gun barrel, and later the velocity measurements which are described in this paper. The Bureau method was first adopted. We have been able, however, to develop a new apparatus which permits the placing of equally sharp lines across the film at very high frequencies. This is of considerable importance in the small arms ballistic work, inasmuch as it is necessary to have the film moving at high speed.

The apparatus for the time records is shown in figures 1 and 2. The tuning fork has a small needle attached to one prong (a weight being attached to the other) against which there bears a light stiff filament of steel ribbon held in place by a massive frame attached to the base mounting of the tuning fork. (The filament is practically a duplicate of the one used in Professor Webster's Phonometer to record the motion of the vibrating disk.)[4] Upon this filament is mounted a small mirror. Light is reflected from it and focused on a stationary slit. The fork is struck with an elec-

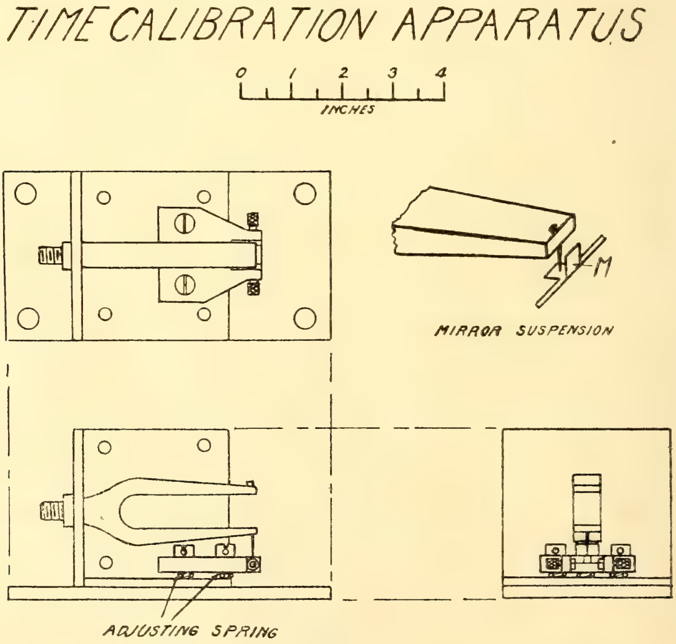


FIG. 1

trical hammer and allowed to vibrate freely, and in so doing causes the image of the slit in front of the arc to move up and down across the stationary slit. The stationary slit, then, is an intermittent source of light, the image of which is focused at a convenient place on the moving film. The optical system should be so designed that the beam only slightly more than covers the mirror. A new fork has been constructed giving about two thousand lines per second. There is no reason why this should not be as high as ten to twelve thousand lines per second. The velocity at the neutral position (where the slit is placed) is a maximum and includes the frequency as a factor. As it is increased the amplitude may decrease and still permit the same exposure of light through the slit as at the lower frequencies. Mechanically, the essential condition to be fulfilled in the design is that the natural frequency of the filament upon which the mirror is mounted should be high in comparison with that of the fork.

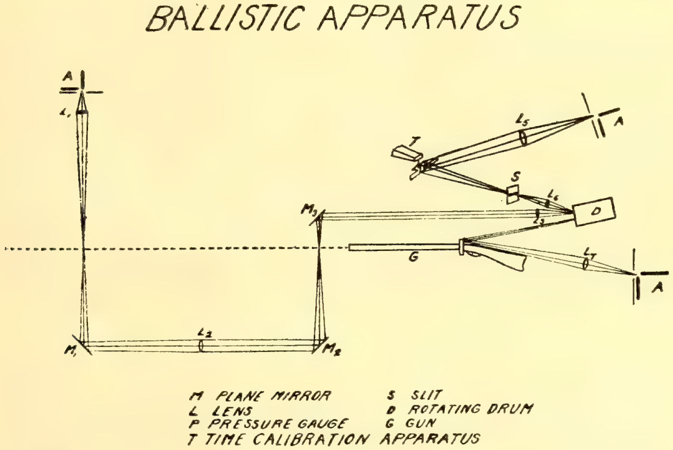

BALLISTIC APPARATUS

FIG. 2

Most of the methods which are at present used in ballistics for the measurement of velocity involve the passage of the projectile through screens of one sort or another, breaking or making electrical circuits. The method which we have devised does not require that any material be touched by the projectile in its flight, but simply the passage twice through a beam of light. It is found that it is only necessary to observe the flight of the projectile over a space of from 3 to 5 feet to get a precision which is certainly greater than that needed for ordinary purposes.

The beam of light obtained from a slit is controlled by the optical system (fig. 2) in such a way that it is focused twice, each time in the path of the projectile, being reflected by the mirrors M_1, M_2 and M_3 and finally brought to the photographic surface on the rotating drum. The image is a short sharp line. It is desirable to have for both this and the calibrating system a short focus lens. As the film rotates, the image of the slit will trace a narrow ribbon on the film surface. As the projectile passes through the ribbon in the two places successively an interruption of light occurs and a dark section appears in the ribbon at each of the corresponding instants. This is in general a longitudinally distorted image of the projectile and if the speed of the film were adjusted to a certain value it would be a miniature reproduction of its cross-section. Some records which have been obtained are shown in figure 3. The measurements have been made from the base line of the projectile in each case and the sharpness of this permits a determination of the length between these lines with a precision which is fully equal to that of the time calibration. The only observations, then, that are required are those of length. Each of these can be carried out to one part in ten thousand if necessary. The fact that a permanent record is provided and that no electrical apparatus is used also adds to the dependability of the results. A compact portable form has been designed for field use.

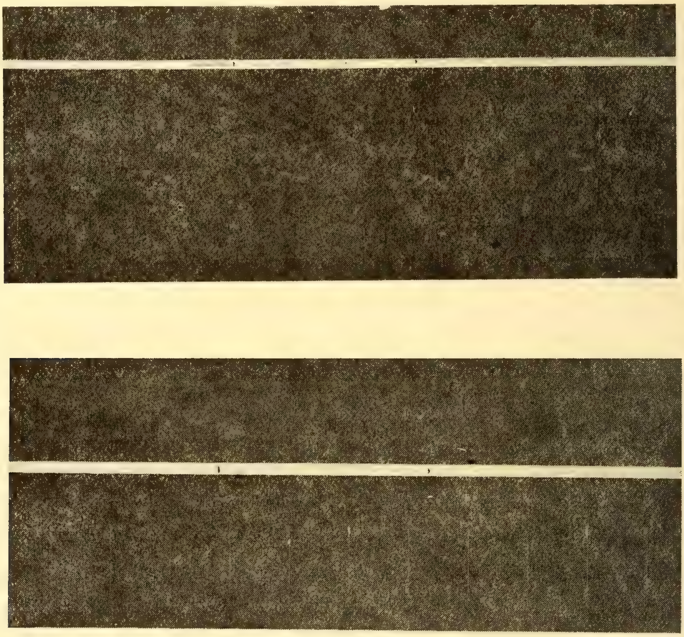


FIG. 3

A general apparatus for ballistic investigation is shown in figure 2. This includes a pressure gauge[5] as well as the velocity equipment. With one discharge of the gun a fairly complete ballistic record is obtained on the film mounted on the rotating drum *D*. The velocity at the muzzle, the pressure-time curve in the barrel, including the pressure when the projectile leaves, that at maximum value, the time calibration, and, by analysis of these curves, the velocity-time and pressure-space curves, as well as data regarding the resistance of the barrel, are all available.

An extensive application of this procedure is to be made in a determination of the resistance of air at various densities, to the flight through it of bodies moving with relatively high velocities. Heretofore, measurements have been made out-of-doors, shooting over a comparatively long range in order to get the desired accuracy. By making use of this velocity apparatus we are able to make two determinations in a length which is to be obtained in a building of laboratory size. The resistance is measured by the work which is done in the passage of the projectile from one point to another through the medium. This will require the two determinations of the velocity for the change in kinetic energy. It is necessary that the velocity be found with a precision which is sufficient to give the difference in the squares of the velocities with an acceptably small error. For a Government thirty caliber ($^1/_{50}$ lb.) projectile the resistance of the air at normal pressure is such that the velocity will decrease an amount of the order of 225 feet per second in travelling 300 feet from the muzzle. At densities greater than normal the decrease will, of course, be greater and at pressures less than normal (which will be obtained as described below) the decrease will be less than this.

$$\frac{\int_{s_1}^{s_2} R ds}{s_1 - s_2} = \bar{R} = \frac{m}{2s}(v^2 - v_0{}^2)$$

is the mean value of the retardation over the distance of observation (100–200 feet).

An approximate idea of the effect upon the final value of the resistance of an error in the measurement of the velocity of one part in ten thousand is obtained by an examination of the above expression.

$$\frac{\delta R}{R} = \frac{2v\delta v}{v^2 - v_0{}^2} + \frac{2v_0\delta v_0}{v^2 - v_0{}^2} + \ldots = \frac{4 \times 2500 \times {}^1/_4}{650000} + \ldots \text{approximately, or about } 0.4\%.$$

Here $\Delta v = 125$; $v_0 = 2650$; $v = 2525$; $\delta v = \delta v_0 = {}^1/_4$. At higher pressures the error will be still less and at pressures down to one-half normal the substitution in this expression shows that the error for a single observation will still be below 1%.

Inasmuch as the ratio of the resistance of the medium to the weight of the projectile decreases as the diameter increases and in direct proportion, the precision which would be expected with large projectiles would be correspondingly less. Nevertheless, by increasing the distance of observation the same order of accuracy would be possible with projectiles up to perhaps 3 inches in diameter. If the firing is done in a tube of from 1 to 3 hundred feet in length, having a diameter sufficient to make observation possible without great inconvenience, the air pressure in this tube being controlled by a pump, the observations of the velocities ranging perhaps from 1200 feet per second up to 3500 or 4000 feet per second can be made at each end of the tube. As long as the velocity of the projectile is greater than that of sound, no interference with the measurements will result from a disturbance reflected from the walls of the tube.

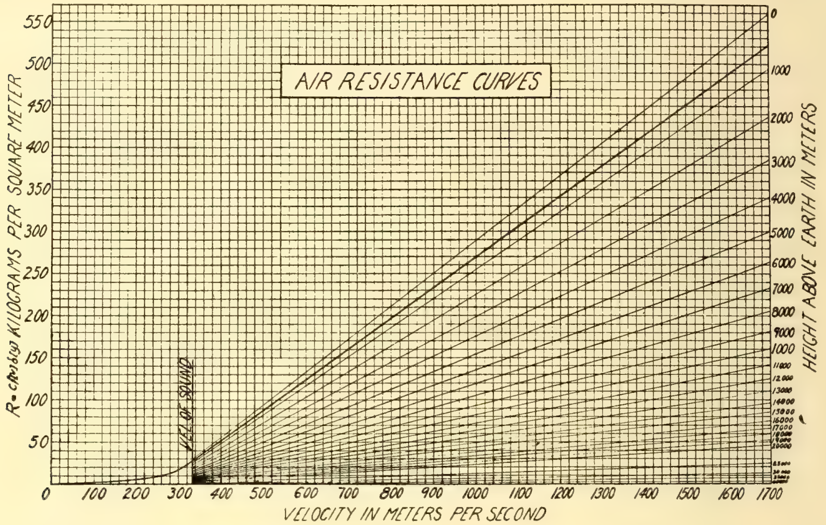
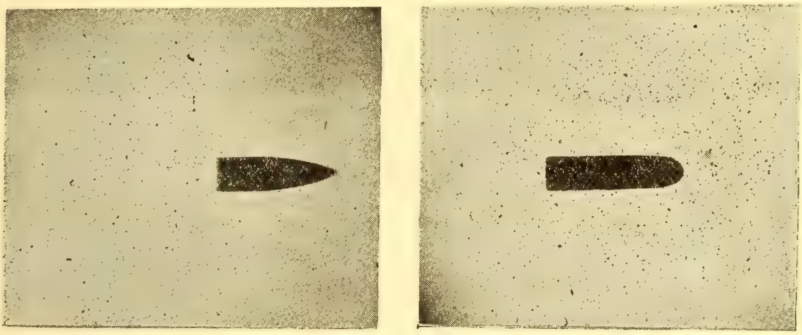


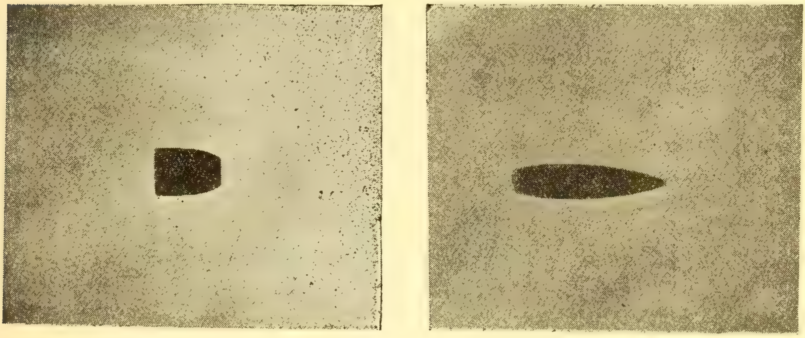
FIG. 4

Figure 4 is a graphical representation of the resistance as a function of the velocity as determined from the measurements made by at least five European observers, their data being averaged by the Italian ballistician, General Siacci. The retardation appears to be a linear function beyond the velocity of sound. Multiplying the datum line of these curves by different factors depending upon the density of the air gives the other curves for various heights. These were used in trajectory computations on long range guns of the type brought out by the Germans to shell Paris.[6] The experiments outlined will be adequate to make a precise determination of the values for the resistance for small projectiles over a considerable part of the usable portions of this graph for the modern rifles of high velocity. The fact that this problem is not the exact equivalent hydrodynamically of the one for very large projectiles does not materially lessen the value or interest of the results of such an investigation with small projectiles.

An important problem is the accurate determination of the effect of varying the form of the projectile. The question of ultimate or optimum form for a given use should be experimentally determinate. The records show not only the velocity but the manner in which the projectile is travelling. Also, it is likely that the precision and the complete control of the conditions of experiment possible would lead to satisfactory results as to the variation of the so-called form factor with the velocity. If the results indicate an entirely new law of resistance for each form of projectile, as has been suggested, this fact should be evident. In this case there will not be a linear relation between the datum line for a projectile of one form over its range of velocities and that for another projectile with a different form. The fact that a projectile in actual flight may gradually change its cross-sectional surface presented in the direction of flight owing to precession (and thus produce a gradual change in the "law") is an argument for making the measurements in a short distance.



30 cal. gov. 30 cal. 220 gr.



44 cal. 30 cal.

FIG. 5

Another ballistic application of the method of determining velocities is in connection with penetration. The energy loss, and even the law of retardation, that is to say the resistance of a solid with the passage of a body through it under given conditions, can be found through spark photography and successive determinations of the velocity. Here it is practically necessary to be able to measure the velocity in a few feet, this apparatus thus being particularly adapted.

While the ballistic application of the experiments is perhaps most important, yet the general investigation of the hydrodynamical aspects is of considerable interest. It is not necessary to restrict the bodies which are hurled through a medium to those of practical ballistic design or dimensions. Coupled with the spark method of obtaining instantaneous photographs of the projectile and the waves connected with its flight, it is possible to examine the condition of the medium and the effect o change in body shape for a great range of conditions.

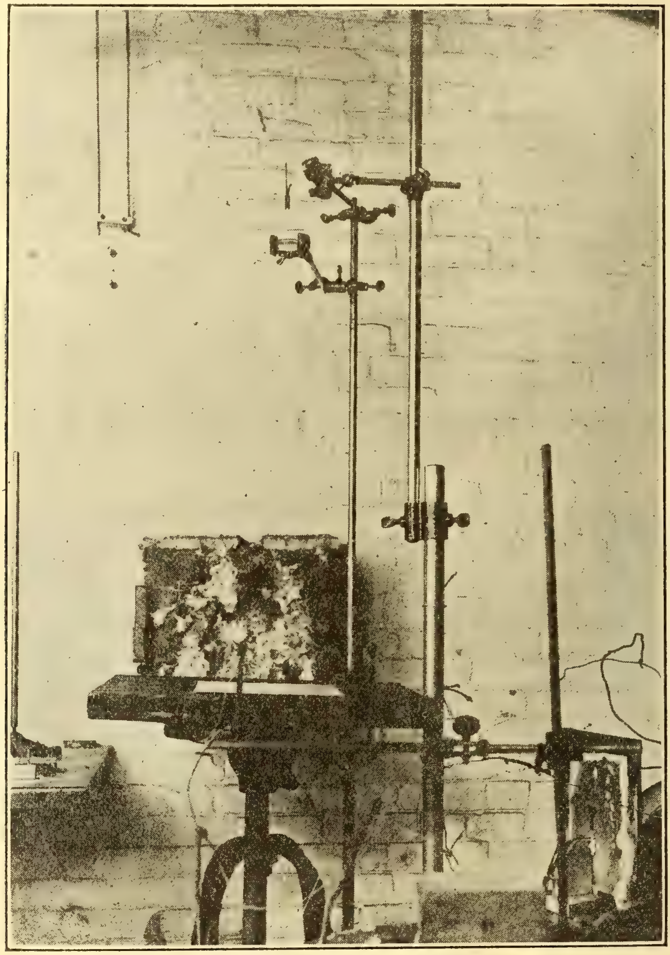


FIG. 6

The energy given up by a body in motion is dissipated in at least three ways—through skin friction, gas compression,[7] and production of the turbulent region particularly evident in the rear (see spark photographs, fig. 5). In order to formulate a theory of retardation it is necessary to have quantitative information regarding these factors and their relative importance. This information is very difficult to obtain.

Another application of the method of time calibration and of determining velocities comes in an entirely different field, namely, the measurement of the velocity of light. With a range of about 40 or 50 miles, which is available, a simple apparatus of this type will provide a record comparing the instant of arrival of light directly from the source (one of short duration, such as a spark or very rapidly moving slits) and that which has gone out and back over the 50 miles. The record will be similar to those which have been shown and obtained in much the same way. A precision which is at least as great as that obtained by the other methods which have been used for the measurement of the velocity of light is to be expected and in addition the permanent record, which will be given by a deflection on the film of the order of two centimeters, this representing the time taken for the light to travel the 50 miles.

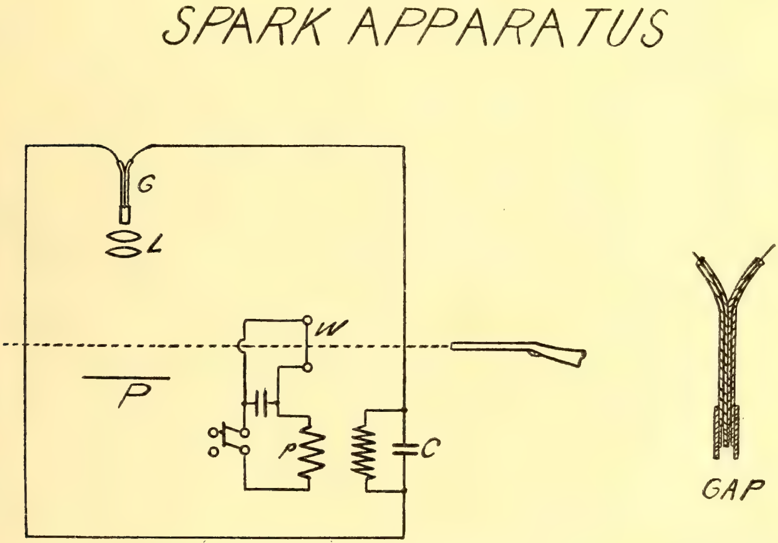


FIG. 7

The shadowgraphs (fig. 5) of projectiles in flight were made by use of the apparatus shown in figure 6, a diagram of which is shown in figure 7. The spark is obtained by breaking a small wire (*w*) through which the current passes to the primary of a large coil. The current is applied just before firing the gun, and in this way a very small wire can be used, not having time to fuse. The secondary condenser *C* controls the time interval elapsing between breaking the wire and the passing of the spark through the gap *G* which is constructed as shown in the figure. This makes a good "point" source and one always in the same place. The lens *L* is quartz. The position of the projectile on the plate can be controlled to perhaps 1 centimeter by moving the wire (*w*) with respect to the plate. This corresponds to a time interval of $^1/_{75,000}$ second.

[1] Contribution from the Ballistic Institute, Clark University, No. 7.

[2] Webster, A. G., "On A Means of Producing a Constant Angular Velocity," *Amer. Jour. Sci.*, **3**, May, 1897 (383–386).

[3] Webster, A. G., "An Experimental Determination of the Period of Electrical Oscillations," *Physic. Rev.*, **6**, 1898 (297–314); Hubbard, J. C., "An Experimental Determination of the Period of Electrical Oscillations," *Physic. Rev.*, **2**, 1, 1913 (247–249).

[4] Webster, A. G., *Proc. Nat. Acad. Sci.*, **5**, May 1919 (163–166).

[5] Webster, A. G. Thompson, L. T. E. "A New Instrument for Measuring Pressures in a Gun," *Proc. Nat. Acad. Sci.*, **5**, p. 258-263.

[6] Webster, A. G., "Some Considerations on the Ballistics of a Gun of Seventy-five-Mile Range," *Proc. Amer. Phil. Soc.*, **58**, p. 373–381. (Note by A. G. W. It should be stated that the linear law is not now considered exact. M. Lugot, of the Commission de Gâvre, informed the writer that the curves should rise a little above the straight lines, but the error is small.)

[7] Experiments to measure the compression are now in progress.—A. G. W.

CHARCOAL ACTIVATION

By H. H. Sheldon

Ryerson Physical Laboratory, University of Chicago

Communicated by R. A. Millikan, February 20, 1920

The results obtained by Dr. Harvey B. Lemon on the variations due to heat treatment in the adsorption of gases by charcoal,[1] although published but recently, were obtained some time ago. There seemed two possible explanations of these results—either that the structure of the

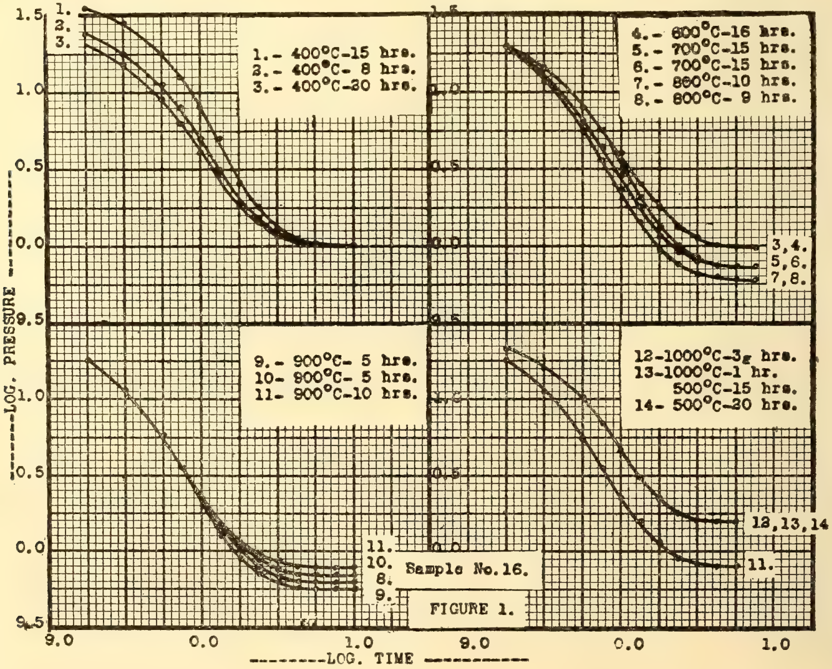


FIGURE 1.

charcoal was itself modified by heat alone or that the air was the agent causing these modifications. On this latter view a very complete theory, the hydrocarbon theory, has since been worked out by the Chemical Warfare Service.

The theory as published was not, however, substantiated by the appearance of any data in the journals, and accordingly certain experiments were begun here to satisfy ourselves regarding the conclusions arrived at. The apparatus and method were identical with those used by Dr. Lemon, and 25.7 gram of the same sample of charcoal, No. 16, was used. A similar sample to the one recorded was used as a check on all the work.

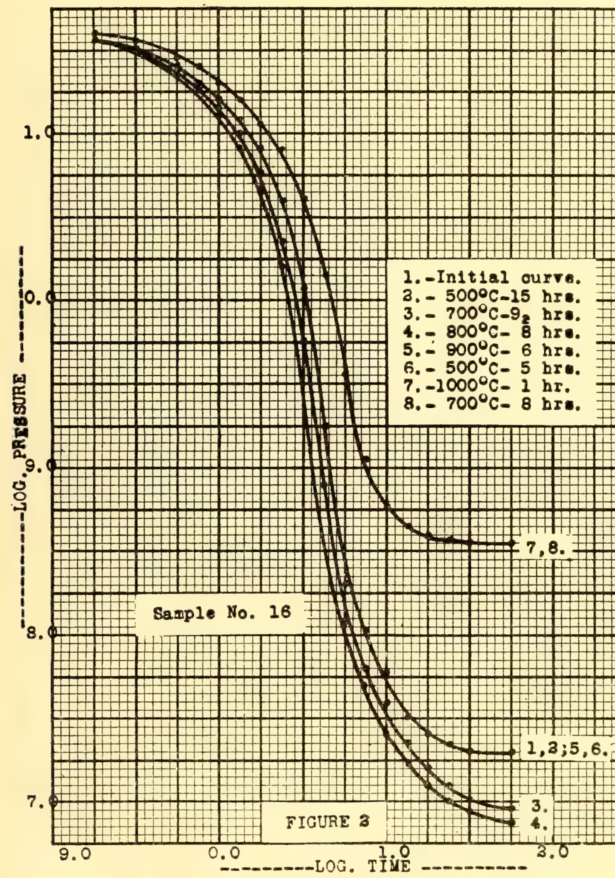


FIGURE 3

The constant end pressure after heat treatments up to 600° C. both in the hydrogen curves, figure 1, and in the nitrogen curves, figure 2, show that the activation recorded by Dr. Lemon for these temperatures, must have been due to oxidation. At temperatures between 600° C. and 900° C. activation takes place, accounted for by assuming that inactive hydrocarbons, stabilized by adsorption, are, however, being boiled away at this temperature. Above this we find deactivation, due to the breaking up of hydrocarbons at this high temperature which form an inactive carbon deposit on the active base.

In the case of U. S. Government 600 minute charcoal, no such deactivation at this high temperature was observed, but in this charcoal the hydrocarbons are supposedly all removed. It offers no contradiction therefore.

The outgassings were as indicated on the next page.

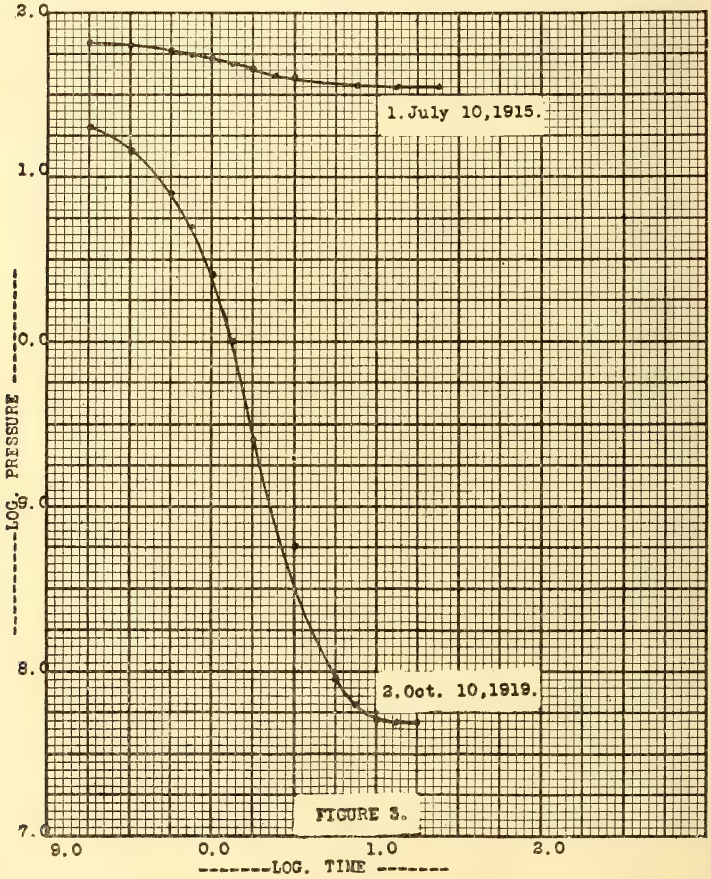


FIGURE 3.

Figure 3 shows how charcoal may be activated by slow oxidation at room temperature. Curve 1 was taken July 10, 1915, and the sample was then put away and left undisturbed until Oct. 10, 1919, when curve 2 was taken.

The ease with which the charcoal could be deactivated for nitrogen compared to deactivation for hydrogen, suggested that a sample might be put into such a condition that it would adsorb hydrogen more readily than nitrogen. Results of this sort are shown in figure 4; curves 1 and 2 are nitrogen and hydrogen, respectively, before treatment, and curves

3 and 4, the same after treatment. This treatment consisted of $3^1/_2$ hours at 1000° C. in a vacuum, kept down by a condensation pump.

Figure 1, Hydrogen			Figure 2, Nitrogen		
1	400° C.	15 hours	1	Initial Curve	
2	400° C.	8 hours	2	500° C.	15 hours
3	400° C.	20 hours	3	700° C.	$9^1/_2$ hours
4	600° C.	16 hours	4	800° C.	8 hours
5	700° C.	15 hours	5	900° C.	6 hours
6	700° C.	15 hours	6	500° C.	5 hours
7	800° C.	10 hours	7	1000° C.	1 hour
8	800° C.	9 hours	8	700° C.	8 hours
9	900° C.	5 hours			
10	900° C.	5 hours			
11	900° C.	10 hours			
12	1000° C.	$3^1/_2$ hours			
13	1000° C. 500° C.	1 hour 15 hours			
14	500° C.	20 hours			

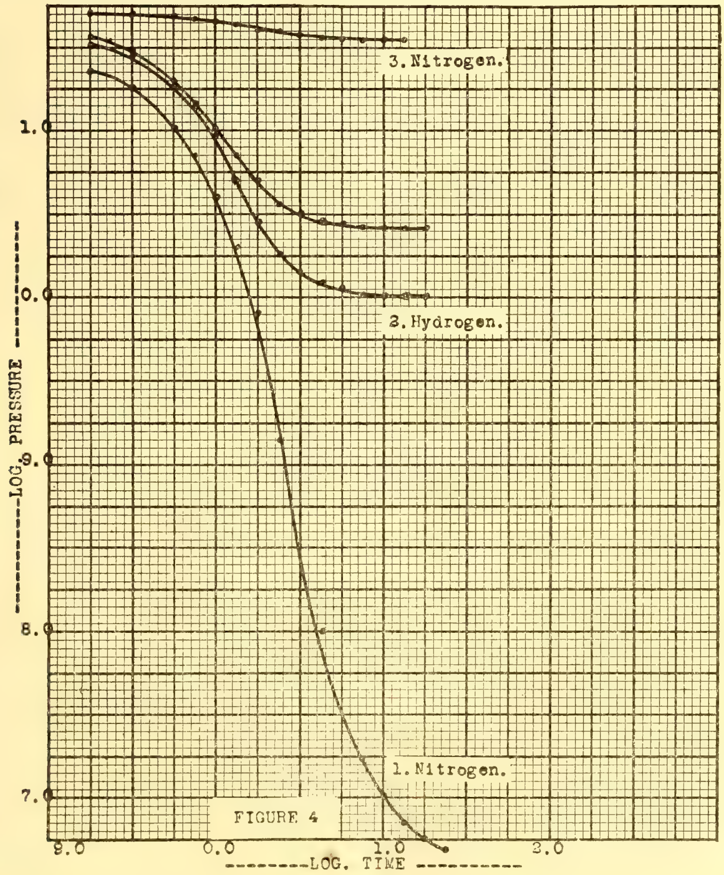


FIGURE 4

Several samples so treated worked successfully, and the fact that it was found impossible with U. S. Government 600 minute charcoal suggests that it was due to action of some sort between the hydrocarbons present and the gas. The fact that McBain's work[2] with hydrogen shows a dual action, and that work done later by others[3] on other gases points to a single action, suggests that while surface condensation is likely to explain the major action in both cases, that in the case of hydrogen there must be something else at work as well; either the hydrogen is dissolved or possibly combines with hydrocarbons unsaturated at this liquid air temperature. It is difficult to draw a distinct dividing line between the two, but the latter view is the one taken.

The deposit of inactive carbons on the active base, is according to this theory, much more effective in deactivating for nitrogen, since, interfering with surface condensation, it interferes with the whole effect, this not being the case for hydrogen which has a dual nature. The observations bear this out.

The decided drop in the initial end of the curves in figure 1 at the beginning of the work with hydrogen, which does not appear in the nitrogen curves of figure 2, again shows a difference in the adsorption of these two gases, possibly resulting from an increased fineness of the division of the material with which the hydrogen unites. Further work with other gases will be undertaken.

The author is greatly indebted to Dr. Harvey B. Lemon for valuable advice and suggestions in connection with this work.

[1] These PROCEEDINGS, **5,** July 1919, pp. 291–295.

[2] *Phil. Mag. London,* Ser. 6, **18,** 1909 (916).

[3] Miss Homfray, *Zs. Phys. Chem.,* **74,** 1910 (129).

THE STARTING OF A SHIP

BY JAMES K. WHITTEMORE

DEPARTMENT OF MATHEMATICS, YALE UNIVERSITY

Communicated by L. B. Mendel. Read before the Academy, November 11, 1919

In this paper we give first the results of a mathematical discussion of the motion of a particle under the action of tangential forces depending only on the velocity of the particle.[1] In the following paragraphs we suggest applications of these results to marine engineering and to the study of the law of resistance of liquids.

We consider a particle, P, moving in any path under the action of forces whose components along the tangent to the path depend on the velocity of the particle alone. We denote the time by t, the distance covered in the time t by x, the velocity at the start by v_0, at any time t by v, and the acceleration along the tangent by a. By hypothesis, $a = F(v)$. Concerning $F(v)$ we assume

$$F(v) = (\lambda - v)\varphi(v), \quad \lambda > v \geqq v_0,$$

and that the function $\varphi(v)$ is not less than a positive number m for such values of v and is finite for $v = \lambda$. It may then be proved

$$(\lambda - v_0)e^{-mt} \geqq \lambda - v > 0, \tag{1}$$

$$\lambda t - x = L - \int_v^\lambda \frac{dv}{\varphi(v)}, \tag{2}$$

$$L = \int_{v_0}^\lambda \frac{dv}{\varphi(v)}, \quad \frac{1}{m}(\lambda - v_0)e^{-mt} \geqq \int_v^\lambda \frac{dv}{\varphi(v)} > 0. \tag{3}$$

These results may be interpreted as follows: the velocity v increases from v_0 and approaches λ indefinitely as t increases without ever reaching that limit. But this "limiting velocity" λ is, for practical purposes, attained and is hereafter called "full speed." To interpret equation (2) we consider a particle P' describing the same path as P, starting from coincidence with P at the time $t = 0$, and moving uniformly with velocity λ; then evidently $PP' = \lambda t - x$. Equation (2) shows that PP' increases from zero approaching the finite limit L. We call L the "lost distance" or the distance lost in attaining full speed. It is clear that L is practically the distance that P is behind P' when P has reached full speed. If P is, for example, a ship attaining and then proceeding at full speed, L could be found from observation, for this lost distance is the difference of the distance run in any time τ at full speed and the distance run in the time τ from the start, provided that full speed is attained in the time τ from the start. We introduce the time T lost in attaining full speed, defined by $L = \lambda T$. This lost time T would, in the case of a ship or of a towed model, perhaps be more easily measured than L, for T is the difference of time in running any distance δ at full speed and in running the distance δ from the start, provided that full speed is attained in the distance δ from the start. Then if T is measured L is given by $L = \lambda T$. Now if the acceleration $F(v)$ is known L may be calculated from equation (3). If $F(v)$ is expressed by a formula containing an unknown constant and if L is found by observation the constant may be calculated from (3). These remarks lead to the suggestions of applications to the motion of a ship starting from rest or increasing speed under its own power or to the motion of a towed model.

For a towed model or for a ship increasing speed under the action of the thrust of the screw the component of accelerating force in the direction of the tangent to the path has the form, $f - r$, where f is the tension of the towing line in the case of a towed model or the effective thrust of the screw in the case of a ship, and r is the resistance of the water. No general expression for r is known, but it may be assumed that for any particular model or ship r is a function of v alone. We consider two forms of f: first f is constant, $f = k$; second f is the force exerted by a constant power, $f = c/v$.

Suppose $f = k$. Then if W is the weight of P,

$$\frac{W}{g}a = k - r(v), \quad a = K - R(v) = F(v).$$

Since $F(\lambda) = 0$, $K = R(\lambda)$, and

$$F(v) = R(\lambda) - R(v) = (\lambda - v)\varphi(v),$$

$$L = \int_{v_0}^{\lambda} \frac{dv}{\varphi(v)} = \int_{v_0}^{\lambda} \frac{(\lambda - v)dv.}{R(\lambda) - R(v)}.$$

If we assume the resistance of the water proportional to some positive power of the velocity, $R(v) = K_1 v^n$, the conditions imposed on $F(v)$ are satisfied, and

$$L = \frac{1}{K_1}\int_{v_0}^{\lambda} \frac{(\lambda - v)dv}{\lambda^n - v^n}.$$

If in particular $v_0 = 0$ and if $n = 2$,

$$L = \frac{1}{K_1}\log_e 2 = \frac{0.693}{K_1}.$$

We have the curious result for this particular case that L is independent of λ and consequently of the force k. If L is obtained from observation K_1 is found from the preceding equations and the force required to develop speed λ is determined. Comparison of K_1 for different models would serve to determine the relative merits of their designs.

An apparatus might be simply constructed to give the effect of towing models with a constant force. Suppose, to outline such an apparatus in its simplest form, a model to be towed by a perfectly flexible horizontal cord passing over a pulley without weight or friction on its axis and attached to a descending weight. The tension of the cord would, under the assumptions, be the same on both sides of the pulley, but would vary with the velocity. The equations of motion for the model, weight W, and the descending weight W' are

$$\frac{W}{g}a = T - r(v), \quad \frac{W'}{g}a = W' - T,$$

where T is the tension. Adding the equations we have

$$a = \frac{g}{W + W'}[W' - r(v)],$$

from which it appears that the motion is the same as if the towing force were constant, the force of resistance however being modified by a constant factor. The preceding discussion would be applicable. In a more practical but less simple apparatus consisting of several cords and pulleys, the former not perfectly flexible, the latter neither weightless nor perfectly smooth, the results appear still to be applicable, account being taken of the stiffness of the cords and of the weight of the pulleys, but the friction of the pulleys on their axles must be eliminated or else the previous results cannot be applied.

If it is found possible so to run the engine of a ship as to deliver constant effective thrusting power at the screw we may give a similar discussion of the motion of the ship, where now $f = c/v$. Even if it is not possible to produce constant effective thrusting power in starting a ship from rest it may be found possible to do so in increasing speed and the following discussion would be applicable. We have

$$\frac{W}{g}a = \frac{c}{v} - r(v), \quad a = \frac{C}{v} - R(v) = F(v).$$

Since $F(\lambda) = 0$, $C = \lambda F(\lambda)$, and

$$F(v) = \frac{\lambda R(\lambda) - vR(v)}{v} = (\lambda - v)\varphi(v),$$

$$L = \int_{v_0}^{\lambda} \frac{dv}{\varphi(v)} = \int_{v_0}^{\lambda} \frac{(\lambda - v)v dv}{\lambda R(\lambda) - vR(v)}.$$

If $R(v) = K_1 v^n$ the conditions imposed on $F(v)$ are satisfied as before. If in particular $v_0 = 0$ and $n = 2$ we have $L = 0.247/K_1$, a value independent of λ and C. The assumption $R(v) = K_1 v^2$ gives $C = K_1\lambda^3$, a formula connecting power and speed which is often used but which is certainly not correct.[2]

It is not possible to represent the resistance offered by the water to the motion of a model or a ship by an expression of the form $R(v) = K_1 v^n$. It is suggested that the law of resistance may be studied in two ways: more complicated laws of resistance may be assumed and tested by comparing the measured values of L with the values given by equation (3) under the assumed law. Either constant force or constant power may be used. Secondly, it may appear from experiment that the distance L for a model started from rest and brought to full speed λ, by a constant force, for example, can be expressed as a function of λ. Then the function $R(v)$ determining the law of resistance might be studied from the integral equation

$$L(\lambda) = \int_{0}^{\lambda} \frac{(\lambda - v)dv}{R(\lambda) - R(v)} = \lambda^2 \int_{0}^{1} \frac{(1 - z)dz}{R(\lambda) - R(\lambda z)}, \quad v = \lambda z.$$

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[1] The complete discussion is to be published in the *Annals of Mathematics*, probably in the number for June, 1920.

[2] See, for example, the "Admiralty Coefficient Formula," $H.P = \Delta^{2/3}V^3/K$, given by C. W. Dyson, *Practical Marine Engineering*, 7th edition, 1918, p. 614.

A THERMODYNAMIC STUDY OF ELECTROLYTIC SOLUTIONS

By Frank L. Hitchcock

Department of Mathematics, Massachusetts Institute of Technology

Communicated by Edwin B. Wilson, January 30, 1920

1. *Scope of the investigation.*—It is well known that aqueous solutions of strong electrolytes do not conform to the requirements of the usual mass law. Measurements of freezing-point and of electric conductivity unite in showing a marked rise of the "ionization-constant" with increase of concentration. The accumulation during recent years of a considerable body of accurate data renders a theoretical examination of these relationships highly desirable.

The theory of chemical potentials, due to Willard Gibbs, offers a ready tool for examining what the rigorous consequences of the dissociation hypothesis must be. From this theory may be deduced, for example:

1°. An expression for the heat of dilution as a function of temperature and of concentration.

2°. A rigorous equation for determining the freezing-point in terms of the concentration—nothing being assumed as to the "ideal" character of the solution.

3°. A generalization of the usual mass law.

These three equations will contain certain constants, dependent on the thermic properties of the solution. Some of these constants can be calculated from specific heats, or from latent heats of melting or of evaporation. Others, in the present state of our knowledge, must be determined empirically. The constants in the three equations, in so far as they are generalizations of the usual equations, are the same. Hence any results obtained under one head can be checked by the others.

2. *Heat developed by chemical or physical changes at constant temperature and pressure.*—It is well known that when a system at constant temperature and pressure undergoes any change, the heat Q emitted during the change is connected with the total free energy φ of the system by the differential equation

$$dQ = d(T\frac{\partial\varphi}{\partial T} - \varphi) \tag{1}$$

This equation is quite general. It is not necessary that the change in question be reversible. If the system consists of several phases it is not necessary that the pressure be the same for all, provided it is constant for each.[1]

3. *The general condition of equilibrium.*—It is also well known that for a system of several phases whose composition is determined by masses $m_0, m_1, m_2, \ldots m_n$ the existence of a state of equilibrium implies the equation

$$\frac{\partial\varphi}{\partial m_0}\,dm_0 + \frac{\partial\varphi}{\partial m_1}\,dm_1 + \frac{\partial\varphi}{\partial m_2}\,dm_2 + \ldots + \frac{\partial\varphi}{\partial m_n}\,dm_n = 0 \qquad (2)$$

which is one of the requirements that the free energy be a minimum. The partial derivatives $\partial\varphi/\partial m_0$, etc., were called by Gibbs the chemical potentials of the respective masses. I shall denote these chemical potentials by the symbols f_0, f_1, etc., that is

$$f_0 = \frac{\partial\varphi}{\partial m_0},\ f_1 = \frac{\partial\varphi}{\partial m_1},\ \ldots f_n = \frac{\partial\varphi}{\partial m_n} \qquad (3)$$

so that the general condition of equilibrium may be written

$$f_0 dm_0 + f_1 dm_1 + f_2 dm_2 + \ldots + f_n dm_n = 0\,. \qquad (4)$$

Equation (4) may be said to be the kernel of the Gibbs theory.

4. *Application to the melting-point curve of ice in contact with an aqueous solution.*—Let our chemical system now consist of m_0 mols of water in which are dissolved m_1, m_2, $\ldots m_n$ mols of various solutes in contact with a mass of ice m (without subscript). If any ice melts an equal amount of water is formed, that is $dm_0 = -dm$. The masses of the solutes are unchanged, that is $dm_1 = 0$, $dm_2 = 0$, etc. Hence, equation (4) becomes for this case

$$f_0 dm_0 - f dm_0 = 0$$

that is

$$f_0 - f = 0. \qquad (5)$$

When ice is in equilibrium with an aqueous solution the chemical potential of the ice is equal to the chemical potential of the solvent.

It is important to notice that f_0 is not the potential of the solvent in the pure state, but its potential as it actually exists in the solution; it is a function of the concentrations of the various dissolved substances as well as of temperature and pressure. On the other hand f, the chemical potential of the ice, is a function of temperature and pressure only. It is also important to notice that one mol of ice or water here means merely one formula-weight, and assumes nothing as to actual molecular weight. Thus the chemical potentials are the free energies per formula-weight of their respective masses.

5. *The latent heat of melting ice in contact with the solution.*—When a little water freezes out of the solution the heat emitted is determined by equation (1). The only variables are m_0 and m. We may, therefore, write

$$dQ = \frac{\partial}{\partial m_0}\left(T\frac{\partial\varphi}{\partial T} - \varphi\right)dm_0 + \frac{\partial}{\partial m}\left(T\frac{\partial\varphi}{\partial T} - \varphi\right)dm \qquad (6)$$

by the ordinary formula for a total differential. But we also have, identically,

$$\frac{\partial}{\partial m_0}\frac{\partial\varphi}{\partial T} = \frac{\partial}{\partial T}\frac{\partial\varphi}{\partial m_0} = \frac{\partial f_0}{\partial T} \text{ and } \frac{\partial}{\partial m}\frac{\partial\varphi}{\partial T} = \frac{\partial f}{\partial T} \qquad (7)$$

while $dm_\circ = -dm$, so that (6) simplifies to

$$dQ = \left[T\frac{\partial f}{\partial T} - f - T\frac{\partial f_\circ}{\partial T} + f_\circ\right]dm \tag{8}$$

We may perfectly well suppose the total amount of solution to be so great that the freezing out of 1 mol of ice does not affect its composition. The expression in brackets is then a constant. Letting Q stand for the latent heat per mol we may, therefore, write

$$Q = T\frac{\partial f}{\partial T} - f - T\frac{\partial f_\circ}{\partial T} + f_\circ \tag{9}$$

6. *The form of the melting-point equation.*—If we adopt, as a temporary notation,

$$y = f_\circ - f \tag{10}$$

equation (9) becomes

$$-T\frac{\partial y}{\partial T} + y = Q \tag{11}$$

whose integral is, pressure being regarded as constant,

$$\frac{y}{T} = -\int\frac{Q}{T^2}dT + F(c) + K \tag{12}$$

where $F(c)$ is a function of the concentrations of the various solutes, but not of the temperature, and K depends on neither concentrations nor temperature. Since by (5) $y = 0$ when we have equilibrium, the equation of the melting-point curve takes the form

$$-\int\frac{Q}{T^2}dT + F(c) + K = 0 \tag{13}$$

7. *The chemical potential of the solvent.*—To determine the form of the function $F(c)$ we now proceed to get an expression for the potential $f_\circ$ of the water in the solution. The concentrations may be defined as

$$c_1 = \frac{m_1}{m_\circ},\ c_2 = \frac{m_2}{m_\circ},\ \ldots\ c_n = \frac{m_n}{m_\circ} \tag{14}$$

Since the potential $f_\circ$ is a continuous function of the concentration and approaches $\varphi_\circ$ the potential of the pure solvent as the solution becomes infinitely dilute, we may, as a general concept, think of $f_\circ$ as expanded in a power series in the concentrations, the first term being $\varphi_\circ$. It is very well known, however, that, as a first approximation, the diminution of potential when any solute is added to pure water is equal to RTc, that is, as a first approximation

$$f_\circ = \varphi_\circ - RT(c_1 + c_2 + \ldots + c_n) \tag{15}$$

which holds good at infinite dilution. As a second approximation we may assume that each of the dissolved substances acts like a perfect gas, which leads to the much more accurate equation[2]

$$f_\circ = \varphi_\circ - RT\ln(1 + c_1 + c_2 + \ldots + c_n) \tag{16}$$

which holds good when the solution is "ideal." It is evident that (15) is a limiting case of (16). If, as is the main object of the present paper, we wish to investigate solutions which are not ideal, we may write the perfectly general equation

$$f_0 = \varphi_0 - RT\ln(1 + \Sigma c) + P(c,T) \qquad (17)$$

where Σc means the sum of the concentrations of the various solutes, and $P(c,T)$ is a power series in concentrations and temperature. This series will, for the most general expression, contain the squares, products, and higher powers of the concentrations. It will not contain the first power, for (15) must be the limiting form of the equation as the solution becomes very dilute.

Instead of the power series $P(c,T)$ explicit algebraic functions might be introduced, based either on theoretical considerations or on empirical data.[3] The main point is that some form of function in concentration and temperature must be added to the right side of (16) to obtain sufficient generality. The object of this paper is to indicate how the entrance of such a function affects the ordinary theories of freezing-point, heat of dilution, and ionization, and for this purpose the power series appears the most convenient, and can be made as general as we please by taking terms enough.

It is important to notice that equations (15), (16) and (17) all assume that we know the molecular weights of the dissolved substances, that is, Σc depends on the actual number of dissolved molecules. This is fully in accord with the usual theory of very dilute solutions. But, equally important, nothing whatever is assumed as to possible hydration of these molecules, nor as to molecular complexes occurring in the solvent. As stated above, the number of mols m_0 of solvent means merely formula-weight. It is precisely such matters as hydration, polymerization of solute, and departure from gas laws which are to be taken care of by the series $P(c,T)$.

It will be most convenient to expand this series first in powers of the absolute temperature T, thus

$$P(c,T) = P_0 + P_1T + P_2T^2 + \text{etc.} \qquad (18)$$

It is doubtful whether we possess any data warranting the use of powers of T higher than the square. That the square, at least, is necessary will appear. The coefficients P_0, P_1 and P_2 will be power series in the concentrations only, it being assumed that the pressure is constant. (If we vary the pressure the coefficients in these new series will vary with pressure.) The expansion (18) is, however, general enough for our present purpose. We shall, therefore, have the potential f_0 of the solvent in the form

$$f_0 = \varphi_0 - RT\ln(1 + \Sigma c) + P_0 + P_1T + P_2T^2 \qquad (19)$$

where P_0, P_1 and P_2 are functions of the concentrations, which, if expanded in series, will all begin with terms of the second degree. In other words, terms in the first powers of the concentrations occur nowhere except in the logarithmic term; if it were otherwise, the solution would fail to obey the perfectly well established laws for very dilute solutions.

8. *Determination of the function* $F(c)$.—Returning now to equation (12), writing $y = f_0 - f$, and putting for f_0 its value we have

$$\frac{1}{T}(\varphi_0 + P_0 - f) + P_1 - R\ln(1 + \Sigma c) + P_2T = -\int \frac{Q}{T^2}dT + F(c) + K \quad (20)$$

where the only terms on the left which are functions of the concentrations alone are $P_1 - R\ln(1 + \Sigma c)$. Hence, by equating,

$$F(c) = P_1 - R\ln(1 + \Sigma c) \quad (21)$$

and by using this result the equation of the melting-point curve (13) becomes

$$-\int \frac{Q}{T^2}dT + P_1 - R\ln(1 + \Sigma c) + K = 0 \quad (22)$$

where, as already stated, P_1 is a function of concentration (or of pressure if that is not constant), but not of temperature.

9. *Heat of dilution.*—Before developing (22) further, it will be well to consider in a brief way the heat of dilution emitted when 1 mol of water is mixed with a large amount of solution under the same pressure. Take the complete system as consisting of a mass of pure water M together with the solution. The potential of pure water is φ_0. By reasoning precisely as in Art. 5, and letting H stand for the heat emitted when 1 mol of water is mixed with a large amount of solution,

$$H = T\frac{\partial f_0}{\partial T} - f_0 - T\frac{\partial \varphi_0}{\partial T} + \varphi_0 \quad (23)$$

Introducing the value of f_0 from (19) this becomes

$$H = P_0 - + P_2T^2 \quad (24)$$

The absence of the term in the first power of T is noticeable.

If the heat of dilution at constant pressure be expanded in powers of T, *the first power is rigorously absent.*

9. *The equation of the melting-point curve.*—Returning now to (22), we may think of the heat of melting Q as composed of two parts, first the heat of melting at that temperature in contact with pure water, second the heat of dilution. The first we may call Q_0, the second is $-H$ (since Q was taken as heat emitted on freezing). Thus

$$Q = Q_0 - H \quad (25)$$

Without assuming the results of the last article, we may expand H in powers of T,

$$H = H_0 + H_1T + H_2T^2 \quad (26)$$

It is more convenient to expand $Q_\circ$ in powers of t, where $t = 0$ is the freezing-point of pure solvent,

$$Q_\circ = q + q't \tag{27}$$

so that q is the latent heat of freezing per mol in contact with pure solvent, while q', to a good approximation, is the difference of specific heats of solvent in liquid and solid state. It is not likely that a term in t^2 will be needed with our present data, but can be introduced if wanted. It is important to notice that q and q' are not functions of concentration, by definition. Inserting values of H and $Q_\circ$ on the right side of the identity (20) and performing the integration we have (putting $t = T - T_\circ$),

$$\frac{1}{T}(\varphi_\circ + P_\circ - f) + P_1 - R\ln(1 + \Sigma c) + P_2T = \frac{q - q'T_\circ}{T} - \frac{H_\circ}{T} +$$

$$H_1\ln T + H_2T + P_1 - R\ln(1 + \Sigma c) + K - q'\ln T; \tag{28}$$

Comparing like terms on the two sides of this identity, we see that on the left the only term involving T^{-1} and varying with concentration is $P_\circ/T$ while on the right the only like term is $-H_\circ/T$, hence $H_\circ = -P_\circ$. Again, on the left there is no term in $\ln T$ varying with concentration, hence $H_1 = 0$. Terms in T give $H_2 = P_2$. Thus

$$H = -P_\circ + P_2T^2$$

which checks with (24). Collecting results, and substituting in (22) we have the equation of the melting-point curve as

$$\frac{q - q'T_\circ}{T} + \frac{P_\circ}{T} + P_2T - R\ln(1 + \Sigma c) + K + P_1 - q'\ln T = 0 \tag{29}$$

where the constant K is yet undetermined. When $T = T_\circ$ the quantities $P_\circ$, P_1 and P_2 are zero since the concentration is zero. This gives for K

$$K = \frac{q}{T_\circ} + q' + q'\ln T \tag{30}$$

whence the melting-point equation by a little arrangement is

$$RT\ln(1 + \Sigma c) - P(c,T) = q\left[1 - \frac{T}{T_\circ}\right] - q'(T - T_\circ) - q'T\ln\frac{T}{T_\circ} \tag{31}$$

10. *Discussion of the melting point equation.*—Equation (31) departs from strict generality only in so far as we have failed to expand to higher powers of T. In principle it is entirely rigorous, and whenever we have data to warrant it we may expand as far as we please. It shows that a rigorous theory differs from the ordinary theory only through the function $P(c,T)$ which, if expanded in powers of the concentrations, begins with terms of the second degree, but which, if expanded in powers of T, contains a term P_1T playing no part in heat of dilution. To omit $P(c,T)$ altogether is, therefore, equivalent to something more than assuming heat of dilution to be zero.

If $P(c,T)$ be dropped, the equation becomes identical (except for slight difference of notation) with the melting-point equation established in a valuable paper by Washburn[4] on the two hypotheses:

1°. That the vapor of the solvent acts like a perfect gas.

2°. That Raoult's law holds good for the lowering of vapor pressure.

Since our equation (31) has been obtained without any recourse to vapor pressure, we may at once follow Washburn's reasoning in inverse order and get a certain amount of new light on the two assumptions about vapor pressure. For by differentiating (13) with respect to T we have

$$\frac{Q}{T_2} = F'(c)\frac{dc}{dT} \tag{32}$$

supposing there is only one dissolved substance. Using $F'(c)$ from (21) we have

$$\frac{Q}{T^2} = \left(P'_1 - \frac{R}{1+c}\right)\frac{dc}{dT} \tag{33}$$

which aside from slight difference of notation, differs from Washburn's differential equation in the presence of P'_1, which we have seen to be a function of c. Taking then the rigorous equation[5]

$$\frac{Q}{vT} = \left[\frac{\partial p}{\partial c}\right]_T \frac{dc}{dT} \tag{34}$$

where p is the vapor pressure, and eliminating dc/dT

$$\left[\frac{\partial p}{\partial c}\right]_T = \left(P'_1 - \frac{R}{1+c}\right)\frac{T}{v} \tag{35}$$

So far we have made no assumptions. If we assume $pv = RT$ we have by integrating

$$p_0 = p(1+c)e^{P_1} \tag{36}$$

a generalization of Raoult's law, to which it reduces if $P_1 = 0$. It is noteworthy that P_1 is the coefficient of precisely the term in the expansion of $P(c,T)$ which does not affect the heat of dilution. In other words the departure of the vapor from Raoult's law is dependent on the function $P(c,T)$, which affects the freezing-point, *minus* all the terms which affect the heat of dilution. Thus if heat of dilution and freezing-point are known we can find the vapor pressure, a fact also pointed out by Washburn. Equation (36) shows the precise relation of this fact to the melting-point equation provided we assume $pv = RT$. In any case (35) shows that the vapor pressure of a solution can be calculated if we know the coefficient of T in the chemical potential of the solvent, and the equation of state the vapor.

It will now be apparent that we can determine empirically the coefficients in the expansion of $P(c,T)$ and hence have a complete knowledge of the properties of the solution if we can get accurate determinations of heat of dilution at various temperature and over a considerable range of concentrations, and also have accurate freezing-point determinations at fairly high concentrations, so as to calculate the missing term P_1. It is doubtful if any such data of high accuracy now exist over a wide enough range for even one substance. If, however, the accurate methods which have been used[6] to obtain freezing-point data for very dilute solutions can be carried to higher concentrations, and if the accurate data on heat of dilution[7] which have been obtained at 25° can be carried over a moderate temperature range, there seems to be no doubt we should have a theory of solution that would be practically complete. A detailed examination of existing data in the light of the foregoing theory is now under way, but would too greatly lengthen the present paper. A single example must here suffice. Taking cane sugar, and, as a first approximation, considering only terms in the square of the concentration, i.e., dropping the cube and higher powers in the expansion of $P(c,T)$ the coefficient C_2 of this term was calculated for values of N, the number of mols sugar per 1000 grams of water. The data[8] of Morse and Fraser gave

N	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1.0	1.2681
C_2	0.110	0.122	0.098	0.119	0.119	0.118	0.121	0.133	0.115

while the data[9] of Ewan gave

N	0.8714	1.2908	1.5347	1.6427	2.1108
C_2	0.118	0.104	0.132	0.128	0.132

Raoult's data[10] gave:

N	0.5056	1.0107
C_2	0.117	0.124

Other data quoted by Landolt and Bernstein gave

N	0.760	0.4236	0.2840
C_2	0.061	0.094	0.097

It is evident that the values of C_2 from the data of these different observers do not agree well. Below about 0.3 normal the solution departs so little from the "ideal" that the calculations become meaningless, depending as they do on differences between ideal and observed numbers.

11. *Application of the foregoing theory to electrolytes.*—Let us now suppose that the only dissolved substance is a single uni-univalent salt, as KCl. Calling c the formal concentration per 1000 grams of water we shall have

$$m_\circ = 55.5, \; m_1 = m_2 = c\gamma, \text{ and } m_3 = c(1 - \gamma)$$

where γ is the fraction of salt which is ionized, m_1 and m_2 the masses of the ions in mols, and m_3 the mols of un-ionized salt. At a given temperature $P(c,T)$ becomes a function of c alone and we may write

$$P(c) = Ac^2\gamma^2 + Bc^2\gamma(1 - \gamma) + Cc^2(1 - \gamma)^2 + \ldots \qquad (37)$$

where A, B, C, ... are constants to be found. It is evident from the form of these terms that they will be without influence in very dilute solution, hence ionization as calculated from freezing-point by the ordinary formula should be exact.

12. *Generalization of the mass law.*—The case is quite otherwise, however, when we consider equilibrium between the ions and un-ionized salt. For if we let f_1 and f_2 denote the chemical potentials of the ions, f_3 that of the un-ionized salt, the general condition (4) of equilibrium gives

$$f_1 + f_2 = f_3 \tag{38}$$

as the equation which determines the ionization. Now by definition

$$f_\circ = \frac{\partial\varphi}{\partial m_\circ},\ f_1 = \frac{\partial\varphi}{\varphi m_1},\ f_2 = \frac{\partial\varphi}{\partial m_2} \text{ and } f_3 = \frac{\partial\varphi}{\partial m_4}$$

and if T be regarded as constant we may expand $P(c,T)$ in the form

$$P(c) = \frac{1}{m_\circ^2}[a_{11}m_1^2 + a_{12}m_1m_2 + \text{etc.}] + \frac{1}{m_\circ^3}[a_{111}m_1^3 + a_{112}m_1^2m_2 + \text{etc.}], \tag{39}$$

while, from (17), $f_\circ$ can be written

$$f_\circ = \varphi_\circ - RT \ln (m_\circ + m_1 + m_2 + m_3) + RT \ln m_\circ + P(c) \tag{40}$$

Taking the derivative with respect to m_1,

$$\frac{\partial f_\circ}{\partial m_1} = -\frac{RT}{m_\circ + m_1 + m_2 + m_3 +} + \frac{1}{m_\circ^2}[2a_{11}m_1 + a_{12}m_2 + a_{13}m_3]$$
$$+ \frac{1}{m_\circ^3}[3a_{111}m_1^2 + 2a_{112}m_1m_2 + 2a_{113}m_1m_3 + a_{122}m_2^2 + a_{123}m_2m_3 + a_{133}m_3^2]$$

Now we have identically

$$\frac{\partial f_\circ}{\partial m_1} = \frac{\partial^2\varphi}{\partial m_\circ \partial m_1} = \frac{\partial f_1}{\partial m_\circ}$$

whence $f_1 = \int \frac{\partial f_\circ}{\partial m_1} dm_\circ$ + terms not involving $m_\circ$. Therefore,

$$f_1 = \varphi_1 + RT \ln\left[\frac{m_1}{m_\circ + m_1 + m_2 + m_3}\right]$$
$$- \frac{1}{m_\circ}[2a_{11}m_1 + a_{12}m_2 + a_{13}m_3] - \frac{1}{2m_\circ^2}[3a_{111}m_1^2, \text{etc.}]$$

where φ_1 involves temperature (and pressure) only. The presence of the term $RT \ln m_1$ is determined from our hypothesis that the solution acts like a perfect gas to a first approximation.[11] It is noteworthy that terms in $f_\circ$ involving *squares* of concentrations lead, in f_1, to terms of the *first* degree. Expressions for f_2 and f_3 are similarly found. If we put

$$x_1 = \frac{m_1}{m_\circ + m_1 + m_2 + m_3}$$

with similar notation for x_2 and x_3, and substitute in (38) the expressions for f_1, f_2 and f_3 we shall have as the condition of equilibrium

$$\ln\left[\frac{x_1x_2}{x_3}\right] - G(c) = k \tag{41}$$

where $G(c)$ is a function of concentration, which, if expanded in series, begins with terms of the first degree in the masses of the dissolved substances. The coefficients in the expansion are functions of temperature (and pressure), and k is also a function of temperature. If we omit $P(c,T)$ in f_0 we must also omit $G(c)$ in this extended mass law (41). It is noteworthy that in the logarithmic term we have rigorously x_1, x_2, x_3, not m_1/m_0, etc., although in solutions not very concentrated it does not matter much.

13. *Discussion of the generalised mass law.*—It is shown in the foregoing theory that if the function $P(c,T)$ can be accurately determined we can at once calculate the function $G(c)$ and hence predict to what extent ionization departs from the mass law. This could be done from accurate measurement of heat of dilution, checked as to terms in T by freezing-point data. Conversely, if ionization be known, we can find $G(c)$ and hence $P(c)$ for that temperature. Many other relations can be brought to bear particularly those connecting specific heats with change in equilibrium under varying temperature.

Furthermore equation (41) can be employed to study the theoretical form of the curve obtained by plotting the "constant" K against the formal concentration c. By putting $m_1 = m_2 = c\gamma$ and $m_3 = c(1 - \gamma)$ we may write (41) as

$$K = \frac{\gamma^2 c}{1 - \gamma} = K_0 e^g \tag{42}$$

where g is a function of concentrations which may be expanded

$$g = ac\gamma + a'c(1 - \gamma) + \text{etc.} \tag{43}$$

By differentiating with respect to c we have

$$\frac{dK}{dc} = K_0 e^g \frac{dg}{dc} \tag{44}$$

while dg/dc found from (43) is

$$\frac{dg}{dc} = a\gamma + a'(1 - \gamma) + (a - a')c\frac{d\gamma}{dc} \tag{45}$$

and the value of $d\gamma/dc$ from (42) is

$$\frac{d\gamma}{dc} = \frac{[ac\gamma + a'c(1 - \gamma) - 1]\gamma(1 - \gamma)}{c[2 - \gamma + c\gamma(1 - \gamma)(a' - a)]} \tag{46}$$

Now as c approaches zero, γ approaches unity. By inspection of (46) we see that $cd\gamma/dc$ approaches zero, for the denominator approaches unity on cancelling c. Hence from (45) we see that dg/dc approaches the constant a. Hence by (44) we have

$$\text{Lim}\ \frac{dK}{dc} = aK_{\circ} \tag{47}$$

This means that we shall not naturally expect the electrolyte to obey the mass law even in very dilute solution. For this would, in strict rigor, mean that the graph of K against c should have a horizontal tangent as c approaches zero. However, a curve differing little from the horizontal would be hard to distinguish experimentally from the other.

In a highly interesting investigation Washburn has worked out this curve for KCl at extreme dilutions,[12] assuming that the graph of K shows no marked change in character as c approaches zero. To compare his results with the present theory I have assumed that his value of $\Lambda_{\circ}$ is correct and also his value 0.02 for $K_{\circ}$. Determining the function g so that the following data (from Washburn and from Adams) should be exactly satisfied:

c............	0.0004	0.001	0.005
γ............	0.98923	0.98163	0.958

the function g was found to satisfy

$$g \log_{10}e = 1075c\gamma - 47234c(1 - \gamma) + 240064c^2\gamma^2$$

Values of γ calculated from this equation were then compared with Washburn's results for round concentrations:

c...............	0.00005	0.0001	0.0002	0.0003	0.0007	
γ..............	0.99796	0.99595	0.9931	0.9910	0.98507	(Calc.)
γ..............	0.99753	0.99529	0.99256	0.99083	0.98511	(Washburn)

The curve for K plotted from the calculated values of γ lies very slightly higher than Washburn's curve at concentrations below 0.0004, but approaches 0.02 as c approaches zero. It coincides almost exactly with Washburn's curve above 0.0004 differing from his curve in having but slight inflection. An inspection of his diagram makes it clear that a curve meeting the vertical axis at a small angle with the horizontal, and having but little inflection still fits his data very well, and, of course, would fit entirely with his supposition that no marked change occurs in the character of the curve at infinite dilution. It is highly probable, too, that the empirical value of g given above is not the best that can be found. Further investigation is now being conducted.

14. *Summary.*—The adoption of Gibbs' principle of chemical potentials leads to an extention of the ordinary theories of melting-point, heat of dilution, vapor pressure, and mass law. In all cases these extensions are the result of the presence, in the expression for the chemical potential of the solvent, of terms in the square and higher powers of the concentrations. In many phenomena these terms are without measurable effect in dilute solution. They lead in the case of the mass law to terms in the first

power of the concentrations, hence are important even in very dilute solutions.

[1] Duhem, *Méchanique Chimique,* Livre VI, Chap. 111. The function $T\partial\varphi/\partial T - \varphi$ is identically equal to Gibbs' heat function $U + pv$.

[2] *Ibid.*

[3] Van Laar, *Sechs Vortrage u. d. thermodynamische Potential,* S. 84.

[4] *Technology Quart., Boston,* **21,** 1908 (372).

[5] *Ibid.,* p. 371.

[6] Adams, L. H., *J. Amer. Chem. Soc.,* **37,** 1915 (481).

[7] MacInnes and Braham, *Ibid.,* **39,** 1917 (2110).

[8] *Amer. Chem. Jour.,* **34,** 1905; **37, 38,** 1907.

[9] *Zs. phys. Chem.,* **31,** 1899; and **27,** 1898.

[10] Duhem, *loc. cit.*

[11] These PROCEEDINGS, **3,** 1917 (569); see also *J. Amer. Chem. Soc.,* **40,** 1918 (106–158).

[12] These PROCEEDINGS, **3,** 1917 (574) and *J. Amer. Chem. Soc.,* **40,** 1918 (145).

THE FORMATION OF THE CELL PLATE IN THE CAMBIUM OF THE HIGHER PLANTS

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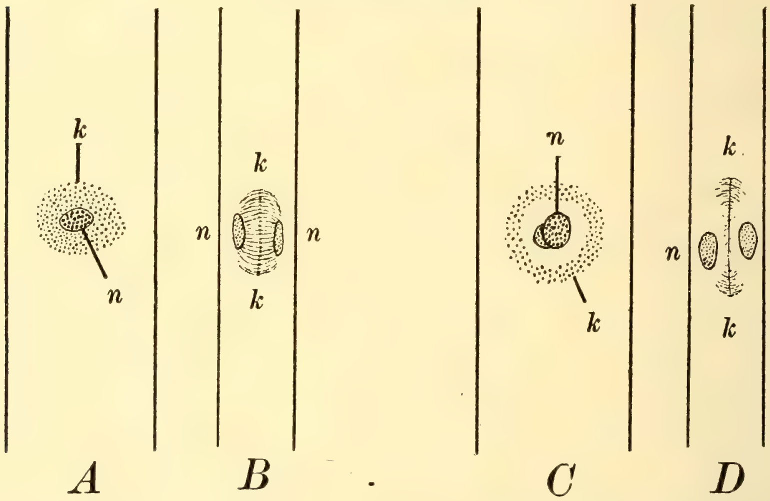
In a previous note[1] the writer called attention to a remarkable type of cytokinesis that occurs in the cambium of Coniferae. The process of cell plate formation is greatly extended, both as regards space and time and is clearly dissociated, except in its initial stages, from the usual phenomena of karyokinesis. Since this type of cell division promises to be of considerable significance in the study of various cytological and physiological problems[2] it is desirable to determine whether it is an isolated phenomenon, i.e., confined to the Coniferae, or one that is characteristic of the cambia of all of the higher plants, angiosperms as well as gymnosperms.

During the last growing season, I secured specimens of the cambium from an extensive series of angiosperms. Selections were made so as to include representatives of all of the larger and more important orders of the dicotyledons and of certain arborescent monocotyledons which have "secondary" growth in thickness. Specimens were obtained from both tropical and temperate environments.

Although the cambial initials in mature stems of angiosperms are on an average considerably smaller than homologous elements of gymnosperms[3] the salient features of cytokinesis are the same in both subphyla. In the former, as in the latter group, each initial contains a single nucleus which is centrally located and divides mitotically. The spindle becomes extended laterally by the addition of peripheral "fibers"[4] and gradually assumes the form of a disk, figures *A* and *B*. As more fibers are successively added the original "connecting fibers" disappear from about the cell plate, leaving a ring-shaped aggregation of kinoplasmic fibers, which in tangential longitudinal sections of the cambium appears as a halo about the daughter nuclei, figure *C*. This ring increases in circumference until it intersects the radial walls, when it becomes more or less flattened on two sides. As soon as the cell plate intersects the radial walls of the cell, the fibers in these two sides disappear leaving two separate aggregations of kinoplasmic fibers which are connected by the cell plate, figures *E*, *F*, *G*, *H*. These aggregations of fibers, kinoplasmasomes, have the same characteristic shape and structure as those which occur in the Coniferae. They extend across the cell—more or less at right angles to its longitudinal axis—from one radial wall to the other, figure *G*, and are located in the center of the protoplast, midway between its tangential surfaces, figure *H*. They move in opposite directions—*usually at equal rates*—towards the ends of the cell. As they move forward, the cell plate is extended until it eventually reaches the two ends of the protoplast, thus dividing it into halves, each of which contains one of the daughter nuclei. The latter usually remain close together near the center of the cell during cytokinesis, as in the gymnosperms; and I have been unable to find any visible connection between them and the kinoplasmasomes.

Robinia Pseudo-Acacia L.



A. Central portion of cambial initial, in tangential longitudinal extension, showing early "disk" stage in the formation of the cell plate. X 900.

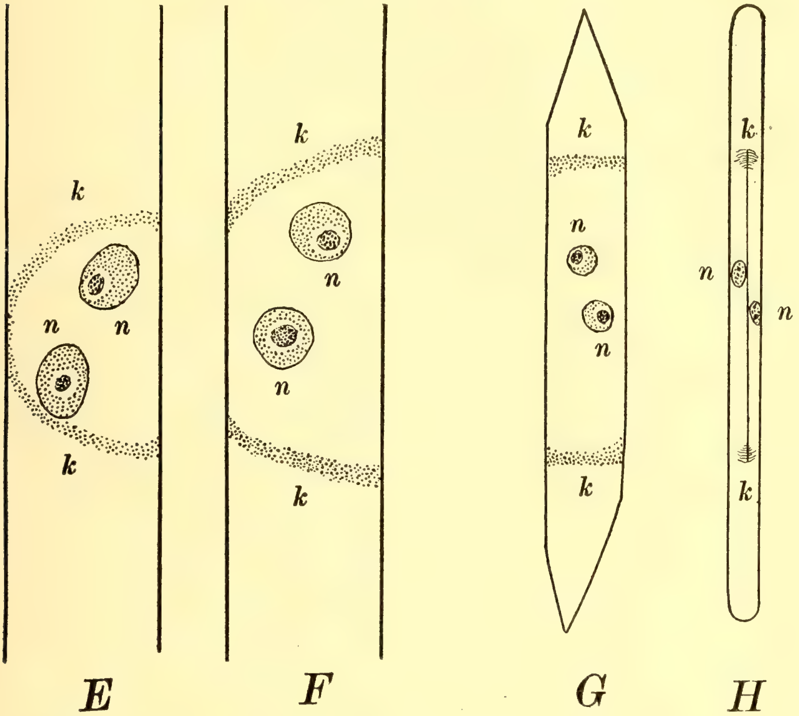
B. The same. Radial longitudinal extension. X 900.

C. Central portion of cambial initial, in tangential longitudinal extension, showing "ring" or "halo" stage in the formation of the cell plate. X 900.

D. The same. Radial longitudinal extension. X 900.

Having determined that this type of cytokinesis is characteristic of the cambia of both gymnosperms and angiosperms, it is of interest to inquire whether it ever occurs in other somatic tissues, and what relation it bears to those types of cell plate formation which previously have been described by Treub,[5] Strasburger[6] and Schürhoff.[7] The writer has accumulated considerable evidence which indicates that the phenomenon in question is not confined to the cambium, but occurs in other somatic tissues, in elongated or much flattened cells whose planes of division have one long and one short dimension. Furthermore, a comparative study of cytokinesis in different somatic tissues and in cells of different shapes and sizes suggests that the various types of cell plate formation, described by Treub, Strasburger, Schürhoff and the writer, are but different phases or stages of a single general or fundamental type of cytokinesis. The particular expressions of the phenomenon which may occur in any given

Robinia Pseudo-Acacia L.



E. Central portion of cambial initial, in tangential longitudinal extension, showing disappearance of two arcs of the kinoplasmic ring. X 900.

F. Central portion of cambial initial, in tangential longitudinal extension, showing early kinoplasmasome stage in the formation of the cell plate. X 900.

G. Cambial initial, in tangential longitudinal extension, showing later kinoplasmasome stage in the formation or the cell plate. X 450.

H. The same. Radial longitudinal extension. X 450.

n. Daughter nucleus.
k. Kinoplasma.

cell are dependent upon the dimensions of the latter, its plane of division and the size and location of the nucleus. Thus, in very small, isodiametric cells, having a large centrally located nucleus, the cell plate quickly intersects the walls of the cell, without any extensive laterial growth—by means of additional peripheral fibers—of the spindle. In somewhat larger elements there is sufficient room for the process of cytokinesis to reach the "halo" stage before the cell plate intersects the sides of the cell. Only in elongated or much flattened elements, i.e., cells which are not isodiametric, is it possible for the phenomenon of cell plate formation to pass through the "spindle," "disk," "halo" and "frame" stages and finally form two entirely separate aggregations of kinoplasmic fibrillae, the kinoplasmasomes.

It is evident, accordingly, that the remarkable type of cell plate formation which was described in a former note, is not an isolated or unusual phenomenon, but is of frequent occurrence in the somatic tissue of the higher plants, gymnosperms and angiosperms. It promises to be significant in any general discussion concerning the dynamics of cytokinesis and karyokinesis.

[1] Phenomena of cell division in the cambium of arborescent gymnosperms and their cytological significance, these Proceedings, **5,** 1919 (283–285).

[2] Bailey, I. W., The significance of the cambium in the study of certain physiological problems, *J. Gen. Physiol.* Ined.

[3] The average length of the cambial initials in 152 stems of gymnosperms was computed as approximately 3400 micra; in 275 stems of dicotyledons as approximately 600 micra.

[4] The word fibers is used without reference to whether the fibrillae are true threads or lines of protoplasmic streaming in the living cell.

[5] Treub, M., "Quelques recherches sur le rôle du noyau dans la division des cellules végétales," *Verh. K. Akad. Wetensch. Amsterdam,* **19,** 1879 (1–35).

[6] Strasburger, E., *Zellbildung und Zelltheilung,* 3 aufl, Jena, 1880.

[7] Schürhoff, P., "Das Verhalten des Kernes im Wundgewebe," *Beih. Bot. Central.,* **19,** 1906 (359–382).

FUNCTIONALS INVARIANT UNDER ONE-PARAMETER CONTINUOUS GROUPS OF TRANSFORMATIONS IN THE SPACE OF CONTINUOUS FUNCTIONS

By I. A. Barnett

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1. *Some general notions.*—It is intended in this abstract to give a few examples of one-parameter continuous groups of transformations in function space which are analogues of well-known groups in the space of a finite number of dimensions, and to exhibit in each case a functional invariant in terms of which every invariant of the group (with suitable restrictions) is expressible. Some of these groups have already been con-

sidered by G. Kowalewski,[1] although, so far as the writer knows, no attempt has been made to discover the invariants of the group.

Let u or $u(\xi)$ be a real continuous function defined on the interval $a \leqq \xi \leqq b$, and τ a real variable ranging over the interval $|\tau - \tau_0| \leqq c$. Consider also a functional operation $F[x,u,z]$ which, for every element (x, u, τ) of the set defined by

$$a \leqq x \leqq b, \quad \max_{\xi} | u(\xi) - u_0(\xi) | \leqq \alpha, \quad |\tau - \tau_0| \leqq c$$

determines a real number. If, furthermore, F is a continuous functional[2] of its arguments, then for each τ, one may regard F as a transformation taking the continuous function u into another continuous function $\bar{u}$ of the variable x,

$$\bar{u}(x) = F[x,u,\tau] \tag{1}$$

As usual, the transformations (1) are said to form a group if the product of every two transformations of the set is in the set, and if, furthermore, the set contains the identity transformation and for every transformation the corresponding inverse transformation.

An infinitesimal transformation is defined by the equation

$$\frac{\partial u(x,\tau)}{\partial \tau} = f[x,u], \tag{2}$$

where

$$f[x,u] = \frac{\partial}{\partial \tau} F[x,u,\tau]_{\iota = \iota_0}$$

τ_0 denoting the parameter giving the identity transformation of (1). If one supposes that the functionals have suitable properties of continuity and differentiability,[2] one can show that equation (2) determines a one-parameter continuous group of transformations.

By a method exactly like that employed by Lie it can be shown that the functionals $G(u)$ which are finite invariants (*finite* is here used in contradistinction from *differential*) of the group (1) may be found from the solutions of the Stieltjes integral equation

$$\int_a^b f[x,u] d_x \varphi[x,u] = 0 \tag{3}$$

where the φ is defined by

$$\int_0^1 u(x) d_x \varphi[x,u] = \Phi[u],$$

and Φ is the Frêchet differential[3] of G. It can be shown that if the functional f has the properties already referred to, solutions of (3) exist.[4]

2. *Examples.*—The set of transformations

$$\bar{u}(x) = u(x) + \alpha(x)\tau = F[x,u,\tau]$$

where $\alpha(x)$ is an arbitrary but specified continuous function evidently represents a group in function space with $\tau = 0$ as the identity parameter. The infinitesimal transformation is given by the equation

$$\frac{\partial u(x,\tau)}{\partial \tau} = \alpha(x).$$

Furthermore, every invariant satisfying suitable restrictions of continuity and differentiability may be expressed as a functional of the invariant

$$I[x,u] = \begin{vmatrix} u(x) & \int_a^b u(\xi)d\xi \\ \alpha(x) & \int_a^b \alpha(\xi)d\xi \end{vmatrix}.$$

The transformations

$$\bar{u}(x) = \tau u(x) = F[x,u,\tau]$$

form a group. The infinitesimal transformation is

$$\frac{\partial u(x,\tau)}{\partial \tau} = u(x),$$

and every invariant is a functional of[5]

$$I[x,u] = \frac{u(x)}{\int_a^b u(\xi)d\xi}.$$

It is easily verified that the transformations

$$\bar{u}(x) = u(x)e^{-\tau} + (e^{\tau} - e^{-\tau})\int_0^1 u(\xi)d\xi = F[x,u,\tau]$$

form a group with $\tau = 0$ giving the identity transformation. The infinitesimal transformation is defined by the integro-differential equation

$$\frac{\partial u(x,\tau)}{\partial \tau} = -u(x) + 2\int_0^1 u(\xi)d\xi.$$

Every invariant is a functional of

$$I[x,u] = u(x)\int_0^1 u(\xi)d\xi - \left\{\int_0^1 u(\xi)d\xi\right\}^2.$$

Consider the group defined by the infinitesimal transformation

$$\frac{\partial u(x,\tau)}{\partial \tau} = \int_0^{2\pi} \sin \sigma(x - \xi)u(\xi)d\xi$$

where σ is an odd integer. It can be shown[6] that the finite group may be put in the form

$$\bar{u}(\quad = u(x) + \frac{1}{2\pi}\left[e^{-\sigma ix}(e^{-\pi\iota\tau} - 1)\int_0^{2\pi} e^{\sigma\iota\xi}u(\xi)d\xi \right.$$
$$\left. + e^{\sigma ix}(e^{\pi ix} - 1)\int_0^{2\pi} e^{-\sigma\iota\xi}u(\xi)d\xi\right] = F[x,u,\tau]$$

Every invariant is a functional of the invariant

$$I[x,u] = u(x) + \frac{1}{2\pi}\left[1 - \int_0^{2\pi} e^{\sigma\iota\xi}u(\xi)d\xi\right]\left[e^{-\sigma ix} - e^{\sigma ix}\int_0^{2\pi} e^{-\sigma\iota\xi}u(\xi)d\xi\right].$$

Some interesting special invariants are obtained when the particular invariant functionals

$$\int_0^{2\pi} e^{-\sigma ix} I[x,u]dx \text{ and } \int_0^{2\pi}\left\{I[x,u]\right\}^2 dx$$

are taken in which cases one obtains, respectively, the invariants

$$\left[\int_0^{2\pi}\cos\sigma\xi u(\xi)d\xi\right]^2 + \left[\int_0^{2\pi}\sin\sigma\xi\, u(\xi)d\xi\right]^2, \int_0^{2\pi} u^2(x)dx.$$

This group is a special instance of the orthogonal group in function space[1] defined by the infinitesimal transformation

$$\frac{\partial u(x,\tau)}{\partial\tau} = \int_a^b K(x,\xi)u(\xi)d\xi,$$

where $K(x,\xi)$ is a skew-symmetric kernel; i.e., $K(x,\xi) = -K(\xi,x)$. The invariants of this group are quite complicated and will not be given.

As a last example, let a group have the last mentioned infinitesimal transformation where the given kernel function $K(x,\xi)$ is continuous and symmetric. The finite transformations may be put in the form[6]

$$\bar{u}(x) = u(x) + \sum_{n=1}^{\infty}\varphi_n(x)(e^{\frac{\tau}{\lambda_n}} - 1)\int_a^b \varphi_n(\xi)u(\xi)d\xi = F[x,u,\iota\tau],$$

where the constants λ_n are the characteristic numbers of the kernel $K(x,\xi)$ and the functions $\varphi_n(\xi)$, are the corresponding normed orthogonal set of characteristic functions. Every invariant is a functional of

$$I[x,u] = u(x) + \sum_{n=1}^{\infty}\varphi_n(x)\left\{\left[\int_a^b \varphi_n(\xi)u(\xi)d\xi\right]\frac{1 - \left[\int_a^b \varphi_1(\xi)u(\xi)d\xi\right]^{\frac{\lambda 1}{\lambda n}}}{\left[\int_a^b \varphi_1(\xi)u(\xi)d\xi\right]^{\frac{\lambda 1}{\lambda n}}}\right\}.$$

If the particular invariant functional

$$\int_a^b I[x,u]\varphi_k(x)dx, \quad k \pm 1,$$

is taken, one obtains the invariants

$$\frac{\int_a^b \varphi_k(\xi)u(\xi)d\xi,}{\left[\int_0^1 \varphi_1(\xi)u(\xi)d\xi\right]^{\frac{\lambda 1}{\lambda k}}}, k \pm 1.$$

One could now try to extend some other results of the theory of continuous groups to functions both for one parameter and for several parameters. For example, it would be interesting to know what the invariant manifolds of a given group are; also the applications to partial differential equations in infinitely many variables, which the writer is now engaged in investigating. Finally, groups in which the functions $u(x)$ are discontinuous would seem to be worthy of consideration.

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[1] G. Kowalewski, *Sitz. ber. Math. Akad. Wiss. Wien,* **120,** Abteilung II*a*¹, II*a*², 1911 (77–109,1435–72).

[2] See writer's doctor's dissertation (Chicago, 1918).

[3] *Trans. Amer. Math. Soc.,* **15,** 1914*r* (139).

[4] In a paper by the writer soon to be published on Linear Partial Differential Equations with a Continuous Infinitude of Variables.

[5] This formula has already been obtained by Volterra as the general solution of a functional differential equation in his paper, *Atte R. Accad. Lincei,* Ser. 6, **23,** 1914, 1st semester, (p. 393.)

[6] See paper by writer entitled, Integro-differential Equations with Constant Kernels, *Bull. Amer. Math. Soc.,* **26,** 1920 (193).

THERMOKINETICS OF LIOMETOPUM APICULATUM MAYR

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Variation in the activity of a cold-blooded animal is largely dependent on metabolic changes, which in turn probably depend mainly on the accelleration of oxidation and of other chemical reactions. The physical nature of the environment affects most of these chemical processes, and we should expect that the same physical properties would also directly influence the kinetic manifestations of animal life. While many observations record the qualitative relation of animal activity to such factors as humidity and external temperature, only a few give definitive numerical results.

An opportunity for precise quantitative measurement of the variation in the rate of expenditure of animal energy under the different natural stimuli arising from changing meteorological conditions, is afforded by the study of the foraging and patrolling activities of ants of the subfamily Dolichoderinae. Although some of my observations have been made on other genera of this group (*Tapinoma, Dorymyrmex*), I find the most suitable material to be the "Argentine ant," *Iridomyrmex humilis* Mayr, and two species of *Liometopum.* In briefly describing the activities of these ants and the observations based upon them, a number of reasons are proposed in the following paragraphs why members of the subfamily, and particularly the California *Liometopa,* are thought to be better adapted than nearly any other organism for the close quantitative investigation of kinetic response to changes in field conditions.

The trail-running habit, which is common to all genera of Dolichoderinae except *Leptomyrmex* of Australia,[1] is fundamental for the observations discussed below. It permits permanent observing stations to be set up along the trail so that the speed over fixed intervals of distance can be measured under different conditions of light, air-temperature, relative humidity, wind-velocity, and barometric pressure, and at different times of the day and during all seasons of the year. The trails are maintained for long intervals of time, many of those of *Liometopum apiculatum* and *L. occidentale* on Mount Wilson have not changed during two years of observation. Colonies of the closely related *L. microcephalum* of Europe are also known to maintain constant trails for several years.[2] Ants running in these files, whether toward or away from the nest, are for the most part unburdened, their function apparently being mainly that of patrol.

Another characteristic of the *Liometopa*, which is very fortunate for this research, is that they are equally diurnal and nocturnal in habit—a characteristic that permits continuous series of observations as well as favorable opportunities for the investigation of natural phototactic reactions.

The conspicuous differences in stature of the workers of *Liometopum* allow the possible correlation of activity and hardihood with size. The average live weight of a worker maxima of *L. apiculatum* is, according to my measures, about five hundredths of a grain (140,000 to a pound); the average worker minor weighs half as much, but the extremes of weight for maxima and minima are probably in the ratio of four to one.

The wide range of temperature, humidity, wind, and light, throughout which these ants are active, enables a thorough testing of the extent to which the various meteorological factors are involved in kinetic reactions. The files are maintained with the relative humidity near its maximum of 100%, or when as low as 5%; the patrols are running when the temperature is 38° centigrade (over 100° Fahrenheit) and also when it is less than 8°. After a few warm days late in December thousands of ants of two species of *Liometopum*, on Mount Wilson, were out of their nests only a few feet from banks of snow.

There is a distinct advantage for thermokinetic studies in the comparative freedom of the patrolling *Liometopa* from the erratic motions which are characteristic of many kinds of insects, or which are in fact characteristic of most active animals of all the higher phyla. It is this common irregularity and discontinuity of action that usually prevents quantitative measurement of the kinetic responses of cold-blooded animals. A fixed patrol of Dolichoderines from a populous colony, however, is essentially a steady flow of animal energy; its quantity is governed largely by food and its speed by meteorological conditions. Except at low temperatures the normal speed of running appears to be very near the maximum speed possible under the prevailing conditions. For temperatures below 15° centigrade the excitation of battle or food temporarily increases activity.

The California *Liometopa* are carnivorous, granivorous, and aphidicolous. This catholicity of taste permits successful baiting for the purpose of maintaining the density of a file, for the establishment of new trails over some special terrain, or for other experiments.

The large population of the nests makes possible the establishment of files of sufficient density for statistical treatment of the observations of speed. The population of colonies of *Liometopum* and of *Iridomyrmex humilis* is to be numbered by the tens of thousands. On an average patrol during the warmer months, 100 ants pass a given point in one minute (see table III, p. 209). From one nest on Mount Wilson, in the summer of 1919, four files issued, and every day, under favorable conditions of weather, 70,000 ants went out along each file and nearly 70,000 ants came in.

The observations tabulated below were made on Mount Wilson (altitude 5700 feet) on ants from a nest of *L. apiculatum*, located in the basal, slightly-decayed trunks of a group of *Quercus chrysolepis* Liebm. This species of *Liometopum* is not assigned to the state in Wheeler's "Mountain Ants of Western North America."[3] The subspecies *luctuosum* Wheeler, however, has been taken on nearby mountains[4] and probably occurs on Mount Wilson. In a discussion of California ants, Woodworth[5] states that "Liometopum is a very rare insect," it is probably uncommon, therefore, in many parts of the state, but in the San Gabriel mountains it is by far the most numerous ant, with the species *occidentale* Emery predominating.[6]

Of the nine different "speed traps" set up for the study of the nest of *L. apiculatum*, Stations A and Y were principally used. The former is at the base of a concrete wall some 50 feet from the nest; the latter, on another trail, is on a plank surface about 2 feet from the main nest openings. These two pieces of trail are of comparable smoothness, and each is 30 centimeters in length. The time required for individuals to pass through the speed traps was recorded (with a stop watch for high temperatures), together with an indication of the size of ant and its direction of travel. At the beginning and end of each observation, which is the mean time in seconds for about 20 individuals, the temperature was read from a thermometer, the bulb of which was placed against the wall or board, some 10 or 15 millimeters from the trail.

As the observations are made within a few yards of a special station maintained by Mr. Hoge for the United States Weather Bureau, we have available, for all the observations on speed, continuous graphical records of the precipitation, wind-velocity, temperature, and barometric pressure, as well as semi-daily cloud and psychrometric observations.

It was found from the first observations that the kinetic response for these ants is essentially a function of temperature alone—other meteorological conditions (except rain, of course) having very little if any direct effect on speed. Even the influence of light and time of day are extremely small compared with the influence of temperature. If the speed is dependent mainly on the velocity of chemical reactions, this result should be expected.

The quantities in the sixth column of table I are the observed mean time required for an ant to travel 30 centimeters, at the corresponding temperature, and the probable error of that mean; uncertainties of timing, frequently contribute nearly a tenth of a second to the probable error. The seventh column contains v, the average speed, reduced to centimeters per second.

Temperature is plotted against speed in figure 1. Observations made at station A are plotted as dots, and those made at station Y as crosses. Two observations of low weight (Nos. L2A and L27A) are plotted as open circles; the recorded temperatures are undoubtedly high, because the timing of ants was begun too soon after introducing the cardboard shade and the artificial lowering of temperature is responded to much more quickly by the ants than by the mercury thermometer. (Observations made in January 1920 at stations F and G confirm the supposition that the two measures in question were erroneous and should be rejected in a mathematical discussion of the curve.)

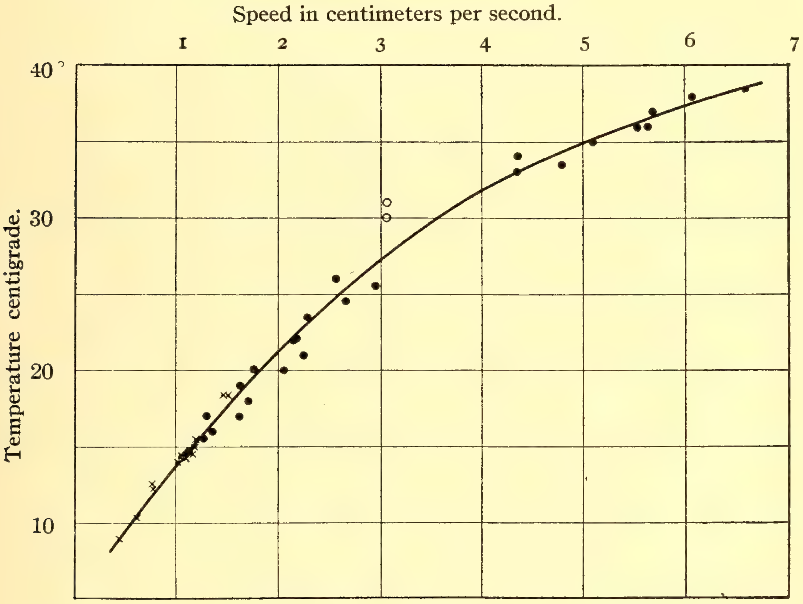

FIG. 1. The temperature-speed curve for *Liometopum apiculatum*.

The quantities in the eighth and ninth columns of table I are, respectively, the percentage difference in speed for inbound and outbound ants, and the percentage difference in speed for large and small workers. These observations of systematic differences were designed to give evidence of the effect on speed of fatigue and mass. A positive value of $(i - o)/0.01v$

indicates that during that observation ants travelled faster toward the nest than away. A positive value of $(M - m)/0.01v$ indicates a greater average speed for the larger workers. The percentage differences are grouped in table II in order of decreasing temperature.

TABLE I

OBSERVATIONS OF TEMPERATURE AND SPEED

Obsn.	Date (1919)	Time (P. S. T.)	Temp. (Cent.)	No. Ants.	Seconds and P. E.		Speed (cm/sec)	$\frac{i - o}{0.01v}$	$\frac{M - m}{0.01v}$	Remarks
L 1A	May, 25	11:20 A.M.	34°,	22	6.9	± 0.1	4.35	— 4	— 1	Sunshine
L 2A		11:35 A.M.	30,	18	9.8	0.3	3.05	— 5	+ 4	Shaded with cardboard
L 3A		11:46 A.M.	35,	21	5.9	0.1	5.08	— 5	— 3	Sunshine
L12A*		5:25 P.M.	21,	20	13.4	0.3	2.23	+13	+ 6	Tree-shadows
L13A		8:35 P.M.	16	18	22.1	0.2	1.36	+ 4	— 2	Dark
L16A		10:38 P.M.	15.5	17	23.5	0.5	1.27	+17	— 1	Dark
L17A*	26	1:28 A.M.	14.5	15	27.3	0.7	1.10	+ 1	— 2	Dark
L20A		4:00 A.M.	14.5	18	27.2	0.6	1.10	+ 6	— 3	Dawn beginning
L22A		0:55 P.M.	38.5	42	4.55	0.05	6.60	— 9	+ 7	Sunshine
L23A*	27	2:25 A.M.	17,	19	23.5	0.4	1.28	+ 9	+ 5	Dark
L25A*		10:55 A.M.	33.5	26	6.3	0.1	4.77	+ 8	0	Sunshine
L27A*		11:25 A.M.	31,	24	9.8	0.2	3.06	— 4	+ 1	Shaded with cardboard
L28A*		11:40 A.M.	37,	22	5.29	0.08	5.67	+ 6	+10	Sunshine
L29A		4:47 P.M.	26,	24	11.7	0.2	2.56	— 1	+ 2	Tree-shadows
L31A		5:24 P.M.	23.5	18	13.1	0.2	2.29	— 8	— 7	Cloudy
L32A		6:15 P.M.	22,	22	13.8	0.3	2.17	—13	+ 8	Twilight, cloudy
L33A		8:15 P.M.	20,	20	16.1	0.2	1.86	— 6	0	Dark
L35A		11:42 P.M.	18,	24	17.6	0.5	1.70	+12	+ 2	Dark, two hours after rain
L36A	28	9:22 A.M.	20,	24	14.7	0.3	2.04	0	— 7	Cloudy
L38A	June, 4	11:24 A.M.	36,	22	5.43	0.08	5.62	— 6	— 1	Sunshine
L39A		11:40 A.M.	38,	20	4.95	0.07	6.06	— 2	+ 8	Sunshine
L40A		11:53 A.M.	36,	21	5.43	0.13	5.52	— 3	— 3	Shaded with glass
L41A		0:02 P.M.	33,	20	6.95	0.13	4.32	— 3	— 1	Shaded with cardboard
L44A		4:40 P.M.	25.5	25	10.2	0.2	2.94	+ 5	0	Tree-shadow
L46A		5:11 P.M.	24.5	20	11.3	0.2	2.65	+10	+12	Tree shadows
L47A		10:55 P.M.	17,	20	18.6	0.4	1.61	+ 8	— 2	Dark
L48A	5	9:20 A.M.	19,	26	18.5	0.4	1.62	— 9	+ 7	Shadow of wall
L50A*		10:05 A.M.	22,	19	13.9	0.4	2.16	—19	— 2	Shadow of wall
L54Y	Dec. 21	1:40 P.M.	14,	20	29.6	0.9	1.01	+12	+14	Tree-shadows
L56Y		2:16 P.M.	14.5	24	25.8	0.4	1.16	— 8	+19	Weak sunshine
L57Y		3:44 P.M.	12.6	20	39.0	1.1	0.77	— 6	+12	Tree-shadows
L58Y		4:36 P.M.	10.3	20	46.9	1.6	0.64	+27	+ 4	Weak sunshine
L59Y		6:15 P.M.	9.0	16	68.1	2.6	0.44	+11	+15	Dark
L60Y	22	11:42 A.M.	14.5	20	27.0	0.8	1.11	—20	+ 6	Tree-shadows
L61Y		11:56 A.M.	14.5	20	28.5	1.0	1.05	—25	+15	Tree-shadows
L62Y		0:47 P.M.	15.5	22	25.4	0.8	1.18	—14	0	Tree shadows
L63Y		3:40 P.M.	18.0	21	25.9	0.8	1.16	— 9	+ 9	Mottled shadows
L64Y	23	2:47 P.M.	18.6	20	20.2	0.5	1.48	— 2	+ 2	Weak sunshine
L65Y		4:08 P.M.	18.5	20	20.8	0.6	1.44	+13	+ 3	Weak sunshine
L66Y	24	7:15 A.M.	12.5	20	38.7	0.9	0.77	— 4	+ 5	Shadows at sunrise

TABLE II

RELATION OF SPEED TO SIZE AND DIRECTION OF TRAVEL

INTERVAL OF TEMPERATURE	NUMBER OF OBSERVATIONS					Mean of $\frac{i - o}{0.01v}$	Mean of $\frac{M - m}{0.01v}$
	Total	Inbound Workers		Major Workers			
		Fast	Slow	Fast	Slow		
Station A							
38.5° to 30.5°	10	2	8	4	5	—2.2	+1.7
30.5 to 22.5	5	2	3	3	1	+0.2	+2.2
22.5 to 14.5	13	8	3	5	7	+3.5	+0.7
Total range	28	12	14	12	13	+0.8	+1.3
Station Y							
18.6 to 9.0	12	4	8	11	0	—2.1	+8.7

TABLE III

OBSERVATIONS OF TEMPERATURE AND AVERAGE FREQUENCY FOR 4570 ANTS

Observation	Date (1919)	TIME (P. S. T.)	TEMP. (CENT.).	INBOUND		OUTBOUND		Ratio of frequencies N_i/N_o
				Number	Frequency N	Number	Frequency N_o	
L 4A	May 25	0:55 P.M.	37°	150	0.95	150	1.14°	0.83
L 8C		3:46 P.M.	34.5	250	1.58	250	1.33	1.19
L11A		5:15 P.M.	21.5	250	2.05	250	1.27	1.61
L14A		8:40 P.M.	16	150	1.32	150	1.04	1.27
L15A		10:30 P.M.	15.5	100	0.88	100	0.65	1.35
L18A	26	1:37 A.M.	14.5	150	0.80	150	1.21	0.66
L19A		3:59 A.M.	14	250	0.94	250	0.82	1.15
L21A		0:56 P.M.	38.5	125	0.44	125	0.42	1.05
L24A	27	2:35 A.M.	17	125	0.87	125	0.48	1.81
L26A		11:10 A.M.	36	100	0.41	100	0.86	0.48
L30A		5:00 P.M.	25.5	200	1.42	200	1.27	1.12
L34A		8:25 P.M.	20	150	1.14	150	1.36	0.84
L37A	28	9:30 A.M.	19.5	110	0.74	110	0.70	1.06
L45A	June 4	4:51 P.M.	25.5	100	0.85	100	0.85	1.00
L51A	5	10:12 A.M.	23	50	0.20	100	0.76	0.26

In order to determine the relation of number of ants running to time of day and to temperature, the series of observations were made at station A which are recorded in table III and summarized for time of day and temperature in tables IV and V. The frequencies N_i and N_o show the average number of inbound and outbound ants passing a given point on the trail per second—a quantity obtained by dividing the number of ants, fifth and seventh columns, into the intervals of time required for their passage. When the ratio N_i/N_o exceeds unity, more ants are going in

TABLE IV

FREQUENCY AND TIME OF DAY

TIME OF DAY	NUMBER OF OBSERVATIONS	MEAN FREQUENCY		
		In	Out	Ratio
0 to 6 A.M.	3	0.87	0.84	1.04
6 to 12 A.M.	3	0.45	0.77	0.58
0 to 6 P.M.	6	1.22	1.05	1.16
6 to 12 P.M.	3	1.11	1.02	1.09
All	15	0.97	0.94	1.03*

TABLE V

FREQUENCY AND TEMPERATURE

TEMPERATURE (Centigrade)	NUMBER OF OBSERVATIONS	MEAN FREQUENCY		
		In	Out	Ratio
14.5° to 22.5°	8	1.09	0.94	1.16
22.5 to 30.5	3	0.82	0.96	0.86
30.5 to 38.5	4	0.84	0.94	0.90
All	15	0.97	0.94	1.03*

* If the terminal territories of different files do not overlap, this ratio in the long run should be unity or less.

than out. In tables IV and V the quantity N_i/N_o is the ratio of average frequencies for the corresponding intervals of time or temperature; it has a different and better meaning than the average ratio of frequencies for the same intervals.

The following summary of conclusions may be based upon the tables and figure; a discussion of the properties of the temperature-velocity curve is reserved for a later communication.

1. From the measurement of the speed of a thousand individuals of the species *Liometopum apiculatum*, an empirical curve is obtained that for any temperature throughout a range of 30° centigrade gives the speed with an average probable error of 5% for one observation. Conversely, from a single observation of the ant-speed, the temperature can be predicted within 1 degree centigrade.

2. The activity is less erratic for higher temperatures. Thus, the percentage probable error, derived for each observation from the deviations of the individual timings, decreases on the average with increasing temperature and speed from about 4% to less than 2%; but for only one observation does the probable error exceed a millimeter a second.

3. As the temperature rises 30° centigrade the speed changes fifteen fold, increasing uniformly from 0.44 to 6.60 centimeters a second. For a large species of *Eciton*, probably *burchelli*, observed by Beebe in British

Guiana, a normal speed of 7.6 centimeters a second is recorded,[7] and on a later occasion as speed of 15.2 centimeters a second.[8]

4. There appears to be little, if any, real difference in speed towards and away from the nest. Large and small workers have practically the same speed for all temperatures during the summer months; after two months of low temperature, the large workers are conspicuously more active than the small workers (ninth column of table I).

5. This species of ant runs as numerously in the night as during daylight hours; maximum activity falls between noon and midnight, according to the scarcely sufficient observations summarized in table IV. Within the limits of 14° and 38° centigrade, temperature appears to have little effect on the number of ants running in the files.

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THE INFLUENCE OF IONS ON THE OSMOTIC PRESSURE OF SOLUTIONS

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I. The osmotic pressure of solutions is usually measured indirectly, namely, by the determination of the freezing-point. Direct measurements have thus far been rare for the reason that it is difficult to prepare membranes permeable for water but not for the solute. The measurements thus far obtained on non-electrolytes by Morse,[1] and by the Earl of Berkeley[2] and their collaborators show that the actual values observed are not far from those expected according to van't Hoff's theory.

The difficulties in the preparation of semipermeable membranes are considerably diminished when we substitute colloidal solutions for solutions of crystalloids. By using solutions of proteins, collodion membranes satisfy the demand of semipermeability.

Gelatin is a very convenient protein for the purpose of such experiments. Like proteins in general, it is an amphoteric electrolyte being capable of forming salts with acids as well as with bases. The hydrogen ion concentration at which gelatin is neither in combination with acid nor with base is $10^{-4.7}N$ ($p_H = 4.7$ in Sörensen's logarithmic symbol). This hydrogen ion concentration is the isoelectric point. When acid is added to a solution of isoelectric gelatin, part of the acid combines with the gelatin to form gelatin-acid salts, and the proportion of isoelectric gelatin transformed into gelatin-acid salt increases with the amount of acid added. There exists always a definite equilibrium between free acid, gelatin-acid salt, and isoelectric gelatin. Since the physical properties of isoelectric gelatin are different from those of the gelatin-acid salt it is necessary to define a gelatin solution not only by the concentration of gelatin present but also by its hydrogen ion concentration. The writer was able to show that gelatin forms monogelatin salts with many dibasic or tribasic acids, e.g., oxalic, tartaric, and phosphoric acids, while it forms digelatin salts with sulfuric acid.[3]

When we add a base, e.g., NaOH or $Ca(OH)_2$ to isoelectric gelatin, metal gelatinates are formed, and the relative amount of non-ionogenic gelatin transformed into metal gelatinate also depends upon the hydrogen ion concentration.[4]

II. We can measure the osmotic pressure of gelatin solutions by putting them into collodion bags (cast in the form of Erlenmeyer flasks) closed with a rubber stopper perforated by a glass tube which serves as a manometer. A 1% solution of gelatin to which a certain amount of acid or alkali had been added was put into the collodion bag which was dipped into a beaker containing water of the same hydrogen ion concentration as that inside the collodion bag. One per cent solutions of gelatin-acid salts had the highest osmotic pressure when the p_H was about 3.4 and solutions of metal gelatinates had their highest osmotic pressure at a p_H of about 8.4. At about 24° the permanent osmotic pressure was reached in less than 20 hours.

The influence of ions on the osmotic pressure of gelatin solutions was as follows:

(1) The osmotic pressure of 1% solutions of metal gelatinates of a p_H of about 8.4 is a little over twice (but less than three times) as great when the metal is monovalent than when it is bivalent.

(2) The osmotic pressure of 1% gelatin-acid solutions of p_H 3.4 is a little over twice (but less than three times) as great when the acid anion is monovalent than in the case of SO_4.

(3) The addition of neutral salts or alkalies to 1% metal gelatinate solutions of p_H 8.4 lowers the osmotic pressure of the solution and the depressing effect increases with the valency of the cation of the electrolyte added.

(4) The addition of neutral salts or acids to 1% gelatin-acid solutions of p_H 3.4 lowers the osmotic pressure of the solution and the depressing effect increases with the valency of the anion.[5]

This influence of electrolytes on the osmotic pressure of gelatin solutions has no connection with the fact that gelatin is a colloid.[6] This latter fact plays only a technical rôle in our experiments since it makes it easy to find a strictly semipermeable membrane. The writer has been able to show that the influence of ions on the osmotic pressure of gelatin solutions is connected with the existence of an electrical double layer at the boundary of watery phase and collodion membrane, whereby the water diffuses into the solution in the form of electrified particles.

III. When we separate a solution of a non-electrolyte like cane sugar from distilled water by a collodion membrane, water diffuses into the solution with an initial velocity which is (up to gram molecular concentrations) a linear function of the concentration of the solute. When we separate a solution of an electrolyte from distilled water by a collodion membrane, water diffuses also into the solute but with an initial velocity which for lower concentrations of the solution is no longer a linear but a more complicated function of the concentration and the nature of the ions. The facts observed can be expressed in the following rules:

(*a*) When pure water is separated from a solution of an electrolyte by a collodion membrane, water diffuses into the solution as if its particles were positively charged and as if they were attracted by the anions and repelled by the cations of the solution with a force increasing with the valency and another constitutional quantity of the ion

(*b*) When the collodion membrane has previously been dipped for a short time into a solution of a protein and if the protein solution is then removed Rule *a* holds also except that in the presence of hydrogen ions and of simple cations with a valency of 3 or above, in sufficient concentration, the particles of water diffuse into the solution as if they were negatively charged and as if they were attracted by the cation and repelled by the anion of the electrolyte with a force increasing with the valency and another property of the ion.

(*c*) The relative influence of the oppositely charged ions of an electrolyte is not the same for different concentrations of an electrolyte. At lower concentrations the influence of that ion prevails which has the opposite sign of charge as the watery phase, while in higher concentrations (of many, but possibly not of all electrolytes), the effect of the other ion prevails. The turning point lies for a number of electrolytes at a molecular concentration of about $M/256$. Finally a concentration is reached where this ionic influence on diffusion ceases and the gas pressure effect prevails.[7]

These three rules apply also to solutions of gelatin salts. In the case of gelatin-acid salts water is negatively charged and is attracted by the gelatin ion (which is positively charged) and repelled by the acid anion. This repulsion is greater when the anion is bivalent (SO_4) than when it is monovalent, e.g., Cl. Hence the initial rate of diffusion of water into the gelatin-acid solution is greater in the case of gelatin chloride or gelatin nitrate, etc., than in the case of gelatin sulfate.

In the case of metal gelatinate the particles of water diffusing through the membrane are positively charged and attracted by the gelatin anion and repelled by the cation with which the gelatin is combined. This repelling effect is greater in the case of bivalent cations (Mg, Ca, Ba, etc.), than in the case of monovalent cations (e.g., Li, Na, K, NH_4). Hence the initial rate of diffusion of water into solutions of metal gelatinates is greater in the case of sodium gelatinate than in the case of calcium gelatinate. When we add an electrolyte to the solution of a gelatin salt the increase in concentration of electrolyte brings about the depressing effect of that ion which has the same sign of charge as the electrified watery phase, as expressed in Rule *c*.

The permanent osmotic pressure of a solution separated from pure water by a semipermeable membrane is that pressure which has to be applied to the solution side in order to cause equal numbers of particles of water to pass simultaneously through the membrane in opposite directions. It follows from this that the relative influence of electrolytes must be the same on the permanent osmotic pressure as on the initial velocity of diffusion through the membrane from pure water to solution, and this was found to be the case with the solutions of gelatin salts.

IV. The terms "attractive and repulsive effect" of the ion on the charged particles of water served the purpose of simplifying the presentation of the facts observed. In reality the "attractive and repulsive effect" of ions as expressed in Rules *a*, *b*, and *c* are the expression of an additive effect of the oppositely charged ions of an electrolyte on the density of charge of the double layer formed at the boundary of collodion membrane and watery phase. This was proved by experiments on electrical endosmose. When the collodion membrane is bounded on both sides by identical solutions, an equal number of particles of water will diffuse in opposite directions through the collodion membrane. When, however, an external potential difference is produced on the two sides of the membrane, an electrical endosmose will be established to one of the two electrodes according to the sign of the electrification of the watery phase in contact with the collodion membrane. By this method I ascertained the influence of electrolytes on the sign of charge on the watery phase which was expressed in Rules *a*, *b*, and *c*.

Cases of abnormal of osmosis, e.g., the fact that liquid diffuses through parchment membranes from oxalic acid into pure water had been known for more than 50 years and it had been suggested by several authors, Girard, Bernstein, Bartell and Hocker, and Freundlich,[8] that potential differences on both sides of the membrane caused these anomalies. To prove this view it was necessary to measure these potentials. Direct measurements of such potentials have been made by Girard, by Bartell and Hocker,[8] by T. Hamburger,[9] and by the present writer (as yet unpublished), but the results have thus far not been satisfactory. One can, however, test the idea indirectly by experiments on electrical endosmose. This method of verification seemed to be confronted with a serious difficulty. The influence of ions on electrical endosmose has been investigated by Perrin[10] who found that only one of the oppositely charged ions of an electrolyte, namely, the one which has the same sign of charge as the watery phase, has any influence (namely, a retarding one) on the velocity of endosmose, while the ion with the opposite charge has no influence. In my experiments on common osmose both ions of an electrolyte influenced the rate of diffusion of water simultaneously but in an opposite sense (as stated in Rules *a*, *b*, and *c*). I have investigated the influence of ions on the rate of electrical endosmose and found that Perrin's rule does not hold for collodion membranes, but that the Rules *a*, *b*, and *c* express not only the influence of ions on the transport of water through collodion membranes in common osmosis but also in the case of electrical endosmose.

According to the formula of Helmholtz as modified by Perrin, we know that in the case of electrical endosmose the following relation holds:

$$v = \frac{9.\epsilon.E.D}{4\pi.\eta.l}$$

where v is the quantity of liquid carried electro-osmotically, ϵ is the potential difference between the two strata of the double layer, E the external electromotive force, D the dielectric constant of the medium, η the coefficient of internal friction, and l the distance of the external electrodes.

In our experiments all quantities occurring in the formula except v and ϵ were kept approximately constant. If the theory of Helmholtz is correct, we must, therefore, attribute the influence of ions on the rate of transport of water in electrical endosmose to their influence on ϵ, i.e., the density of charge on the watery phase; and since, moreover, my experiments show that the influence of ions is the same for electrical and for free endosmose through collodion membranes we must conclude that the influence of ions on the initial rate of diffusion and on the osmotic pressure of a solution is due to the influence of ions on the density of charge in the double layer at the boundary of watery phase and membrane.

V. This permits us to define more accurately the influence of ions on osmotic pressure. At the boundary of a membrane and liquid an electrical double layer is formed. In the case of collodion membranes the latter usually assumes a negative charge while the watery phase assumes a positive charge. When the collodion membrane is bounded on one side by pure water on the other by a solution of an electrolyte the anions of the latter increase the negative charge on the membrane and the cations diminish it, both effects increasing with the valency and another property of the ions. At lower concentrations of the electrolyte the influence of the anion increases more rapidly with increasing concentration of the electrolyte than the depressing effect of the cation, while at higher concentrations the reverse occurs. The turning point lies for a number of electrolytes at a molecular concentration of about $M/256$. Positively charged particles of water in the pores or interstices of the membrane will be driven to that side of the membrane which is more negatively charged. Since this is usually the solution side, water will be driven from the side of pure water into the solution. When, however, the membrane has a higher negative charge on the side of pure water, the flow of the positively charged liquid through the membrane will be from the side of the solution to that of pure water (negative osmosis). This occurs in the case of solutions of $(CaOH)_2$ and $Ba(OH)_2$ when the concentration exceeds $M/256$.

When the collodion membrane has been treated with a protein, it is also generally negatively charged except when the solution contains hydrogen ions or simple trivalent or tetravalent cations beyond a certain concentration (which for H is 10^{-4} N); in this case the membrane is positively and the watery phase is negatively charged. If we add in this case an electrolyte to the water the charge on the membrane is increased by the cations and diminished by the anion of the electrolyte, both influences increasing with the valency of the ion, and another property which is still to be defined. In lower concentrations the influence of the cation increases more rapidly than that of the anion, in higher concentrations of the electrolyte the reverse is true. Whenever the positive charge on the solution side of the membrane is greater than on the opposite side, the negatively charged particles of water will diffuse from the side of pure water to the side of solution. When, however, the positive charge on the solution side of the membrane is less than that of the side of pure water, the negatively charged layer of liquid will flow through the membrane from the side of the solution to that of pure water (negative osmosis). This will happen especially in the case of dibasic or tribasic acids when the concentration exceeds a critical limit. (Since the collodion membrane is permeable for electrolytes we are dealing in reality not with pure water separated by the membrane from solution, but with the condition of weak solution separated from a more concentrated solution.)

It is obvious that with increasing concentrations of the solution the opposite effects of the oppositely charged ions of an electrolyte on the density of charge of the double layer tend to become equal and then the gas pressure effect becomes the main or only driving force for the diffusion of water into solution. For lower concentrations of electrolytes the algebraic sum of both forces, the differences of potential on both sides of the membrane and the gas pressure effect, determine the rate of diffusion of water through a membrane into a solution.

The new experiments on which these conclusions are based will appear in the *Journal of General Physiology*.

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[4] Loeb, J., *Ibid.,* **1,** 1918–19 (483).

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[6] The influence of electrolytes on the osmotic pressure of gelatin is not due to differences in the degree of electrolytic dissociation of the gelatin salts, since, e.g. Na gelatinate and Ca gelatinate of the same concentration of gelatin and hydrogen ions have practically the same conductivity.[4]

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EVIDENCE ON THE NATURE OF NUCLEAR ACTIVITY

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Binuclearity hypotheses, founded in part on Richard Hertwig's chromidial hypothesis, have influenced interpretations of extra-nuclear bodies in the cytoplasm profoundly. The obvious dual capacity of the metazoan nucleus, exhibited in kinesis and interkinesis, i.e., in generative (propagatory), and somatic phases, has been made to lend itself to analogy with a true binucleate condition and to an assumption that the nucleus contains two kinds of chromatin. Of these one is supposed to be propagatory (idiochromatin), in evidence at the time of cell division, the other trophic (trophochromatin, somatochromatin), formed by the idiochromatin, but resident in the cytoplasm. This phase of the binuclearity idea is quite independent of that to which support is given by the demonstration of dimorphism in chromosomal groups during oogenesis and spermatogenesis.

The somatic phase of the nucleus covers the period during which it may be assumed that the nuclear enzymes have passed from the nucleus to the cytoplasm, and the cytoplasm has become the seat of synthetic activities. The nucleus at this time is in a "resting" condition; it seems comparatively empty, is acidophile, and basophilic granules may be found in the cytoplasm.

What is the nature of these basophilic bodies? Are they of direct or indirect nuclear origin?

The accounts of the extrusion of chromatin from the nucleus are numerous. Under the influence of the binuclearity hypothesis supposed particles of chromatin in the cytoplasm have been interpreted as chromidia, or as trophochromidia. Schaxel (1911) in a series of able papers, has presented evidence in support of the idea of emitted chromatin; Dantchakoff's (1916) observations support the idea. On the other hand Beckwith (1914), by no means denying, and Gatenby (1919), strongly supporting the idea of emissions from the nucleus, find no evidence of the emission of *formed* materials, in the sense of filtration of chromatin through the nuclear membrane.

There have been no observations which have enabled us to link, in any satisfactory way, our theories concerning the synthetic activities of the nucleus with the observations of Chambers (1917) on reversible changes in the cytoplasm, and with other knowledge of colloidal solutions. The material on which this paper is based makes an attempt at such a linkage possible.

In 1908 (*Biol. Bull.*, **15,** p. 132), in discussing sections of *Arbacia* eggs fertilized by *Moira* sperm, an inter-ordinal cross, I wrote, "In eggs in which the daughter nuclei are in the resting condition succeeding the first division, the cytoplasm contains many deeply staining rods. The nucleus at this time does not take the chromatin stain and appears like an empty vesicular structure."

"In eggs of the same lot and on the same slides, in which the fibers of the second amphiaster have begun to form, the nucleus again takes the stain and shows the chromatic net, while the cytoplasm is seen to be free from the bodies described."

"These structures have puzzled me not a little, but I have finally reached the conclusion that the eggs in which they occur are degenerating. Even though this be true it is difficult to explain the simulation or perhaps occurrence of longitudinal and transverse divisions of these chromosomes lying free in the cytoplasm."

The facts were so unusual that in 1911 I prepared and later studied a more complete set of material. One of my graduate students, Miss Pauline Shorey, also sectioned and studied duplicate stages, but it is not until recently that I have been willing to venture an explanation.

The material comprises ten stages, taken at short intervals through a period extending from 25 minutes after insemination to 100 minutes after insemination, or through the anaphases of the third division. Unfortunately, from the point of a study of cytoplasmic inclusions in general, but especially of mitochondria, but one fixing fluid, acetic sublimate (5% acetic), was used. Nevertheless, as the straight fertilized Arbacia eggs used for comparison were fixed in the same way, the opportunity for a sound and logical comparison exists. The sections on which the greater part of the study was made were stained in Heidenhain's iron-haematoxylin, but for the determination of special conditions Auerbach's acid fuchsin-methyl green, basic fuchsin, basic fuchsin-methylen blue, saffranin-gentian violet, thionin, and some of the carmine stains were used.

In the sections of the Arbacia-Moira material a cycle of changes may be noted, changes concerned with the appearance and disappearance of basophilic bodies in the cytoplasm. The development of these bodies may be followed as they appear in a cloud outside of the nucleus, then in lines radiating from the nucleus, then scattered irregularly in the cytoplasm, the nucleus during this time being in the resting condition.

The basophilic bodies appear first in the form of fine granules in the immediate vicinity of the nucleus, then as short rods, which may be seen either singly or in short chains throughout the cytoplasm. These rods are of about the thickness and length of the chromosomes of Arbacia.

A study of the nucleus in the sections of the eggs of this period reveals the fact that the nucleoli have become much paler. In the earlier stages they stain uniformly; in the later the surface (rim in the sections), only stains, while the center remains clear. Deeply stained granules may be seen aggregated at the inner surface of the nuclear membrane as though adsorption were taking place. In some sections a massing of granules may be seen at one side of the nucleus, this mass probably being the expanded chromatin of the male nucleus. There is a distinct impression of diffusion from the nucleus, but there is no evidence of the emission of chromatin as such. At the end of this stage the nucleus is distinctly acidophile.

Then a reversal of the process begins. The sections give the impression of a movement of some of the contents of the cell toward the nucleus; again there is the appearance of a diffuse cloud in the region of the nucleus; the number of rods becomes distinctly smaller, until in the late prophases the cytoplasm has cleared almost completely. The nucleus becomes increasingly basophilic as it passes into the prophases of division. The nucleus increases in size. In the metaphase and in the anaphases of division the cytoplasm is free from rods. During this stage there is no visible evidence of the passage of materials through the nuclear membrane and into the nucleus, but the nuclear content has distinctly increased.

If we correlate these facts with the conditions shown by Chambers to exist in the egg at this time, we find that the diffuse cloud appears while the nucleus is lying in the liquid hyaloplasm sphere. This is at the beginning of the "pause" Wilson (1895), succeeding the fusion of the nuclei in fertilization. The sudden appearance of granules and then of rods indicates a rapid diffusion of enzyme throughout the cell. The rods are very numerous at this time and lie without definite orientation in the cytoplasm. M. R. Lewis determined for Chambers that diffusion of cresyl-blue takes place quickly—in a few seconds—after injection within a cell. The formation of the granules and rods may be interpreted as the result of a reaction of the cytoplasm to the nuclear enzymes.

Following this stage the hyaloplasm sphere divides, as shown by Chambers, and each portion moves and lies like a cap on the nucleus at opposite poles, and the formation of the asters, accompanied by the transition to the gel phase in the cytoplasm, begins. In my material there is evidence of a centripetal flowing, as indicated in the orientation of the rods, as this reversal occurs.

What are these rods? They are not mitochondria. The acetic acid in the fixing fluid has dissolved these. They are not chromosomes. The mass and number of the rods greatly exceed that of the chromosomes. I do not believe them to be chromidia, fragments of chromatin lying freely in the cytoplasm.

I believe that they are a coarse precipitate, possibly a synthetic product, formed by the action of extruded nuclear enzymes into the cytoplasm, and that the processes that I have described are similar to those that occur normally, but which cannot be seen under ordinary conditions, for the reason that the particles ordinarily formed are below the limits of microscopic vision. Nor does there seem to be a probability that they could be distinguished from other moving colloidal particles if the living egg were to be studied with the ultra microscope. It should be emphasized that the granules and rods which are visible in this experimental material and which I have interpreted as a coarse precipitate, are not formed in the straight fertilized eggs.

Turning now to the explanation of the formation of these particles. If we were dealing with solutions of electrolytes the explanation would lie probably in the application of von Weimarn's law on the relation between concentration of the reaction mixtures to the size of the particles of the precipitate. If we compare von Weimarn's microphotographs of the precipitates of barium sulphate formed by pouring together solutions of barium cyanid and magnesium sulphate, with the sections here under consideration, a striking similarity to those obtained at concentrations of 1/500 normal to 1/50 normal will be evident. (Reproductions of some of these microphotographs may be found in Ostwald and Fischer, *Theoretical and Applied Colloid Chemistry*, pp. 27–32.)

But we are dealing with an emulsion colloid, possibly a mixture of suspensoids with emulsoids. The phenomenon that I have described, the formation of granules and rods, is clearly due to a coalescence of particles into coarsely dispersed aggregates.

It seems probable that the suspensoid particles surrounded, "protected," by emulsoid particles, coalesce, as a result of dehydration due to an enzyme emitted from the nucleus. Inasmuch as the change is reversible and these particles pass again into solution the electrical nature of the process is also probable. This naturally would be the case if we were dealing with a suspensoid fraction.

Passing from the changes shown prior to and during the first division, it should be noted that in this material there is no further formation of rods. The coarsely granular precipitate is well marked in the second and third divisions, but no rods are formed.

It is evident that my conclusion from a study of the material described is that the basophilic bodies found are not in the nature of chromidia, but are the result of indirect nuclear activity. As to the applicability of these results to cases in which basophilic inclusions occur normally, it is impossible to say more than that such cases should be considered in the light of the evidence here given. The explanation offered for the formation of the basophilic extra nuclear bodies described is intended to be suggestive rather than conclusive. It brings together facts which have not hitherto been associated.

A more detailed paper with illustratons is forthcoming.

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A STUDY OF THE PERSISTENCE OF VISION

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Introduction.—It was observed by Allen,[1] while investigating the effect of the color of the light on the persistence of vision, that there seemed to be portions of the retina where the persistence of the retinal impression was less than on the fovea. That is, when no flickering of the color under observation was perceptible in the center of the retina, a slight movement of the eye in any direction which allowed the light to fall upon the peripheral portions of the retina was sufficient to destroy the apparent continuity of the light. Allen attempted to measure the persistence for regions on the temperal side of the retina at 10 and 20 degrees from the axis of the eye but found that the results were "too uncertain to be of any use." The writer has measured the persistence of vision for several colors within the cone whose semi-vertical angle is nearly 40 degrees. More than one hundred points on the retina within this area were observed for each color used. From these data, it is possible to construct a map of the retina showing the persistence of vision for each portion.

Results of this sort should be of interest, not only to the illumination engineer, but to the physiologist and the psychologist as well. If the number of observers were large to insure that the results represent the average eye, it would be possible to construct a map of the retina with "contour lines" to show equal values of the persistence of vision. This was done by the author using the values obtained for his own eyes. The general shape of the lines was found to coincide more or less with the shape of the color fields given by Abney.[2] The extent of the color fields is, of course, dependent upon the intensity of the light. It was not possible to show that the area of the retina covered in this investigation was greater than the color field for the blue for the intensity used. As the color field for the blue is larger in area than for any other color, it seems natural to suppose that the persistence of vision should depend only upon the intensity of the light on portions of the retina outside this area and should be independent of the wave-length.

Description of apparatus.—The persistence of vision was measured by observing the minimum speed at which a sectored disk could be driven without destroying the apparent continuity of the light. The source of light was a concentrated filament incandescent lamp operated at constant voltage. A lens system was used to bring the rays to focus on the sectored disk. When the position of the disk is such that the rays do not strike it, they diverge until they strike a ground-glass screen about 5 centimeters square. An iris diaphragm placed just in front of it makes the size of the illuminated area on the ground-glass adjustable without altering the brightness. The sectored disk, the necessary electric motor to drive it, the incandescent lamp and the lens system are all placed in a light tight box. The eye was then placed 1 meter in front of the ground-glass and a chin rest was provided to insure steady conditions of the retina while making the observations. Needless to say, the investigation was carried on in total darkness. A small electric lamp operated on the storage battery current and carefully shielded was used to read the instruments when necessary. The time for the recovery of the retina after this stimulus was less than the time required to place the apparatus in adjustment for the next reading.

The speed of the disk was measured by means of a small magneto and a voltmeter calibrated to read the speed directly in revolutions per minute. The persistence of vision was first determined for the fovea by causing the disk to rotate at sufficient speed so that no flicker was apparent and then slowly to lose velocity until the first flicker was observed. On the average, it was found possible to determine the critical speed so that subsequent readings would not differ by more than 2 per cent. Observations were also made with the speed of the disk increasing and the average was taken as the persistence measure. A set of filters made by the Wratten and Wainwright Company was used one at a time when it was desired to use light of a particular color. These filters were found to be very nearly monochromatic. The use of spectrum colors would be more accurate but the intensity of the light cannot be adjusted within as wide limits.

To determine the persistence of vision for off center portions of the retina, a small radiolight sight was used. This was mounted on a slider attached to a long rod and so constructed as to revolve about the center of the diaphragm. In this way it was possible to place the sight in any desired position with respect to the center of the diaphragm. Shallow grooves were placed at intervals along the rod so that it was possible to read the position of the slider in the dark. In the experimental work, readings were taken about every 3 degrees from the center and along directions which made angles with the horizontal of 45, 90, 135, 180, 225, 270 and 315 degrees. The manipulation was the same as before except that the attention was directed toward the radiolight sight and the persistence of vision measured with the light from the ground-glass screen falling on some other portion of the retina. It was, of course, necessary to cover one eye during all of the experimental work.

Experimental results.—Before results could be obtained which were consistent with themselves, it was found necessary to take several precautions. For example, time was given for the eye to become accustomed to the darkness. Results were obtained which showed that 5 minutes in total darkness was sufficient. It was also found that any motion of the body, however slight, would cause the interest to flag. For this reason, the motor controls had to be adjusted so that the motor would change its speed slowly as it was impossible to operate a rheostat by hand. One hand was held on a key which was pressed at the instant that the flicker was seen to appear or disappear and the critical speed noted.

The size of the diaphragm which seemed to give the best results was a circle of diameter 5.84 mm. The persistence of vision is dependent upon the size of the retinal area stimulated and also the scintillation of the light from a small aperture caused more or less uncertainty.[3] The above aperture was chosen as being the smallest that it was practicable to use. With the diaphragm placed at a distance of 1 meter from the eye, the angle subtended by the diaphragm at the eye is 3.36°.

As has already been said, the persistence of vision was determined for several colors and in each case the persistence was measured for about one hundred points on the retina lying inside a circle which is the base of a cone whose semivertical angle is 38.7°. No attempt will be made to give the results in full. They represent the persistence of an impression on the retina of the eye of the author. The eye is known to be normal for color perception but has a moderate amount of astigmatism which should not affect the persistence of vision. A few results will be given to show the nature of the inferences which may be drawn from the investigation.

For red light (6776 A°) the persistence of vision in the fovea was 0.0209 second. The persistence for points lying at equal distances from the fovea was found to be very nearly the same. That is, if lines are drawn showing equal values of the persistence of vision, they appear to approximate circles with the fovea at the center. The deviation from the circle is enough to make them resemble the limits of the color fields for the retina. The circles are in every case flattened so that the major axis of the resulting ellipse is horizontal. The persistence is less for the fovea than for any other part of the retina, and there is a steady increase in the persistence nearly proportional to the distance from the fovea. The maximum value observed occurs on the nasal side of the retina at about 38° from the fovea. The persistence is slightly greater on the nasal side than on the temporal. The maximum value is 0.109 second.

For the yellow-green (5310 A°) very similar results were obtained. The persistence of vision for the fovea is 0.0179 second and is less than any other portion of the retina. The lines of equal values of the persistence are ellipses with the major axes horizontal. The persistence is still slightly greater on the nasal side. The maximum value is observed to occur for the same region as for the red light but the maximum in this case is 0.0339 second showing that the persistence is more nearly constant over the whole retina.

For the blue-violet (4631 A°) the persistence of the fovea is 0.0346 second. There is little change in the persistence for different portions of the retina. The region which gave a maximum value for the red and the yellow-green, now gives a value of 0.0339 second or slightly less than the fovea. The maximum occurs about 7° from the fovea on the nasal side and is 0.0401 second. The minimum of 0.0305 second occurs on the temporal side at an angle of 35° from the fovea. The change between the maximum and minimum amounts only to the difference between $^1/_{25}$ second and $^1/_{35}$ second. For the blue-violet light used, the persistence is very nearly constant over the whole retina.

It will be noticed that these values for the persistence are smaller than those which are sometimes quoted. The values given here represent the time required for the impression on the retina to fade sufficiently to be noticed when compared to a fresh stimulus. They do not represent the time for the total extinction of the retinal image.

The above results were obtained at the laboratories of the Department of Physics at the University of California.

[1] *Physic. Rev.*, **28,** 1909 (48).

[2] Sir William Abney, *Researches in Color Vision*, p. 190, et. seq.

[3] See Abney, *loc. cit.*, p. 181.

A CONTRIBUTION OF GENETICS TO THE PRACTICAL BREEDING OF DAIRY CATTLE

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When the writer began his work at the Maine Agricultural Experiment Station some 13 years ago the only genetic concept which played any part in or influenced the actual operations of the animal breeder was that of phaenotypic selection. The slogan and the practice, so far as there was any intelligent practice, was to "breed from the best to get the best." This was perhaps theoretically a sound enough doctrine, but there was lacking one essential to its practical operation with the anticipated degree of success. This lack, as the rapidly advancing knowledge in the field of genetics soon clearly showed, was of any precise criterion as to how the best *genotypes* were to be selected, when the breeder operated on the basis only of a knowledge of *somatic* performance or characters alone. As a matter of fact it was easy to show, as the writer did show, in the case of egg production in fowls,[1] and milk production in cattle,[2] that the phaenotypic performance was an exceedingly unreliable guide to real breeding worth.

The real value of the measure of an animal as a breeder—as a factor in the improvement of a herd or flock—is the kind of progeny which it produces. Confining the discussion to performance in respect of milk or butter-fat production, a cow may have herself a perfectly enormous record of performance but she is worthless as a *breeder* if her progeny are uniformly poor producers, which can in fact be easily shown to be often the case. Again a bull may come from ancestry of the highest performing ability in respect of milk and fat production, and consequently he may as a calf command a price in five figures, but such ancestry has repeatedly been shown to be no guarantee in fact that his progeny will be great performers.

The paramount importance of having a bull of such genotypic constitution as to insure relatively high dairy production in his offspring, if one is to attain success in the breeding of dairy cattle, is obvious. Nearly 10 years ago the writer began the intensive study of the practical problem of scientifically measuring the breeding worth or value of bulls of the dairy breeds, to the end that the farmer and breeder might operate more intelligently and successfully. It appeared evident that the most important thing which a breeder of dairy cattle desires to know is whether the animals which he breeds are transmitting productive qualities to their offspring. This information is particularly desired for the herd bull since his offspring are much more numerous than those of any single cow and he constitutes one-half the heritage given to them. It would appear beyond question that if a bull's daughters are on the average poorer milkers, or poorer in the quality of their milk, than the dams from which they came, then the bull is exercising a harmful effect upon the breed. On the other hand, if a bull's daughters are on the average measurably better than the dams from which they came, in productive qualities, then that bull is exercising a beneficial effect on the breed. Stated specifically, if D_d represents the milk production of a daughter of a given bull, and D_m the milk production of the dam of this daughter, then the measure of this bull's transmitting qualities for milk production in respect of this pair would be:

$$\textit{Sire's transmitting power} = D_d - D_m.$$

If the daughter's production is more than the dam's the sign will be plus. Insofar as one daughter-dam pair is indicative the bull is increasing the production of his daughters and consequently improving the breed. If the daughter's production is less than her dam's the sign will be minus. The bull is consequently insofar detrimental to the breed. By the summation of these plus and minus differences with regard to sign, the amount and sign of this summed quantity gives the measure of what he did for all his daughters, and dividing by the number of daughter-dam pairs gives a measure of the mean or average effect of this sire on the breed.

Being convinced that this reasoning was sound, and that if carried out thoroughly the consequences would be bound in the long run to be of incalculable, but great, practical economic value to mankind, the writer inaugurated in his laboratory at the Maine Experiment Station the project of determining the transmitting power as above defined, of every bull in the Jersey breed recorded up to 1916 as having two or more daughters whose milk-producing qualities were known, as well as the producing abilities of the dam. This project involved a very large amount of labor and was interrupted by the war, but has now been finished and will shortly be published.[3] It is the purpose of the present paper to give a brief account of the scope of the work and some of its chief results.

A prerequisite to the comparison of daughter and dam production was an accurate method of correction for change of milk and fat production with age. Considerable study has been given this problem. I showed in 1914[4] that the equation relating milk flow to age in dairy cattle was of the form

$$y = a + bx + cx^2 + d \log x.$$

Since that time these curves have been worked out in my laboratory for all the principle dairy breeds, chiefly by Miner, Gowen, Patterson, and myself. The curve for annual milk production in the Jersey breed is

$$y = 4586.50 + 307.55x - 12.65x^2 + 2216.62 \log x$$

where y denotes milk production in one year in pounds, and x denotes age in years. How closely this equation agrees with the observed facts is shown in figure 1.

It is obviously impossible to present here anything like the total results of this extensive piece of work. As a sample of the sort of results obtained table 1 is shown. This gives the net average change in butter fat production of daughters as compared with dams. The first column of table 1 gives the place occupied by the bull in the series of bulls. The second column gives the name of the bull. The third column states how many pairs of daughter-dam tests the bull under discussion had. The fourth column gives the number of pounds of butter-fat that the bull's daughters are in excess or defect of their dams. It is by this column that the bulls are arranged, the bull whose daughters produced the most butter-fat over the butter-fat production of their dams coming at the top of the list. The plus sign shows the daughters produced more butter-fat than their dams, the minus sign that they produced less butter-fat.

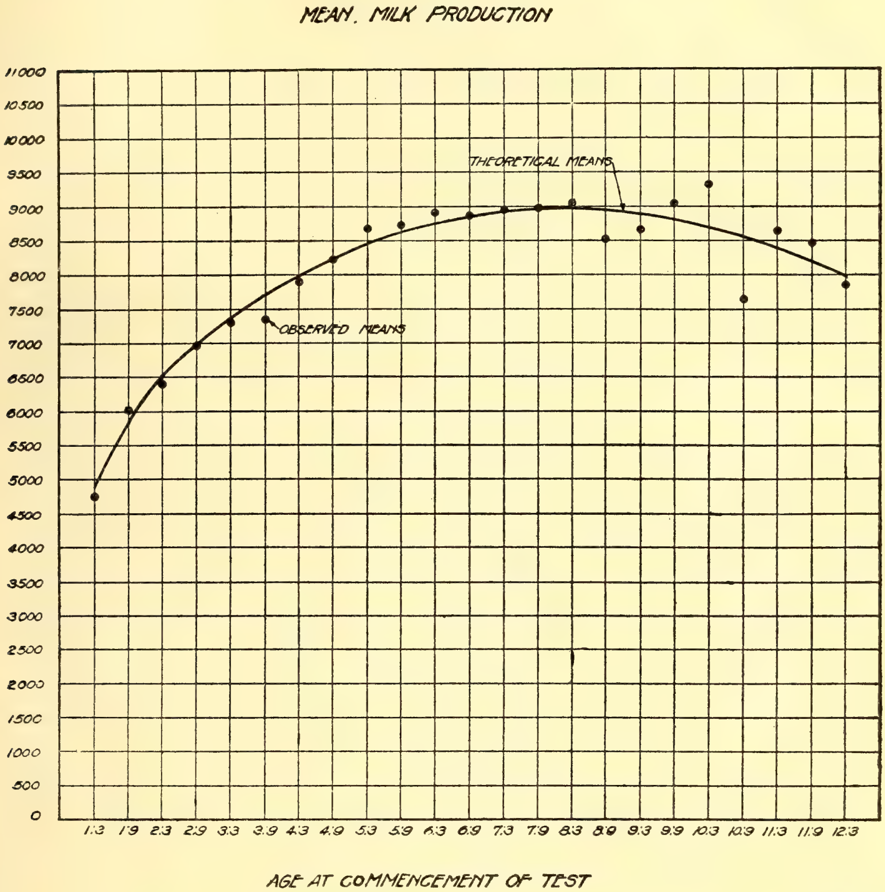

FIG. 1. Observational and theoretical curves of mean milk production (365 day) at various ages in Jersey cows.

TABLE I

BULLS IN ORDER OF CHANGE IN NET FAT OF DAUGHTERS

NO.	NAME OF BULLS	NO. OF PAIRS	NET CHANGE LBS.
1	Sans Aloi	2	+210.56
2	Signal's Successor	3	+177.72
3	Golden Glow's Chief	4	+161.93
4	Hood Farm Pogis 34th	2	+159.24
5	Tormentor's Landseer Signal	4	+153.55
6	Clear Brook Chief	2	+153.31
7	Channel King	2	+150.30
8	Golden Nero	2	+149.77
9	Majestic Fern	3	+143.97
10	Roma's Rioter of St. L.	3	+143.79
11	You'll Do Oxford	2	+136.61
12	Hood Farm Figgis Torono	3	+132.50
13	Themisia's Interested Prince	2	+131.12
14	Rinda Lad of S. B.	2	+126.38
15	The Plymouth Lad	4	+123.55
16	Hood Farm Torono	34	+121.51
17	Agatha's Brookhill Fox	2	+115.40
18	Fontaine's King	2	+111.60
19	Golden Fern's Son	9	+109.32
20	Baronetti's Golden Lad	2	+100.14
21	Spermfield Owl	26	+97.71
22	Sultan of Oaklands	3	+95.06
23	Melia Ann's King 9th	2	+93.48
24	Chenille's Golden Prince	2	+93.06
25	Eurybia's Blue Boy	3	+90.17
26	Forfarshire's King Dalton	7	+88.00
27	Rioter's Jersey Lad	7	+87.10
28	Queen's Raleigh	3	+82.68
29	Pogis 99th of Hood Farm	14	+82.34
30	Royal Majesty	5	+81.34
31	Jenny's Redfern	2	+79.56
32	Rearguard	3	+79.24
33	Oxford's Fairy Boy	2	+77.14
34	Gertie's Stoke Pogis	3	+70.59
35	Mabel's Raleigh	3	+70.51
36	Pogis 66th of Hood Farm	3	+66.86
37	My Jubilee	2	+66.75
38	Oxford Lass's Cream King	3	+66.23
39	Great Edison	2	+62.78
40	The Warden's Sir Prince	2	+60.87
41	Nova Boy of Turner	2	+58.82
42	Nebraska Lad	2	+57.14
43	Hood Farm Golden Lad	5	+56.50
44	Spermfield Owl 2d	2	+51.85
45	Marston's Interested Prince	9	+51.82
46	Eminent's Tormentor	2	+51.81
47	Chief Engineer	6	+50.20
48	Hood Farm Pogis 9th	39	+47.49
49	The Owl's Double Grandson	5	+47.45
50	Atwell Farm Melia Ann's King	2	+45.84
51	Dorinda Carling's Diploma	6	+41.77
52	Golden Shylock	5	+41.00
53	Loretta D's Champion Son	5	+38.66
54	Taurus Perfection	2	+38.56
55	Brown Bessie's Columbus	3	+38.18
56	Blue Belle's Gold Fern	3	+37.08
57	Golden Lad of Glenwood Farm	3	+35.08
58	Tormentor Summit Lad	4	+34.39
59	Melia Ann's Hero 4th	3	+33.50
60	Keepsake's Golden Lad	5	+32.18
61	Bessie Bate's Lad	2	+30.80
62	Nutley Tones Owl	3	+30.55
63	Flying Fox's Oxford Duke 2d	2	+29.30
64	Lady Letty 4th's Rioter	2	+28.33
65	Golden Lad of Summit	6	+27.79
66	Golden Alcazar	3	+27.32
67	Foxhall's Jubilee	10	+26.64
68	Eminent's Pilot	3	+24.18
69	The Owl of Meridale	5	+24.17
70	G. G. Chief of Ashburn	3	+23.75
71	Imp. Rozel's Noble	2	+22.76

TABLE 1—(*Continued*)

NO.	NAME OF BULLS	NO. OF PAIRS	NET CHANGE LBS.
72	Eminent 4th	5	+21.54
73	Interested Prince	21	+21.07
74	Pogis 75th of Hood Farm	7	+21.00
75	Sayda's Heir 3d	15	+20.87
76	Eminent of Acca Farm	3	+20.39
77	St. Oner's Torono	2	+20.32
78	Flying Fox's Victor	2	+18.58
79	St. Lambert's Ridgewood King	2	+16.32
80	Loretta's King	18	+15.94
81	Romulus of Spring Hill	4	+13.34
82	Cream Princess' Fritz	2	+12.57
83	Irene's King Pogis	16	+12.31
84	Anderson's King	2	+11.11
85	Storrs Golden Lad	2	+9.80
86	The Warden	12	+8.47
87	Lord Letta of Merridale	2	+7.89
88	Eva's Grey Fox	2	+7.67
89	Oonan's Count	8	+7.55
90	Valentines Oonan	10	+5.19
91	Marigold St. Helier	4	+4.42
92	Noble of Oaklands	3	+4.24
93	Fairfield's Chancellor	2	+3.51
94	Exile of the Highlands	4	+3.23
95	Foxy's Brown Poet	7	+2.54
96	Nimbus of Brondale	3	+1.59
97	Gedney Farm Oxford Lad	2	+1.57
98	Foxy's Fox of Althea	2	+1.52
99	Golden Butter Lad	2	+1.25
100	Pedro's Vidi	2	—0.51
101	Mabel's Raleigh	3	—1.46
102	Merry Maiden's Grandson	6	—3.27
103	Model's Oxford Lad	5	—6.99
104	Lookout Torono	17	—7.94
105	Hood Farm Golden Lad 4th	3	—8.37
106	Hazel Fern Golden Kin	8	—8.74
107	Eurybia's Son	3	—9.94
108	Rosetta's Golden Lad	5	—10.04
109	Pogis of Goliad	3	—12.48
110	White Oak Monarch	3	—12.64
111	Prince Noel	4	—13.04
112	Mr. Inez Marigold Pedro	3	—13.49
113	Tonona Pogis	14	—14.00
114	Fontaine's Caiest	5	—15.01
115	Imp. Noble Fontaine Raleigh	5	—15.04
116	Lady Letty's Victor	15	—15.70
117	Guenon's Lad 2d	2	—15.80
118	Oxford's Brigadier	2	—17.48
119	Cinxia's Gamboge Lad	3	—17.69
120	Portland Prospect	3	—18.98
121	Gamboge's Knight	5	—20.07
122	Oxford Daisy's Flying Fox	6	—20.38
123	Hood Farm Torono 21st	4	—20.85
124	Copper Baron	2	—21.24
125	Karnak's Noble	4	—21.25
126	Jersey Lad's Caspar	5	—21.69
127	Marigold's Exile King	8	—22.37
128	Nero's Garfield Lad	2	—22.72
129	Alcarano	4	—23.69
130	Oonan's Major	2	—25.33
131	Merry Miss' Son	5	—26.17
132	Meridale Interested Prince	6	—26.55
133	Oonan's Conqueror	2	—27.48
134	Fontaine's Lodestar	4	—27.56
135	Seven Gates	5	—29.25
136	Noble's Jolly Golden Lad	2	—29.78
137	Naiad's Golden Lad	3	—31.57
138	Romeo of Sheomet	4	—31.75
139	Willson's Exile	2	—33.00
140	Lad's Oxfordshire	2	—33.39
141	Mabel's Poet	5	—36.33
142	Eminent's Raleigh	2	—37.30
143	Reservation Stoke Pogis	5	—37.98
144	Fairy Glen's Raleigh	5	—38.32
145	Gertrude's Jap	2	—38.47
146	Rosaire's Olga Lad	13	—38.93
147	Pogis 95th of Hood Farm	5	—39.71
148	Prince Ramaposa	2	—39.83

TABLE I—(*Continued*)

NO.	NAME OF BULLS	NO. OF PAIRS	NET CHANGE LBS.
149	Valentine's Count	6	—40.75
150	Sultana's Oxford Lad	3	—40.77
151	Melia Ann's Berkshire	2	—42.39
152	Glory's Pedro	2	—42.85
153	Oxford King of Fernwood	3	—44.37
154	Gertie's Son's Jamont	2	—45.00
155	Pogis Alden Boy	2	—45.04
156	King Allie Rioter	2	—45.11
157	Hood Farm Torono 20th	19	—45.59
158	Olga Hazel Pogis	2	—46.97
159	Marigold's Champion	3	—47.12
160	Sensational Fern	3	—47.58
161	Imp. King of Hambie	2	—48.02
162	Rosaire's Lad of Glenwood	2	—48.79
163	Owl's Model Fox	2	—49.41
164	Rochette's Noble	2	—49.84
165	Biltmore's Torment	4	—50.61
166	Gendey Farm Girl's Oxford	14	—53.27
167	Mistletoe Pogis	2	—53.30
168	Altama's Pogis	3	—53.31
169	Raleigh's Fairy Boy	10	—53.67
170	Quintin Owl	3	—56.07
171	Altama Interst	4	—59.39
172	Golden Lucy's Eminent Lad	7	—60.09
173	Eminent 10th	9	—60.15
174	The Imported Jap	11	—61.91
175	Gamboge's Oxford Lad	2	—64.25
176	Roving Gipsy	2	—64.75
177	Golden Laddie of C.	3	—65.75
178	Lord North	2	—65.98
179	Inez's Stoke Pogis	4	—66.65
180	Golden Paul	3	—68.05
181	Oxford John D.	3	—68.26
182	Ben Hur Pogis	2	—68.79
183	Island Lodestar	6	—70.89
184	Daisy's Prince of St. L.	2	—73.38
185	Pogis 94th of Hood Farm	2	—75.91
186	Amy's Chief	4	—76.37
187	Fontaine's Duke	6	—76.54
188	St. Mawes	2	—78.44
189	Fontaine's Count	2	—80.51
190	Pedro of Brick House Farm	3	—82.31
191	Jacoba's Emanon	9	—84.27
192	Fad	2	—85.61
193	Cowslip's Ashley	3	—87.42
194	Benedictine King	2	—88.14
195	H. F. Golden Fern's Lad	7	—90.49
196	Tormentor's Torono	5	—90.51
197	Girldine's Champion Pogis	3	—90.60
198	Kings Rioter Lawrence	2	—94.96
199	Gazelle's Fawn Rioter King	3	—98.85
200	Noble Peer	4	—98.85
201	Violet's Oakland Count	2	—99.71
202	Minaret Exile	9	—100.59
203	Brown Lassie's Compass	5	—100.61
204	Fort Hill Farm Torono	2	—102.25
205	Viola's Golden Jolly	2	—102.51
206	Oaklands Sultan King	2	—103.95
207	Gertie's Son's Boy	2	—106.18
208	Adelaide's Merry Pogis	3	—108.14
209	Mona Rose's Glory	5	—110.94
210	Financial Raleigh	4	—115.05
211	Jacoba's Premier	3	—115.10
212	Magery Golden's Fox	4	—119.76
213	Fern's Air	2	—123.55
214	Harry B. Gordon	2	—124.46
215	Combination Golden Lad	3	—138.06
216	Hector Marigold	2	—144.12
217	Lady Mary's Fox	2	—148.02
218	Rosaire's Golden Lad	2	—149.95
219	St. Helier of Sheomet	3	—150.59
220	Oonan 23d's Grandson	3	—156.92
221	Jubilee of Bois d'Arc	3	—162.92
222	Oxford Lad's Owl	2	—164.84
223	Hood Farm S. Tormentor	3	—219.72
224	Gertie's Son of Washington	3	—227.01

This table gives to the breeder information of a sort he has never had before. It enables him at once to form a real judgment of the worth of various bulls which appear in the pedigree of Jersey cattle. Many of the bulls in the minus side of the table have at different times been regarded as leaders of the breed and they and their progeny have sold for fabulous prices. With a scientific measure in hand of their actual effect upon the breed it will in the future be exceedingly difficult to persuade any intelligent breeder of the transcendent merits of these minus sires. On the theoretical side there are many interesting points which flow from this and similar tables, but space cannot be taken here for their discussion.

After presenting the basic tables for milk production, fat percentage, and net fat, similar to table 1 here, the complete paper goes on to discuss various related problems, such as the relation of inbreeding, amount of Island bred stock, etc., to production. The chief results of the whole study may be stated as follows:

1. There are 224 Jersey Registry of Merit sires which meet the requirements of this performance test for their transmitting qualities in milk production. One hundred and five of these sires or less than one-half raise the milk production of their daughters over that of the dams of these daughters. The largest number of daughter-dam pairs is 39 for the sire Hood Farm Pogis 9th, 55552. Of those sires which have a large number of pairs, Hood Farm Torono 60326 with 34 pairs stands first in his transmitting qualities raising the milk production of his daughters on the average 2620.1 pounds.

2. Two hundred and twenty-five sires are included in the table giving the sires which met the requirements of the daughter-dam performance test for transmitting qualities of butter-fat percentage. Out of this number 101 sires raised the butter-fat percentage of their daughters' milk as compared with the butter-fat percentage of the dams of these daughters. The leading sire in this butter-fat percentage performance test was Clear Brook Chief 74685 raising his daughters on the average 1.338% of butter-fat. This sire had two daughter-dam pairs. Hood Farm Pogis 9th leads in number of daughter-dam pairs with 42. This bull raised the butter-fat percentage of his daughter on the average of 0.243% over the butter-fat percentage of the dams of their daughters.

3. The sires mentioned as superior in the milk transmitting ability, Hood Farm Torono and Spermfield Owl, do not check up so well in their ability to transmit high butter-fat percentage. Hood Farm Torono caused his daughter on the average to be 0.225 per cent of butter-fat below what the dams of these daughters produced. Spermfield Owl only raised his daughters on the average 0.027% of butter-fat over what the dams of these daughters produced.

4. There are 224 sires of known transmitting ability for net butter-fat. Of this number only 99 sires raise the butter-fat production of their

daughters over that of their dams. The sires which raised the production of their daughters' butter-fat most were Sans Aloi 81012, Signal's Successor 72758, and Golden Glow's Chief 61460. The sires which made the deepest impress on the breed by raising the butter-fat of the largest number of daughters over that of their dams was Hood Farm Torono with 34 pairs and an average increase for each daughter of 121.51 pounds of butter-fat. The next bull, Spermfield Owl, with 26 pairs raised the butter-fat production 97.71 pounds on the average for each of his daughters. Some of the bulls lowering the production of their daughters markedly were Gertie's Son of Washington 83799, Hood Farm S. Tormentor 96311, and Oxford Lad's Owl 75599.

5. The information summarized above was arranged to reveal the transmitting qualities for milk production, butter-fat percentage and butter-fat of Jersey sires to their sons. There were 159 sires which had sons whose progeny performance was known. Of this number 69 or significantly less than half had sons who raised the butter-fat production of their daughters over that of their dams.

6. The sires of superior merit are defined as those which raise the milk production and butter-fat percentage of their daughters as compared with that of their dams. The inferior sires are defined as those sires who lower the milk production and butter-fat percentage of their daughters as compared with the same variables in their dams. The superior sires so defined were arranged by the amount of butter-fat that they increase the production of their daughters over that of their dams. The inferior sires were classified according to the amount of butter-fat that they decrease the production of their daughters in comparison with that of their dams. These two groups of sires are subjected to four generation pedigree analysis to determine their inbreeding and relationship, the amount of Island and American stock, "males and females" and "on the sire's side of the pedigree and on the dam's side of the pedigree," and the individual animals most frequently repeated into two groups of pedigrees.

7. There are 28 sires in the group of sires superior in their transmitting qualities for milk production and butter-fat percentage. In the group of sires inferior in their transmitting ability for these two characters there are 47 sires, a ratio of 1 to 1.7. Such a difference speaks for itself. It emphasizes with startling clearness the need of exact knowledge of the transmitting qualities of bulls to be bred as sires and of the necessity for exact knowledge of the inheritance of milk production and butter-fat percentage.

8. The inbreeding coefficients show that the sires of superior merit have 7.08 % of the greatest possible inbreeding up to the fifth generation. The inferior sires are inbred 9.65% of the greatest possible amount (continued brother and sister mating). The group of sires poorer in their transmitting

qualities are consequently more inbred than the group of sires with superior transmitting qualities.

9. The analysis of the pedigrees for the amount of relationship that may exist between the sire and dam of the individual bulls in the superior group and in the inferior group shows that there is little or no difference in the amount of this relationship within the two groups.

10. The resolution of the four generation pedigrees into the Island bred Jerseys and by difference into the American bred Jerseys showed the mean number of Island males in the pedigrees of the superior sires' group to be 8.07 and the mean number of females 7.79. The mean number of Island bred males in the inferior sires' group were shown to be 6.94 and the mean number of females 6.55. The group of sires which increased the production of their daughters over that of their dams had, consequently, more Island bred stock in their pedigrees. The females in each group of the pedigrees had a smaller proportion of Island bred individuals than the males had in each of the groups.

11. Study of the pedigrees of these two groups of sires discloses the fact that all the animals which appeared in the pedigrees of the superior sire on the male side of the pedigrees more than four times or on the female side of the pedigree more than three times also had appearances in the pedigrees of the sires inferior in their transmitting qualities. This fact alone makes it clear that the appearance of certain famous animals in pedigree of a given bull is no guarantee of that particular bull's worth.

My former student and successor at the Maine Experiment Station, Dr. John W. Gowen, expects presently to publish similar investigations for the other chief dairy breeds. It is a great pleasure to acknowledge the faithful and painstaking help of my two assistants, Mr. John Rice Miner and Dr. John W. Gowen in bringing this investigation of the Jersey breed to completion. Without their aid it is doubtful if this would ever have been finished, particularly in view of the influences, which have operated on my own scientific work during the past three years.

---

[1] Pearl, R., and Surface, F. M., Studies on the Physiology of Reproduction in the Domestic Fowl II. Data on the Inheritance of Fecundity obtained from the Records of Egg Production of the Daughters of "200-Egg" Hens. *Maine Agric. Expt. Stat. Ann. Rept.*, 1909, pp. 49–84. Also Pearl, R., and Surface, F. M., Is there a Cumulative Effect of Selection? *Zs. Ind. Abst. Ver.*, **2,** 1909 (257–275).

[2] Pearl, R., Breeding for Production in Dairy Cattle in the Light of Recent Advances in the Study of Inheritance, *Eighth Ann. Rept. Comm. Agr. Maine,* 1910, pp. 118–129.

[3] The complete paper will have the following bibliographic status: Pearl, R., Gowen, J. W., and Miner, J. R. Studies in Milk Secretion, VII. Transmitting Qualities of Jersey Sires for Milk Yield, Butter-fat Percentage and Butter Fat, *Maine Agr. Expt. Stat. Ann. Rept. for* 1919, pp. 89–204.

[4] Pearl, R., On the law Relating Milk Flow to Age in Dairy Cattle, *Proc. Soc. Exp. Biol. Med.,* **12,** 1914 (18–19).

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CONTENTS

Page
Geology.—Differentiation by Deformation............By N. L. Bowen 159
Genetics.—The Evidence for the Linear Order of the Genes........... By T. H. Morgan, A. H. Sturtevant, and C. B. Bridge 162
Genetics.—The Arrangement of Genes in Drosophila Virilis............ By C. W. Metz 164
Physics.—The Functions of Intensity and Phase in the Binaural Location of Pure Tones..By G. W. Stewart 166
Physics.—The Measurement of Small Time Intervals and Some Applications, Principally Ballistic.. By L. T. E. Thompson, C. N. Hickman, and N. Riffolt 169
Physics.—Charcoal Activation........................By B. H. H. Sheldon 178
Mathematics.—The Starting of a Ship.............By James K. Whittemore 182
Mathematics.—A Thermodynamic Study of Electrolytic Solutions....... By F. L. Hitchcock 186
Botany.—The Formation of the Cell Plate in the Cambium of the Higher Plants...By Irving W. Bailey 197
Mathematics.—Functionals Invariant under One-Parameter Continuous Groups of Transformations in the Space of Continuous Functions...By I. A. Barnett 200
Physiology.—Thermokinetics of Liometopum Apiculatum Mayr........... By Harlow Shapley 204
Chemistry.—The Influence of Ions on the Osmotic Pressure of Solutions By Jacques Loeb 211
Zoology.—Evidence on the Nature of Nuclear Activity.................. By D. H. Tennent 217
Psychology.—A Study of the Persistence of Vision.......By A. C. Hardy 221
Genetics.—A Contribution of Genetics to the Practical Breeding of Dairy Cattle ....By R. Pearl 225

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CALCULATING ANCESTRAL INFLUENCE IN MAN

A MATHEMATICAL MEASURE OF THE DEMONSTRATED FACTS OF BI-SEXUAL HEREDITY

By Harry H. Laughlin

Eugenics Record Office, Carnegie Institution of Washington

Communicated by C. B. Davenport. Read before the Academy, November 10, 1919

By ancestral influence is meant the contribution of definite hereditary, mental, physical and moral qualities, by a given ancestor to a given descendant. Pedigree-studies have set forth in man, as in many other organisms, rules of inheritance of many specific traits or qualities. Cytological investigations have shown how chromosomes duplicate, segregate and recombine (sometimes crossing-over and showing other special phenomena) in the course of gametogenesis and fertilization.

In bi-sexual species of plants or animals, the germ-plasm is not indefinitely dilutable or divisible as many genealogists have assumed, but its dilutability and divisibility are limited by the number of chromosome-characteristics of the gametes of the particular species. In the human zygote two gametes each with 12 chromosomes unite. This means that, barring crossing-over, mutation and other special phenomena, normally the human germ-plasm, and consequently all hereditary human qualities, that is, the "blood," is divisible only into 24 unequal units corresponding to the 24 zygotic chromosomes.

Note—There is still disagreement among cytologists concerning the number of chromosomes in man.

Wieman, H. L., "The Chromosomes of Human Spermatocytes," *Amer. J. Anat.*, **21**, No. 1, January, 1917. This paper concludes that in man the zygotic or diploid number of chromosomes is 24, of which 2 are the *xy* idiochromosomes.

von Winiwarter, H., "Études sur la spermatogenèse humaine," *Arch. Biol.*, **27**, 1912. This paper concludes that the zygotic number of chromosomes in the human male is 47, in the human female, 48.

See also Stevens, Guyer, Montgomery and Evans.

When this problem is settled beyond peradventure, the correct number can be used in the accompanying formulas which are here stated in general terms.

It is well demonstrated that the chromosomes are composed of genes or the determiners of natural inheritance. Each chromosome is a gene-radical or linkage-group of trait determiners. The chromosomes are the germ-plasm. If then it is sought to measure the influence of a given ancestor upon the hereditary traits of a given propositus, it is necessary to develop a series of mathematical formulas which measure the probabilities of definite situations happening in reference to the transmission of chromosomes from the given ancestor to the given propositus.

In developing such formulas the following factors must be taken into consideration:

First. Whether the species in question is bi-sexual.

Second. Whether in the particular species the male or the female is the heterozygous sex.

Third. Due provision must be made for sex-linked traits.

Fourth. The number of chromosomes, both gametic and zygotic, characteristic of the species, must be known.

Fifth. For the present the individual chromosomes will not be weighed, but simply counted. Doubtless the ontogenetic working out of each chromosome is very specific both qualitatively and quantitatively. When ultimately genetical and cytological research locate in definite chromosomes the genes for definite traits, then the several chromosomes can be weighted, and due provision for their differential values properly made in the formulas for measuring the several features of ancestral influence.

Sixth. Due cognizance must be taken of the possibility of mutation, crossing-over, non-disjunction and other special phenomena occurring in the course of the descent of chromosomes from a given ancestor to the propositus. In the accompanying formulas such phenomena are not provided for, because their rates of occurrence are not yet measurable in man, but when one by one their mathematical measures are attained, each such measure can be woven into the accompanying foundation formulas. The present formulas will not be upset by such corrections, but simply modified in the direction of greater accuracy.

Seventh. It must be considered that in a given gamete a definite chromosome may have been derived from either the paternal or the maternal source. This is especially important in cases of zygotes of mixed blood.

Eighth. In certain situations it will be necessary to specify whether a given trait is dominant or recessive to its allelomorph.

Finally, all formulas must be stated in general terms, so that they may apply with equal accuracy to any species concerning which chromosomal data are at hand.

There are many features of ancestral influence which may be mathematically formulated. Among them are (1) the average contribution in number of chromosomes that a given ancestor, taken as a type, will contribute to the propositus; (2) the range in such numbers; (3) the probability that the given ancestor, taken as a type, will contribute all of the hereditary influences which the particular half-pedigree furnishes to the propositus; (4) the probability that the particular propositus, and consequently all of his or her direct ancestors, will be entirely eliminated as a contributor to the hereditary constitution of the propositus; (5) that a definite number of chromosomes specified as to maternity or paternity of origin, carried by a given ancestor, will by such ancestor be passed on to the propositus; (6) as one of many special cases, the probability that a given ancestor who appears once or more in each the maternal and paternal half-pedigree, will be duplicated exactly in zygotic chromosomal constitution by the propositus.

The principles set forth in the paper of which this is an abstract make possible the mathematical formulation of an indefinitely great number of complex situations in reference to ancestral influence. The completer paper presents in detail twenty-seven such formulas. Eight of these (numbers 1, 4, 5, 6, 7, 8, 9 and 10) for each individual pedigree-place, to which the particular formulas apply, were worked out for each member of the first five ancestral generations in man and shown on charts, entitled "Ancestral Influence in the Human Male," and "Ancestral Influence in the Human Female." (In the accompanying charts bearing the above titles these same formulas are worked out for the first three ancestral generations only.) These particular calculations in reference to man are based upon the following data and conditions: (*a*) that the gametic number of human chromosomes is 12; the zygotic or somatic number in each sex 24; (*b*) that the male is heterozygous (*xy*) and the female homozygous (*xx*) in reference to sex; (*c*) that all of the purely hereditary qualities are carried by the several chromosomes; and (*d*) that in the first or foundation formulas no provision will be made for mutation, crossing-over, non-disjunction or other special phenomena.

These eight formulas worked out in detail on the human pedigree-charts follow:

No. 1. A = Average number of chromosomes passed on by the selected ancestor, taken as a type, to the F_1 zygote, i. e., the propositus.

A = Also the average number of chromosome-pairs each of which contributes one member to the diploid or somatic group of chromosomes of the F_1 zygote.

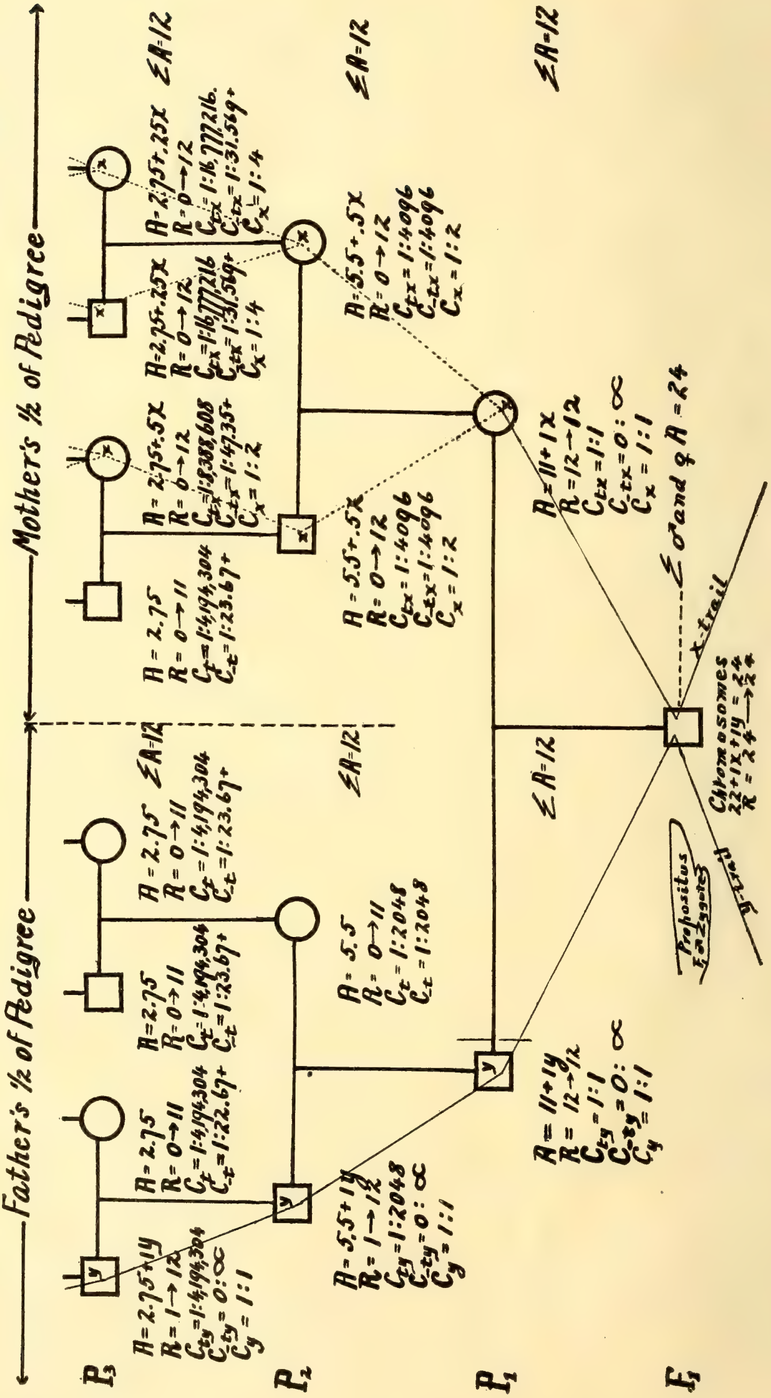
In man the diploid or somatic number of chromosomes is taken tentatively at 24, i. e., 11 equivalent pairs and one *xx* or *xy* sex-pair.

$$A = \frac{n}{2^{(p-1)}} + \Big\} (C_y \text{ or } C_x \text{ in case of ancestors from whom an } x\text{- or a } y\text{- chromosome } may \text{ descend.})$$

See notes on *x*- and *y*- trails, under formulas 4 and 5.

n = Number of equivalent chromosome-pairs (i. e., excluding the *xy* or the *xx* sex-pair) characteristic of the species. In man n is taken tentatively at 11.

p = the numerical value of the sub-figure of P used in pedigree-manner



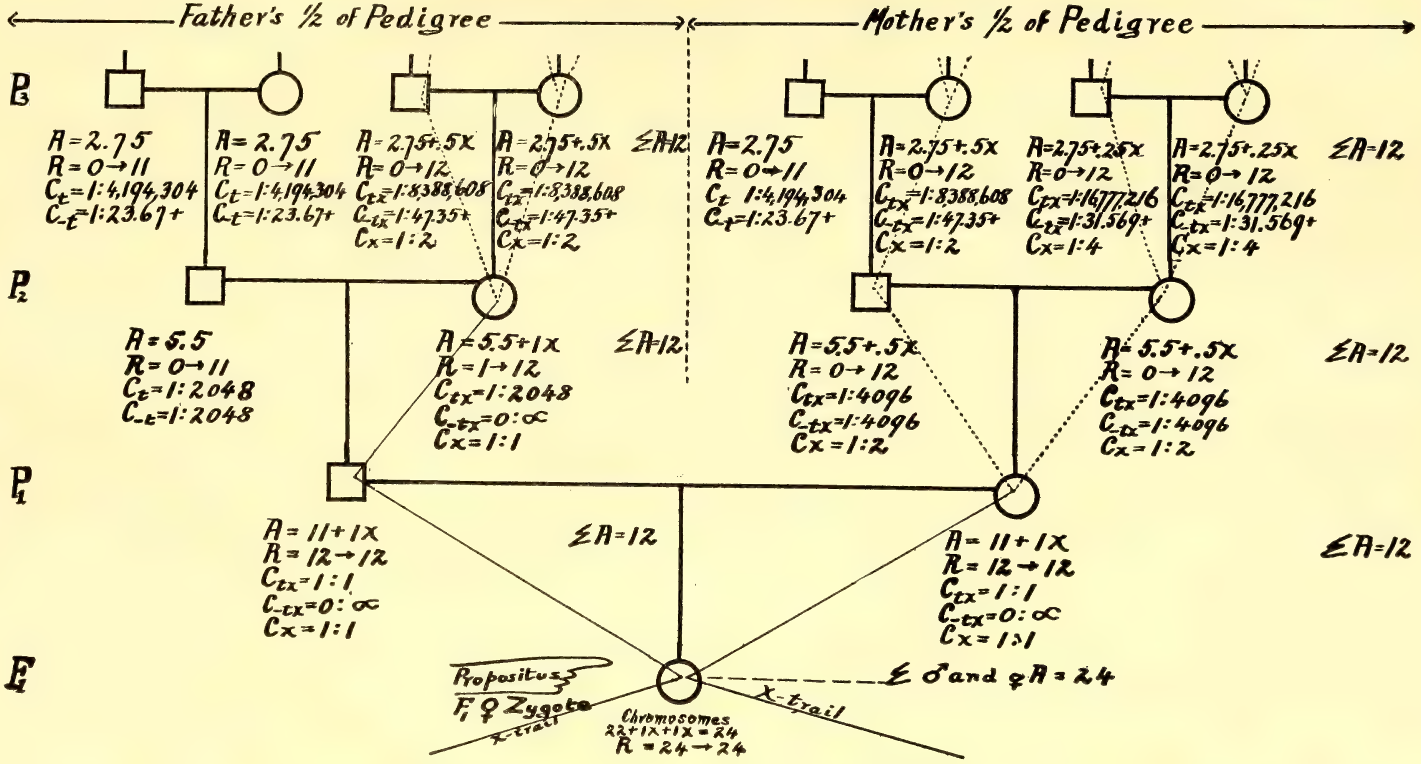
1. Ancestral Influence in the Human Male

based upon average, range and chance in the contribution of chromosomes to the F_1 Zygote.

Father's ½ of Pedigree
Mother's ½ of Pedigree
P3
P2
P1
F1
A=2.75+1y R=1→12 Cty=1:4,194,304 C-ty=0:∞ Cy=1:1
A=2.75 R=0→11 Ct=1:4,194,304 C-t=1:22.67+
A=2.75 R=0→11 Ct=1:4,194,304 C-t=1:23.67+
A=2.75 R=0→11 Ct=1:4,194,304 C-t=1:23.67+
ΣA=12
A=2.75 R=0→11 Ct=1:4,194,304 C-t=1:23.67+
A=2.75+.5x R=0→12 Ct=1:8,388,608 Ctx=1:47.35+ Cx=1:2
A=2.75+.25x R=0→12 Ct=1:16,777,216 Ctx=1:31.569+ Cx=1:4
A=2.75+.25x R=0→12 Ctx=1:16,777,216 C-tx=1:31.569+ Cx=1:4
ΣA=12
A=5.5+1y R=1→12 Cty=1:2048 C-ty=0:∞ Cy=1:1
A=5.5 R=0→11 Ct=1:2048 C-t=1:2048
ΣA=12
A=5.5+.5x R=0→12 Ctx=1:4096 C-tx=1:4096 Cx=1:2
A=5.5+.5x R=0→12 Ctx=1:4096 C-tx=1:4096 Cx=1:2
ΣA=12
A=11+1y R=12→12 Cty=1:1 C-ty=0:∞ Cy=1:1
ΣA=12
A=11+1x R=12→12 Ctx=1:1 C-tx=0:∞ Cx=1:1
ΣA=12
Σ ♂ and ♀ A=24
x-trail
y-trail
Chromosomes 22+1x+1y=24 R=24→24
Propositus F1 Zygote

2. Ancestral Influence in the Human Female

based upon the average, range and chance of contribution of chromosomes to the F_1 Zygote.


Father's ½ of Pedigree
Mother's ½ of Pedigree
P_3
P_2
P_1
F_1
$A=2.75$ $R=0\to 11$ $C_t=1:4,194,304$ $C_{-t}=1:23.67+$
$A=2.75$ $R=0\to 11$ $C_t=1:4,194,304$ $C_{-t}=1:23.67+$
$A=2.75+.5X$ $R=0\to 12$ $C_{tx}=1:8,388,608$ $C_{-tx}=1:47.35+$ $C_x=1:2$
$A=2.75+.5X$ $R=0\to 12$ $C_{tx}=1:8,388,608$ $C_{-tx}=1:47.35+$ $C_x=1:2$
$\Sigma A=12$
$A=2.75$ $R=0\to 11$ C_t $1:4,194,304$ $C_{-t}=1:23.67+$
$A=2.75+.5X$ $R=0\to 12$ $C_{tx}=1:8,388,608$ $C_{-tx}=1:47.35+$ $C_x=1:2$
$A=2.75+.25X$ $R=0\to 12$ $C_{tx}=1:16,777,216$ $C_{-tx}=1:31.569+$ $C_x=1:4$
$A=2.75+.25X$ $R=0\to 12$ $C_{tx}=1:16,777,216$ $C_{-tx}=1:31.569+$ $C_x=1:4$
$\Sigma A=12$
$A=5.5$ $R=0\to 11$ $C_t=1:2048$ $C_{-t}=1:2048$
$A=5.5+1X$ $R=1\to 12$ $C_{tx}=1:2048$ $C_{-tx}=0:\infty$ $C_x=1:1$
$\Sigma A=12$
$A=5.5+.5X$ $R=0\to 12$ $C_{tx}=1:4096$ $C_{-tx}=1:4096$ $C_x=1:2$
$A=5.5+.5X$ $R=0\to 12$ $C_{tx}=1:4096$ $C_{-tx}=1:4096$ $C_x=1:2$
$\Sigma A=12$
$A=11+1X$ $R=12\to 12$ $C_{tx}=1:1$ $C_{-tx}=0:\infty$ $C_x=1:1$
$\Sigma A=12$
$A=11+1X$ $R=12\to 12$ $C_{tx}=1:1$ $C_{-tx}=0:\infty$ $C_x=1:1$
$\Sigma A=12$
Propositus F_1 ♀ Zygote
X-trail
Chromosomes $22+1X+1X=24$ $R=24\to 24$
X-trail
Σ ♂ and ♀ $A=24$

to indicate the ancestral or parental generation in relation to F_1. Thus in the great-grandparental generation P_3, $p=3$.

No. 4. C_y = the average number of y-chromosomes contributed to the F_1 zygote by the particular *male* ancestor taken as a type.

No. 1. C_y = the probability that a particular *male* ancestor will contribute the single y-chromosome found in the F_1 *male* zygote.

The value of $1 : C_y$ is always $1 : 1$, and it is applicable to the *direct male* line only.

This direct paterial line down which the y-chromosome descends is the so-called "y-trail."

No. 5. C_x = The average number of x-chromosomes contributed to the F_1 zygote by the particular ancestor taken as a type.

$1 : C_x$ = The probability that the particular male or female ancestor will contribute an x-chromosome to the F_1 zygote.

The value of C_x is determined by the following rule:

(*a*) Beginning with the x-chromosomes in the F_1 zygote, without exception (i. e., with the chance of $1 : 1$) the *single* x-chromosome in the *male* is derived from his *mother;* and of the *two* x-chromosomes in the *female, one* is derived from *each parent.*

(*b*) In the P_1 and all older generations, in the case of a female zygote the chance of the descent of an x-chromosome from the *father* or *mother* is *equal* (i. e., $1 : 2$), but in the *male* the x-chromosome is always received from the *mother* alone (i. e., the chance is $1 : 1$).

Therefore, the probability that a given ancestor, taken as a type, will contribute an x-chromosome to the F_1 zygote (i. e., $1 : C_x$), ise qual to the product of all probability ratios, made according to rules (*a*) and (*b*) above, along the x-trail from the selected ancestor to the P_1 generation inclusive.

Thus $1 : C_x$ for the mother's mother's father of a female zygote = $(1 : 1).(1 : 2).(1 : 2) = 1 : 4$; but for the mother's father's mother of a female zygote $1 : C_x = (1 : 1).(1 : 2).(1 : 1) = 1 : 2$.

Thus the so-called "x-trail"—from a *son to the mother only,* but from a *daughter to both the father and mother,* traces the several lines in the ancestral pedigree down *one* of which an x-chromosome *must* descend.

NOTE.—The x-trail and the y-trail and all calculations herein based upon them apply only to those species in which the male is the heterozygous and the female the homozygous sex. In species in which the reverse is true the x- and the y-trails exchange positions. The general rule is that the y-trail follows the heterozygous sex; the x-trail from the homozygous individual to both parents, but from a heterozygous individual to the homozygous parent only.

No. 6. R = Range in number of chromosomes (i. e., in number of single members of chromosome-pairs) possible for the selected ancestor, taken as a type, to contribute to the 24 (i. e., 11 equivalent pairs and 1 xy or 1 xx pair) chromosomes of the F_1 zygote.

In a parent of the P_1 generation, $R = 12 \longrightarrow 12$, i. e., there is no range in the number of chromosomes which the given parent may contribute to the F_1 zygote; this number is always 12. For all ancestors of the P_2 and all older generations, $R = 0 \longrightarrow 11$, except (*a*) for those ancestors from whom an *x*- or a *y*-chromosome *must* certainly descend (solid line *x*-trail or *y*-trail in chart), in whom $R = 1 \longrightarrow 12$; except also (*b*) for those ancestors from whom an *x*-chromosome *may* by alternate chance, descend (dotted line *x*-trail in chart) in whom $R = 0 \longrightarrow 12$.

In a general formula 11 would be substituted for by n, and 12 by $n + 1$, with value as in formula 1.

No. 7. $1 : C_t$ = The probability that *one or the other* member of *each* of *all* of the *equivalent* chromosome-pairs characteristic of the species (i. e., an entire chromosome-series excluding the *xx* and *xy* pairs), from the given ancestor will enter the F_1 zygote.

In the case of ancestors not in the *x*-trail or the *y*-trail, such an occurrence is the extreme limit of ancestral influence possible to a given ancestor, and its happening precludes any hereditary influence at all on the propositus, i. e., the F_1 zygote, on the part of any other ancestor of the same paternal or maternal half-pedigree, as the case may be, except the lineal ancestors and descendants of the given ancestor.

$1 : C_t = 1 : (2^n)^{p-1}$

n = Value as in formula 1. In man n is taken tentatively at 11.

p = Value as in formula 1.

No. 8. $1 : C_{tx}$ or $1 : C_{ty}$ = The probability that *one or the other* member of *each* of the several equivalent chromosome-pairs characteristic of the species, and *also* one *x*- or one *y*-chromosome, as the case may be, from the given ancestor will enter the F_1 zygote. (In man this total gametic number of chromosomes is taken tentatively at 12.)

Such an occurrence can happen only in reference to ancestors in the *x*- or *y*-trails, and its happening precludes any hereditary influence at all on the propositus, i. e., the F_1 zygote, on the part of any other ancestor of the same paternal or maternal half-pedigree, except the lineal ancestors and descendants of the given ancestor.

$1 : C_{tx} = (1 : C_t).(1 : C_x)$

Values of $1 : C_t$ and $1 : C_x$ as in formulas 5 and 7.

$1 : C_{ty} = (1 : C_t).(1 : C_y)$

Values of $1 : C_t$ and $1 : C_y$ as in formulas 4 and 7.

No. 9. $1 : C_{-t}$ = The probability that *no member* of *any* of the equivalent chromosome-pairs characteristic of the species (thus barring for the time being all consideration of the *xx* or *xy* chromosome-pairs) from the given ancestor will enter the F_1 zygote.

In such an occurrence the given ancestor and all of his or her lineal ancestors are *entirely eliminated* as contributors to the natural inheritance of the propositus, i. e., the F_1 zygote, *provided* that such given ancestor is not in the x-trail or y-trail, and does not, due to inbreeding, appear elsewhere in the ancestral pedigree.

$$1 : C_{-t} = 1 : \left(\frac{2^{p-1}}{2^{p-1} - 1} \right)^n$$

n = Value as in formula 1.
p = Value as in formula 1.

No. 10. $1 : C_{-tx} =$ or $1 : C_{-ty} =$ The probability that no member of any equivalent chromosome-pair, *nor* of the *xx* or *xy* chromosome-pair, from the given ancestor will enter the F_1 zygote, i. e., the probability (barring crossing-over and other special phenomena, and the appearance of the given ancestor elsewhere in the pedigree), of the *complete elimination* of the hereditary influence of the given ancestor and all of his or her lineal ancestors, on the propositus, i. e., the F_1 zygote.

This formula is applicable *only* to those ancestors *in the x-trail or y-trail.*

$1 : C_{-tx} = (1 : C_t).(1 : C_{-x})$
$1 : C_{t-y} = (1 : C_{-t}).(1 : C_{-y})$
Values of $1 : C_{-t}$ as in formula 9

$$C_{-x} = \frac{C_x}{C_{x-1}}$$

$$C_{-y} = \frac{C_y}{C_{y-1}}$$

Summary.—The present problem of measuring ancestral influence by tracing chromosomes or gene-s-radical is demonstrated to be a special case under the following general situation:

Given: First. A single unit factor (a gene) in a known situation (in the P_1 generation) and the quotient measuring the probability of its reacting in a definite manner or reaching a definite new situation at the end of a single system of moves or transformations (in the F_1 generation).

Second. The demonstrated synchronization or sequence in the systems of moves or transformations of two or more such unit factors.

By applying the correct principles of combination and chance, the probability that a given complex situation will result from a given set of constituent conditions at the end of a given number of definite kinds of transformations may be mathematically formulated.

In case of measuring ancestral influence by tracing chromosomal descent, the situations and transformations concern definite chromosome-complexes and their breaking-up, their re-combination and their transmission to offspring.

The formulas here set forth are foundational. They are stated in general terms whose validity depends upon their presenting correct mathematical pictures of the chromosomal processes which work out in the germ-cell cycle.

THE EFFECT OF ANESTHETICS ON LIVING CELLS

BY GRACE MEDES AND J. F. McCLENDON

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Communicated by A. G. Mayor, March 17, 1920

The object of this research is to determine the effect of different anesthetics on several activities or properties of living cells, permeability, oxygen consumption, CO_2 production, photosynthesis, protoplasmic streaming and cell structure. Plant cells, Elodea as well as animal cells, Cassiopea, were used. Not all anesthetics had the same effect and the same anesthetic affected the same activity of a plant differently from an animal and affected different activities of the same cell differently. One correlation, however, was evident. All the anesthetics tried increased plant cell respiration and permeability. What might seem to be exceptions to this rule occurred under overdoses of the anesthetic and we were dealing with dead cells, which are outside the limits of this research.

All of the experiments were performed in thermostats at 30°. The Cassiopeas were kept in sea water, usually of alkaline reserve = 0.0025 N and $p_H = 8.2$ at the start, and the Elodea in a distilled water solution of $NaHCO_3$ of alkaline reserve = 0.0025 N and $p_H = 7.6$. At this alkaline reserve a change in p_H of 0.1 was produced by a change in CO_2 content per liter of 1.5 cc. By determining the change in p_H colorimetrically the production of CO_2 could be estimated. The oxygen consumed was determined by the Winkler method. The results were expressed in cc. of tenth normal thiosulphate for 100 cc. water in the Elodea experiments and as cc. of O_2 in the Cassiopea experiments. Since these data cover many pages even when condensed into tabular form, it is thought best to convert the results into percentages of the normal and summarize them. In the first place, higher concentrations of anesthetics evidently killed the cells and it was necessary to decide at just what concentration permanent injury occurred in order to separate such data from that concerning reversible changes characteristic of anesthesia. In the higher concentrations of anesthetics Elodea cells became irreversibly plasmolyzed (false plasmolysis), respiration and photosynthesis ceased, protoplasmic rotation stopped, the chloroplasts shrunk to a very small size and gave out some chlorophyll to the surrounding water, and there was a very sharp rise in the exosmosis of chlorides from the cells. On return to water, respiration did not commence again and the cell became the prey of bacteria. The cell might recover, however, after lowering of the rate of photosynthesis. Cessation of respiration seemed to be the best criterion of death in Elodea, but if the cells did not all die at the same time, the death of some cells could not be detected by this means. It seemed that a cell might live some time after protoplasmic rotation had ceased, but such cessation was an index that the lethal dose of anesthetic was approached, and it had the advantage of being observable in individual cells.

If the experiments in which protoplasmic rotation had ceased are omitted we may be confident that we are dealing with living cells and may summarize the data in the following table:

	PERCENTAGE CONCENTRATION	ABSORPTION OF O_2	ELIMINATION OF CO_2	EXOSMOSIS OF CHLORIDES	PROTOPLASMIC ROTATION	PHOTOSYNTHESIS	SIZE OF CHLOROPLASTS
Water		100	100	100	100	100	100
Alcohol	0.05			100		94	100
	1.00	112	105	100	100	75	100
	1.50	135	125	150	105	62	99
	3.00	189	140	500	135	44	79
	6.00	163	175	2000	71	00	52
Ether	1.50	150	117	200	90	49	76
	3.00	186	133	1000	30	12	68
Chloroform	0.05	113	100	1500	130	47	91
Chloretone	0.05	117	150	150	71	100	100

In the above table the results with 6% alcohol might best be omitted. Some shoots of Elodea seem to be injured by the alcohol and it seems probable that some dead cells may have been used in each experiment. Since rotation did not cease but was only retarded in the cells observed, these data are included to avoid criticism. In some cases rotation stopped in 3% ether.

It may be seen from the table that all the anesthetics caused increased oxygen consumption and the greater the concentration of the anesthetic the more effective it was. There seemed to be some delay in the elimination of CO_2 as shown by successive time periods in the same experiment. This factor and the lesser sensitiveness of the method may be the reason cause for the failure of exact agreement between the CO_2 and the O_2 data. With marked increase in respiration there was also marked increase in exosmosis of chlorides as determined with the nephelometer. Protoplasmic rotation might be increased or decreased by the anesthetic, whereas photosynthesis was always decreased. In higher concentrations of the anesthetic the size of the chloroplasts was markedly reduced.

Since the effect of anesthetics on the conscious and automatic (rhythmical) centers in the central nervous system has been so thoroughly studied, it was decided to omit such data. Cassiopeas of about 15 cm. diameter were deprived of their automatic nerve centers by cutting off the margin of the bell. They were also deprived of the manubrium in order to eliminate the numerous mucous glands of that organ. The respiratory quotient was found to be about 95, and since the CO_2 data are less accurate than the oxygen data, only the latter are given. It was found that nerve conduction in the bell was abolished in one-half per cent ether. The jellyfish died at the end of an hour in 3% ether but by limiting the experiment to one hour, reliable data could be obtained at this concentration. Ether concentrations of one-half per cent, one per cent, two per cent and three per cent had no effect on the rate of respiration.

Owing to limited time it was thought best not to run through a long list of anesthetics in a hurried manner with no basis for hopes that a change in respiration rate might be observed, but to look for one which had been reported to change the rate. Carbon dioxide has been considered by many biologists as an anesthetic and used to anesthetize lower animals. I had previously observed that an increase in hydrogen ion concentration of the sea water might lower the respiration rate of Cassiopea, but had not determined whether this was due to H ions alone or to an associated increase in CO_2. The following experiments show that the metabolism is affected by CO_2 and not by increase in H ions without increase in undissociated CO_2 molecules.

Although Tortugas sea water contains about 44 cc. CO_2 per liter when estimated by adding acid and pumping it out, only a small fraction of this exists as CO_2 molecules in the water, most of it being in the form of HCO_3' and CO_3'' ions. If more CO_2 is added to the water a large increase in undissociated CO_2 molecules may be obtained. If HCl is added to the water, a limited increase in CO_2 molecules may be obtained, due to decomposition of carbonates and bicarbonates. An increase in H ions without increase in CO_2 molecules or even with decrease in CO_2 molecules may be obtained by adding enough HCl to neutralize the alkaline reserve, pumping out the CO_2 and adjusting the H ion concentration by minute additions of H_3PO_4. The results on relative rates of oxygen consumption after these methods of increasing H ions are as follows:

p_H	8.2	6.6	6.3	5.8	5.7	5.5
By adding CO_2	100	96	92	62	41	24
By adding HCl	100	98		70		
By adding HCl and removing CO_2	100			95		

In the above table the decrease of 5% in oxygen consumption after removing CO_2 is well within the limits of error of the method and is strikingly different from the decrease of 38% on adding CO_2 to obtain the same p_H without removal of CO_2. Therefore, the increased H ions are not responsible for the great fall in respiration after addition of CO_2.

In conclusion it seems evident that all anesthetics may not act in exactly the same way or exactly the same on both plants and animals, and that, whereas anesthetics may induce surface changes assumed to be associated with permeability, they are active on the inside of the cell as evidenced in shrinkage of chloroplasts. Anesthetics affect different cell activities differently. This may be due to effects on different cell structures : plasma membrane, chloroplasts and cytoplasm.

A GENERAL SINKING OF SEA-LEVEL IN RECENT TIME

By Reginald A. Daly

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Communicated by J. M. Clarke, March 10, 1920

Among the causes of world-wide or "eustatic" shifts of sea-level are: appropriate crustal movements whereby the volume of the ocean basin is changed; delta-building and volcanic extravasation on the sea-floor, the displacement of sea-water not being compensated by crustal subsidence; volcanic addition of new water to the ocean; subtraction of water which becomes chemically bound during the alteration of rocks; glaciation on land, lowering sea-level by the abstraction of water from the ocean; deglaciation on land, raising sea-level; changes in the earth's center of gravity and in her speed of rotation. Numerous as these possibilities are, eustatic shifts at definite times and in definite amounts have not often been postulated by geologists. The proof of such a shift, either positive or negative, is not easy. The present note records some field facts suggesting the probability of a sinking of general sea-level to the extent of nearly 20 feet, during the Human period. The suggestion is published, not to express a fixed conclusion but to invite criticism by those familiar with shorelines in different parts of the world, for its testing is manifestly a world-wide problem. The matter is important because it affects judgment as to the reality of many so-called uplifts of land or sea-bottom, and because a close study of the latest shift of sea-level may help to systematize the criteria for earlier and larger eustatic shifts.

Nearly twenty years ago the hypothesis of a recent, negative movement of sea-level first presented itself to the writer while engaged in the correlation of post-Glacial elevated strands in Labrador and Newfoundland with those of Quebec. The highest strand mapped between Newfoundland and Nachvak Bay, 500 miles to the northwest, was found to be strongly warped, like the highest strand from Newfoundland to Massachusetts. On the other hand, the lowest emerged terrace along the shores of the Gulf of St. Lawrence was seen to be conspicuously level throughout a distance of 300 miles or more below Quebec City. This apparent uniformity of level prompted the question whether this particular strand had been abandoned by the waves because of a sinking of general sea-level. For lack of accurate leveling, correlation with the more closely studied, lowest beaches of Labrador was impossible, and with the data in hand serious belief in the hypothesis of a eustatic change was withheld.

However, that explanation was recalled by the discovery of new facts during recent field studies on the shores of New England, Florida, and the Samoan Islands. Meantime Goldthwait had made a careful study of the St. Lawrence strand, to which he gave the name "Micmac terrace;" the results of his leveling agree so well with those made in the other regions mentioned that the suspicion of past years became a hypothesis deserving more attentive consideration.

For the opportunity to collect data from the two tropical regions the writer is indebted to Dr. A. G. Mayor, Director of the Marine Laboratory of the Carnegie Institution of Washington.

In his excellent account of the Micmac terrace, Goldthwait noted the nearly uniform level of its inner edge at about 20 feet above high tide, as well as its relatively great strength and continuity, with width varying from a few feet to 1.5 miles or more. Though only a part of its width is due to wave-cutting and though the formations affected are weak, the higher level of the sea must have been steadily kept for a large fraction of post-Glacial time.[1] Farther east in the Gulf of St. Lawrence, on Anticosti, and again in Nova Scotia, Twenhofel has found wide terraces at about the same level.[2]

A similar feature characterizes the coast of Maine, as emphasized in the folios and monographs of the United States Geological Survey and as confirmed by the present writer.

Mayor had already found wave-cut rock benches, about 8 feet above high tide on all sides of Tutuila, the largest island of American Samoa.[3] The island is 16 miles long. At its eastern end is Aunuu island, and about 60 miles farther east is Tau island of the Manua group. The writer found very fine wave-cut benches at 8 to 10 feet above high tide on Aunuu and Tau, and others at the same general elevation on Olosega island, 7 miles west of Tau. The distance between the most widely separated benches is 75 miles; nowhere in that long stretch did the bench crests depart essentially from a constant level. Explanation by local uplift was at once seen to be highly improbable, for crustal uplift of such uniformity is unknown to geology.

Eight or ten feet of bench elevation does not, however, measure the total of recent emergence in Samoa. At several points in Tutuila large sea-caves were found. Their floors varied in height, from 14 or 15 feet at their mouths to 25 feet at the back walls of the caves. Many caves of similar forms and cut in similar rocks are being made by the surf of the present day. The lower lips of these newer caves are characteristically 4 to 6 or more feet below high-tide level and the floors rise inwards to heights of a few feet above high tide. Hence, the emerged caves were cut when the sea-level was nearly 20 feet higher than now. That assumption also explains the 8-foot benches, for the cliffs now being cut on the Samoan headlands (where not protected by coral reefs) have regularly about 12 feet of water directly at their feet during high tide. The writer fully agrees with Mayor's conclusion that reef corals were not living on any of these Samoan shores when the emerged caves and benches were developed. Hence, the nearly or quite reefless headlands of the present time are those where the relation between sea-level and bench-level of the earlier period is best determined.

Correlation of the Samoan and North American terraces was suggested not only by the agreement in level but also by the state of preservation of the strand-marks, as well as by the comparable amounts of work done by the waves in the two regions during the higher stand of the sea.

The hypothesis of a eustatic shift must obviously meet the test of matching the facts observable on the shores of every continent and of most islands. The application of the test is difficult for many reasons which need not here be recounted, but a partial study of the records scattered through geological literature shows that the hypothesis merits discussion and further testing. The actual compilation of facts relates to the British Isles, the Atlantic coast of North America south of New York, the West Indies, Brazil, Patagonia, Antarctica, New Zealand, Australia, and many groups of the Pacific islands. For details the reader is referred to an article in a forthcoming number of the Geological Magazine (London).

A specially instructive example is seen in the agreement of the lower Cape May terrace of New Jersey and Pennsylvania, the Talbot terrace of Maryland (marine part), the Satilla terrace of Georgia, and the lower Pensacola terrace of Florida in age, height and strength of development, both among themselves and with the other terraces above-mentioned. The Pensacola-Satilla terrace alone keeps a practically uniform set of levels (maximum about 20 feet) for an air-line distance of 500 miles; the explanation of its emergence by crustal uplift seems hardly credible.

According to Andrews, "indications of the apparent elevation of the shore-line for a few feet are to be found along the whole eastern side of Australia;" he attributes the cause to "either an apparent rise of the land, or a retreat of the sea."[4] Süssmilch specifies the amount of the emergence (New South Wales) as "10 to 20 feet."[5] Cadell notes similarly recent emergence of western Australia by amounts between the same limits.[6] In this case nearly or quite uniform emergence has affected a thousand miles of coast on one side of a continent and several hundred miles on the other side, and again there is practical agreement with the amount of emergence shown in the mid-Pacific, in North America, and in many other regions.

Thus, in spite of complications, including the present-day warping of some coasts, the hypothesis appears to stand the test involving the amounts of local emergence.

A second test naturally inheres in the expectation that the deleveling should be synchronous in all parts of the ocean. Observers dwell on the recency of the sea-level shifts registered in all of the cases compiled. Fossils from the St. Lawrence, Scottish, Irish, Cuban, Patagonian, New Zealand, Australian, Funafuti, Murray Island, New Jersey, Maryland, Georgia, and Florida beaches, reefs, or coastal plains belong to species now living in the adjoining seas, or, exceptionally, to species now living in slightly warmer water. Great recency is also shown by the relatively small damage done by erosion to the strand-marks, except in the case of those exposed to powerful surf. The beaches of the higher latitudes are clearly post-Glacial and also of later dates than the post-Glacial uplifts following the melting of the ice-caps. Brögger puts the completion of the last 4-meter to 8-meter emergence of the Christiania region at about the year 500 B.C. The development of the corresponding sea-cliffs, benches, and other strand-marks is referred by him to the late Tapes (Neolithic) period, between 1400 B.C. and 2400 B.C. During late Tapes time the Christiania sea was about 2° C. warmer than now.[7] The fossils in the Patagonian beaches also betoken water warmer than the adjacent sea.

Simultaneous emergence of the many separate regions is the more credible because of the similarity in the histories of their respective coasts before emergence. Repeatedly one encounters the description of terrace, bench or beach near the 20-foot level as the best developed, the most conspicuous, of the most persistent in areas characterized by "raised" strand-marks. One may conclude that the different regions must have had sea-level nearly constant for a considerable time, during which the strand-marks were well incised or built up. Such widespread accordance itself must deepen suspicion that local deformations of the earth's crust have not been responsible for the recent emergence of so many continental and island coasts.

In conclusion, the facts at hand seem to permit belief in the synchrony of the different strand-markings and emergences here considered, but this second test of the hypothesis clearly demands further investigation.

Before the fact is established, it would be idle to dwell overmuch on the explanation of a 20-foot drop in the level of the ocean. The most promising idea appears to be that a few thousand years ago there was an increase in the volume of the existing, non-floating glaciers. If the Antarctic ice-cap were then thickened to the average amount of about 700 feet, an average sinking of sea-level to the extent of nearly 20 feet would be inevitable. In favor of this suggestion would be evidence of a world-wide oscillation of climate like that which seems to have affected the Christiania region in Recent time. If the whole earth was, in the Tapes period, a little warmer than now, less water may have been taken from the ocean to constitute the ice-caps; sea-level was a little higher than at present. The oscillation as a whole would be but an incident in a series of climatic and oceanic changes which began with the opening of the Glacial period.

[1] Goldthwait, J. W., *Amer. J. Sci.*, **32,** 1911 (293).

[2] Twenhofel, W. H., *Ibid.*, **28** ,1909 (147); **30,** 1910 (66, 69).

[3] Mayor, A. G., These PROCEEDINGS, **3,** 1917 (523).

[4] Andrews, E. C., *New South Wales Handbook,* Brit. Assoc. Adv. Sci., 1914 (525, 532).

[5] Süssmilch, C. A., *Introduction to the Geology of New South Wales,* Sydney, 1911 (154).

[6] Cadell, H. M., *Trans. Edinburgh Geol. Soc.*, **7,** 1897 (179).

[7] Brögger, W. C., *Norges geol. Unders.*, No. 31, 1901 (713), and No. 41, 1905.

A NOTE ON THE HUMAN SEX RATIO

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Communicated by R. Pearl, March 20, 1920

From data collected at the Sloane Maternity Hospital in New York City, it has recently been shown[1] that there is a significantly higher male sex ratio at birth among progeny whose parent are not of similar nationality. This result confirmed a paper published by the Pearls[2] in 1908, based on data derived from vital statistics of the City of Buenos Ayres, and had in addition a decreased source of error due to improved methods of recording the data and the inclusion of still births as well as living births in the calculations.

During the past year, a further study of the same records has been made concerning the sex ratio among the progeny of certain other types of matings and a preliminary attempt at analysis made by contrasting the sex ratios of the offspring of *primipara* with those of subsequent births.

Table 1 shows the sex ratios of five categories of matings: European "pure," European "hybrid," United States white, British West Indies colored, and United States colored. European pure are matings in which both parents were of the same nationality. European hybrids are offspring whose parents are of different nationality, United States white include a random sample of parents coming from various parts of the United States but preponderantly from the East and especially from New York State and New York City. British West Indies colored, are offspring from colored parents born in the British West Indies, and United States colored are offspring from colored parents born anywhere in the United States.

In each case the probable error of the ratio obtained has been calculated for the numbers observed. On the basis of these probable errors, the differences between the various ratios can be fairly compared with respect to their probable significance.

As has before been shown, there is a significant difference between the European "pure" and the European "hybrid" and between the former and the United States white. There is, however, no significant difference between the European "hybrid" and the United States white. This shows that in the data studied, the United States white ratio is essentially that of a hybrid race.

TABLE 1

	MALES	FEMALES	RATIO
(*a*) European Pure	2807	2689	104.54 ± 0.97
(*b*) European Hybrid	677	551	122.86 ± 2.14
(*c*) United States White	994	840	118.33 ± 1.71
(*d*) British W. I. Colored	667	618	107.92 ± 2.65
(*e*) United States Colored	695	723	96.12 ± 1.76

COMPARISON	DIFFERENCE	DIFFERENCE / PROBABLE ERROR OF DIFFERENCE
European Pure × European Hybrid	18.32 ± 2.35	7.8 Significant
European Pure × U. S. White	13.79 ± 1.9	7.2 Significant
European Pure × B. W. I. Colored	3.38 ± 2.82	1.2 Not significant
European Pure × U. S. Colored	8.42 ± 2.0	4.2 Significant
European Hybrid × U. S. White	4.53 ± 2.74	1.6 Not significant
U. S. White × U. S. Colored	22.21 ± 2.45	9.0 Significant
B. W. I. Colored × U. S. Colored	11.80 ± 3.18	3.7 Significant

This behavior of the United States matings is not in the least surprising for the number of different racial stocks involved in the production of the parents of the offspring recorded is undoubtedly large.

The British West Indies colored stock, while far from pure, is obviously more nearly so than the United States colored. This is true because of the fact that the United States colored in addition to including offspring whose grandparents were born in the United States, also admits the chance that part of the grandparents may have been themselves British West Indies stock, thus adding the hereditary variations of this stock to that of the United States colored stock. It is interesting to note that the ratio of the B. W. I. stock does not depart significantly from that of the European pure whites (see table 1). On the other hand, the United States colored differs significantly from both B. W. I. colored, and from European pure and United States whites.

We may, therefore, class our matings into two main groups—the European pure and the B. W. I. colored, as being relatively "pure" and the European hybrid, United States white, and United States colored as being relatively "hybrid."

Let us now examine the nature of the ratios when the progeny of *primipara* are separated from other births.

TABLE 2

	1ST BIRTHS			SUBSEQUENT BIRTHS				
	♂♂	♀♀	Ratio	♂♂	♀♀	Ratio	Difference	Difference / Probable Error of Diff.
European Pure	1236	1070	115.51 ± 1.5	1571	1614	97.33 ± 1.18	18.18 ± 1.86	9.7
B. W. I. Colored	313	302	103.64 ± 2.75	354	316	112.02 ± 2.76	8.38 ± 3.9	2.1
European Hybrid	303	250	121.20 ± 3.14	374	301	124.25 ± 2.91	3.05 ± 4.28	0.7
U. S. White	534	446	119.73 ± 2.26	460	394	116.75 ± 2.49	2.98 ± 3.36	0.9
U. S. Colored	264	282	93.61 ± 2.44	431	441	97.73 ± 2.03	4.12 ± 3.17	1.3

Table 2 shows that the three hybrid matings show a not significant difference between the sex ratio of offspring of *primipara* and those of subsequent births. On the other hand, there is a marked significance in the case of European pure matings and a barely possibly significant difference in the B. W. I. colored. The entire excess of male births in the total sex ratio of 104.54 in the European pure matings is found in the offspring of *primipara*—where 115.51 ± 1.50 males to 100 females are found.

It is interesting to note that the negro matings in every case differ from one another in exactly the opposite direction from the white races. Thus if the direction of arrows shows the change in the *male* ratio the following diagram may be constructed: Single arrows = white, double arrows = colored.

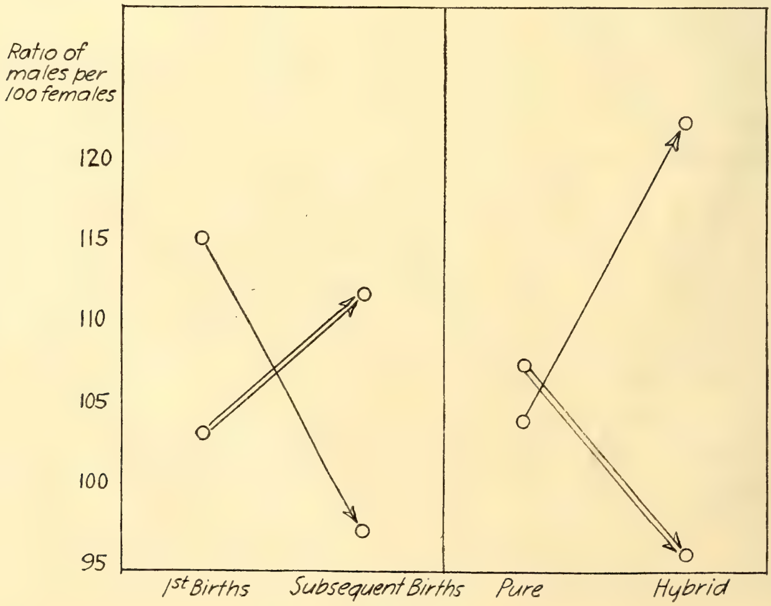


FIG. 1

The British West Indies colored race starting with a sex ratio of 103.64 ± 2.75 in the offspring of *primipara* shows in subsequent births a ratio of 112.02 ± 2.76 while in the European pure the ratio of first births is 115.51 ± 1.5 and subsequent births show a *decrease* of males; the ratio being 97.33 ± 1.18.

So also the differences between "pure" and "hybrid" races are in the opposite direction in the colored and white races. The "pure" European have a sex ratio of 104.54 ± 0.97 and the European hybrids a ratio of 122.86 ± 2.14—showing an *excess of males.* On the other hand the British West Indies colored which as we have seen is, compared with the United States colored, relatively the purer, has a ratio of 107.92 ± 2.65, while the United States colored has a ratio of 96.12 ± 1.76 an *excess of females.*

It seems, therefore, that the following conclusions as regards material in the records of the Sloane Maternity Hospital of New York City, may be drawn as a guide for further investigations.

(1) Hybrid white matings give a significant *excess of males* over "pure" white matings.

(2) Hybrid colored matings give a significant *excess of females* over relatively "pure" colored matings.

(3) The difference between the sex ratio of the United States born whites and the United States born colored is nine times its probable error.

(4) In "pure" European matings the offspring of *primipara* have a sex ratio of 115.51 ± 1.5, while the offspring from subsequent births have a ratio of 97.33 ± 1.18. The difference is 9.7 times its probable error.

(5) In the hybrid matings studied no such difference between the sex ratio of offspring of *primipara* and of subsequent births exists.

(6) The sex ratios of the United States white births recorded is not significantly different from that of *hybrid* European matings.

[1] *Proc. Soc. Exp. Biol. Med.*, 1919.

[2] *Biol. Bull.*, 1908.

THE EXISTENCE OF HOMOGENEOUS GROUPS OF LARGE IONS

By Oswald Blackwood

Ryerson Laboratory, University of Chicago

Communicated by R. A. Millikan, March 12, 1920

The Zeleny mobility apparatus has been modified by the author so as to be capable of giving a decisive answer to the question as to whether the large ions produced by spraying distilled water are of many different sizes or whether—as claimed by certain observers[1]—only a few different sizes are present in the ionization. In other words, is there a *continuous* spectrum of mobilities or a *line* spectrum? The investigation is fully described in an article which is about to appear in the *Physical Review.*

The mobility apparatus consists of a brass cylinder 160 cm. long and

5 cm. internal diameter, which is fitted with two co-axial electrodes 2 cm. in diameter and, respectively, 105 and 35 cm. long. (See fig. I.) The electrode L' is grounded, while L'' is connected to a pair of quadrants of an electrometer. Any desired potential may be established between the cylinder and the electrodes by means of a battery of dry cells.

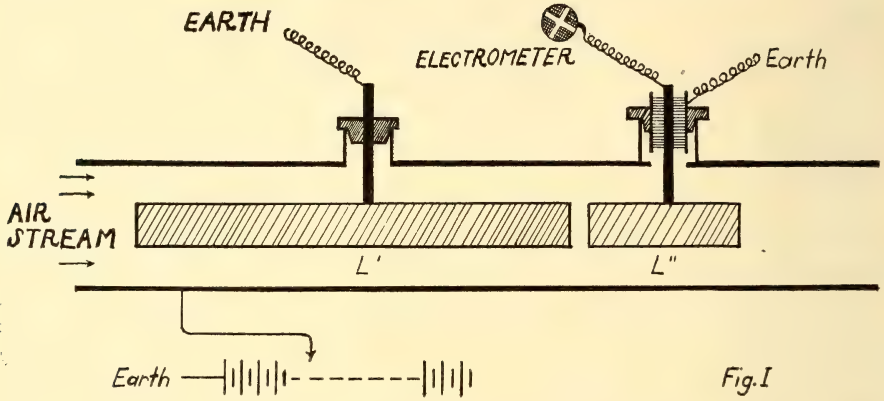


Fig. I

The ions to be tested are carried by a blast of air through the cylinder in a direction parallel to its axis. If there is no electric field, none of the ions are driven to the electrodes and the electrometer current is zero. When, however, a field is established and is increased progressively, more and more ions are driven from the air stream; the current increases and achieves its maximum value at a critical voltage V' at which all the ions are caught. As the field is still further augmented, the long, grounded electrode catches more ions, fewer reach the second electrode, and finally —at a voltage V''—the current again becomes zero. The voltages V' and V'', corresponding to the maximum and zero currents, respectively, may be computed for ions of mobility k by means of the formula:

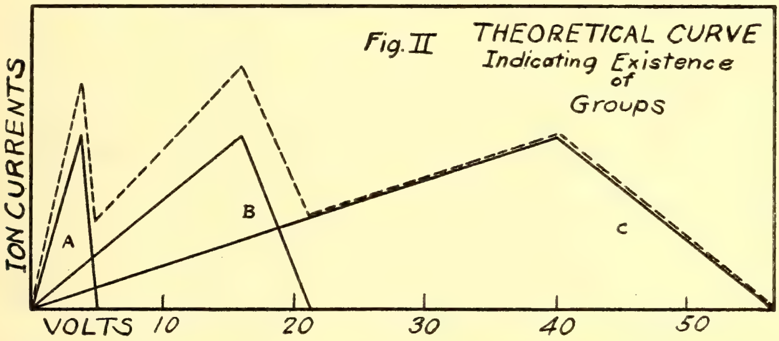
$$k = \frac{Q}{2\pi L'V''} \log_e b/a = \frac{Q}{2\pi(L' + L'')V'} \log_e b/a$$

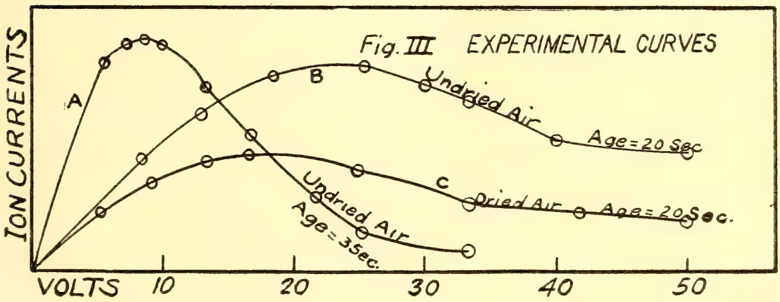
a and b being the radii of the outer and inner cylinders, Q the volume of air traversing the tube each second, L' and L'' the lengths of the long and short electrodes.

Figure I shows the forms and relative positions of the three theoretical current-voltage curves for ions of respective mobilities, 0.0043, 0.0010, and 0.0004 cm. per second in a field of one volt per cm. These are the values found for successive groups of ions by the observers mentioned above.[1] The dotted line indicates the summation curve produced when the three classes of ions are simultaneously present in equal concentration. It will be noted that this curve has three well-defined *peaks*, each corresponding to a certain class of ions.

The experimental curves which have been determined by the author for ionization from spray, and also for that from hot wires, are all of the type shown in fig. III. The fact that no curve has a plurality of peaks proves that—contrary to the findings of other workers[1]—a series of widely separated, well-defined mobility groups does not exist.

Decrease of Mobility with Time.—It has been found that if the ions are tested soon after their production the mean mobility is relatively high. If, however, metal tubes are interposed between the sprayer and the testing tube, so as to increase the period between the production of the ions and their testing, the mean mobility decreases and higher voltages are necessary to produce maximum ion currents. (Fig. III, (*A*) and (*B*).) The author, using a red hot platinum wire as a source of ionization, finds that the rate of decrease of mobility can be retarded by drying the air in which the ions are produced. (Compare curves (*B*) and (*C*), fig. III.) This indicates that the decrease of mobility is due, in part at least, to the growth of the ions by condensation of water vapor.





Nature of the Large Ion.—It has now been shown that—contrary to the findings of other observers[1]—a series of widely separated, well-defined mobility groups does not exist in the ionization from spray. Their hypothesis that the large ion is a complex cluster of stable water globules may consequently be abandoned in favor of the simpler conception of J. J. Thomson and others who have pictured the large ion as merely a charged globule of water having a radius of equilibrium which is a function of the humidity of the air.

[1] McClelland, J. A. and Nolan, P. J., *Proc. Roy. Irish Acad.*, A **33,** 1916 (29); Nolan, P. J., *Ibid.*, A **33,** 1916 (10); McClelland, J. A. and Nolan, P. J., *Ibid.*, A **35,** 1919 (1).

THE RELATION BETWEEN THE ISOELECTRIC POINT OF A GLOBULIN AND ITS SOLUBILITY AND ACID COMBINING CAPACITY IN SALT SOLUTION

By Edwin J. Cohn

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Communicated by L. J. Henderson, March 11, 1920

Globulins are, by definition, soluble in solutions of the salts of strong acids and strong bases[1] and tuberin, the protein of the potato, has accordingly been classified as a globulin.[2] In the juice that can be squeezed from the potato, tuberin is dissolved on account of the high concentration of electrolytes.[3] It is precipitated by dialysis of the juice, and readily redissolves in solutions of neutral salts. The effect of neutral salts in dissolving tuberin is, however, variable with the hydrogen ion concentration. The nature of this variation, and to some extent its causes and its meaning for the classification of proteins, is considered in this paper.

The tuberin that was employed in these experiments was precipitated by dialyzing potato juice that had been freed from starch by filtration through pulp, in a collodion bag against tap water. After a period of days the tuberin had largely precipitated and settled. The contents of the collodion bag were then evenly suspended, transferred in 100 cc. samples to flasks, and brought to different hydrogen ion concentrations by the addition of the amounts of sodium hydroxide and hydrochloric acid recorded in table I. The systems containing tuberin and acid, or tuberin and base in identical concentration were next divided, and added to vessels containing different amounts of sodium chloride. The systems, therefore, varied in two respects; in the first place in the amount of free, and, therefore, of combined protein, acid or base; in the second place in the amount of salt that they contained. After they had stood at constant temperature for about 6 hours, and it was believed that equilibrium had been established, the hydrogen ion concentrations of the systems were electrometrically determined, and the solubility of the tuberin in them was estimated by determining nitrogen in an aliquot part after the precipitated tuberin had been removed by centrifuging. In certain of the systems it will be noted that all of the protein dissolved. In these cases, therefore, the data do not represent solubility; that is solutions saturated with respect to protein.

The results obtained in this preliminary investigation were not, it is true, altogether regular. Abnormal values of nitrogen and also of hydrogen ion concentration appear in the data. None the less, certain conclusions can quite safely be drawn, the more so since they are borne out by several other experiments.

TABLE I

EFFECT OF NaCl UPON SOLUBILITY AND UPON ACID COMBINING CAPACITY OF DIALYZED TUBERIN

N/10 HCl added to 100 cc. dialysed potato protein cc.	NaCl ADDED TO 100 CC. OF DIALYSED POTATO PROTEIN. VOLUME INCREASED BY ADDED ELECTROLYTES TO 120 CC.											
	0 gm.	0.25 gm.	0.5 gm.	1.0 gm.	2.0 gm.	5.0 mg.	0 gm.	0.25 gm.	0.5 gm.	1.0 gm.	2.0 gm.	5.0 gm.
	pH						Tuberin dissolved (mg. nitrogen in 10 cc.)					
50	1.58	1.58	1.58		1.56	1.46	5.96	5.79	5.79	5.79	5.70	3.40
20	2.67						5.70	5.70	5.45	5.11	4.34	2.55
5	3.28	3.33	3.37	3.50	3.49	3.45	5.45	5.28	5.34	4.94	4.09	3.05
3	3.93	3.97	4.02	4.09	4.03	4.13	4.77	5.28	4.17	4.34	4.68	4.17
2	4.29	4.34	4.41	4.44		4.34	4.09	4.17	4.51	4.09		4.26
1	5.22	5.13	5.13				3.40	3.83	4.09	4.43	4.68	4.60
0	5.71	5.68	5.92	5.75	5.48	5.36	2.47	4.09	3.66	5.02	4.09	4.26
NaOH.												
0.5	6.40	6.23	6.02	6.28	6.10	5.84	3.58	4.51	4.85	5.19	4.77	4.94
1	6.95	6.76	6.78			6.52	4.98	4.17	4.77	5.96	5.19	5.36
3	8.99	8.77	8.71	8.77	7.95		5.87	5.02	5.21	5.70	5.87	5.87
5	9.41	9.06	9.23	9.25	9.09	8.98	5.87	5.70	5.87	5.62	5.87	5.70
10	10.39	10.09	10.03	10.20	9.72	9.83	5.96	6.04	5.79	5.70	5.87	5.79

I. THE EFFECT OF SODIUM CHLORIDE UPON THE SOLUBILITY OF TUBERIN

Solubility of Tuberin in Salt Solution.—The tuberin that had been precipitated by dialysis, in the way that has been described, must not be regarded as pure protein, but rather as a natural protein compound. It contained phosphorus and may have contained calcium. It was, however, largely redissolved by salt.

The solubility of the tuberin was increased by the addition of sodium chloride to the systems that were otherwise unchanged. It was increased more by sodium chloride in the systems which contained small amounts of sodium hydroxide. However, when a small amount of acid was added the power of sodium chloride to increase the solubility was greatly weakened. This "effect of minute quantities of acid on the solubility of a globulin in salt solutions" appears to be quite general. It was studied by Osborne and Campbell in 1897[4] and will be discussed later in this paper.

In our experiments, which were extended over a wider range of hydrogen ion concentration, the effect of sodium chloride steadily decreased as the acidity increased, until at hydrogen ion concentrations in the neighborhood of 10^{-4} *N*, the solubility of the protein was nearly independent of the salt concentration.

Solubility of Tuberin in Acid Solution.—Not only was tuberin dissolved by salt solutions, in the absence of acids, but it was also dissolved by acid in the absence of salt. In solutions of lower hydrogen ion concentration than 10^{-4} N, the dialyzed tuberin compound was only slightly soluble;* less so than in salt solutions. At greater hydrogen ion concentrations than 10^{-4} N, tuberin was largely dissolved in the systems to which no sodium chloride had been added. Its solubility in solutions of this degree of acidity was, however, decreased by sodium chloride. Indeed in the presence of sufficient salt the tuberin was nearly completely precipitated. The precipitation of many other globulins besides tuberin from acid solution by neutral salts has been observed. Osborne[5] studied the precipitation of edestin under these circumstances, and Hardy[6] that of serum globulin. This phenomenon is also apparently quite general.

Solubility of Tuberin at pH 4.3 Independent of Salt.—Between the systems of such acidity that sodium chloride decreased the solubility of tuberin and the more nearly neutral ones in which sodium chloride increased solubility, there existed a narrow range where sodium chloride had very little effect upon the solubility of tuberin. This range coincided with a slightly lower hydrogen ion concentration than 10^{-4} N.

These results are reported in table I. If represented graphically, the solubility of tuberin plotted as ordinates, and hydrogen ion concentration as abscissae, the curves representing the solubility of tuberin in the presence of different amounts of sodium chloride converge, and intersect each other within a narrow range of hydrogen ion concentrations, slightly lower than 10^{-4} N, probably nearest to 0.5×10^{-4} (pH 4.3).

The Isoelectric Point of Tuberin.—In a previous investigation the method of cataphoresis was employed in determining "the Isoelectric Points of the Proteins in Certain Vegetable Juices."[3] Potato juice was brought to different hydrogen ion concentrations and the migration of tuberin in an electric field determined. The direction of migration was found to change at a hydrogen ion concentration slightly lower than 10^{-4} N. At greater acidities tuberin migrated to the cathode, at more nearly neutral reactions to the anode. At hydrogen ion concentrations slightly lower than 10^{-4} N, tuberin migrated in neither direction. At this reaction, therefore, it was isoelectric.

The Ionization of Tuberin.—The ionization of a protein compound can be inferred from the direction of its migration during cataphoresis. In neutral potato juice and potato juice less acid than 10^{-4} N, tuberin bore a negative charge. Presumably therefore tuberin as it existed in the juice of the potato was ionized as an acid. This compound decomposed on the addition of the stronger acid HCl, and tuberin was freed at its isoelectric point. In the neighborhood of its isoelectric point tuberin was, moreover, less ionized than at greater or at lesser acidities and was also combined with fewer of the electrolytes that existed in potato juice or were formed by the addition of acid to it. For this reason the flow of current during cataphoresis was greatest at the isoelectric point (3, p. 155). At hydrogen ion concentrations greater than the isoelectric point, where tuberin migrated to the cathode, and bore, therefore, a positive charge, it was evidently ionized as base and combined with acid.

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* This requires further study.

Solubility of Acid and of Basic Tuberin Compounds.—The coincidence of the hydrogen ion concentration at which sodium chloride has least effect upon the solubility of tuberin with the isoelectric point allows of but one interpretation; sodium chloride affects tuberin differently as the migration of the protein varies. At hydrogen ion concentrations greater than 10^{-4} N, where tuberin was ionized as base and combined with acid, sodium chloride decreased its solubility. When tuberin was least ionized, at its isoelectric point, sodium chloride had least effect upon its solubility. Expecially when tuberin was ionized as acid did sodium chloride increase its solubility in the way that has led to its being classified as a globulin.

Solubility of Other Globulins.—This conception of the behavior of a globulin is not new. It was expounded by W. B. Hardy in his Croonian Lecture of 1905[6] on the basis of his studies of serum globulin.[7] Excepting in one point, which will presently be considered, his observations and conclusions agree with ours on tuberin. Neutral salts only increased the solubility of serum globulin at reactions more nearly neutral than its isoelectric point. At acidities greater than the isoelectric point, neutral salts precipitated serum globulin, as they did tuberin.

The earlier observations that have been mentioned indicate (1) that the solubility of many other globulins in salt solutions is decreased by small amounts of acid, and (2) that many globulins are precipitated from acid solution by small amounts of neutral salt. Up to the present the isoelectric points of most of these proteins have not been determined.* The way in which they are ionized is, however, suggested by these criteria. This appears from a consideration of the effect of sodium chloride upon the dissociation of the compounds that tuberin forms at greater and at lesser acidities than correspond to its isoelectric point.

II. THE EFFECT OF SODIUM CHLORIDE UPON THE ACID COMBINING CAPACITY OF TUBERIN

The chemical analysis of the effect of sodium chloride upon the ionization of the different tuberin compounds, whose solubility it differently affects, is carried one step further by the measurements of hydrogen ion concentration. The hydrogen ion concentration of a system containing protein and acid or protein and base is, as a first approximation, defined by the ratio of protein to acid, or protein to base,[9,10,11] and depends merely on the ionization constant of the acid or base and the amphoteric constants of the protein. The effect of a neutral salt upon such systems has recently been exhaustively studied by Sörensen, Hoyrup, Hempel and Palitzsch.[9] It is illustrated by the data in table I. These are in substantial agreement with those recently published by Sörensen and his collaborators. They moreover derived an equation from the mass law which enabled them to calculate the hydrogen ion concentration of any ampholyte in the presence of an acid, and the salt of the acid and a strong base, if the dissociation constants of ampholyte, acid, and salt, and the concentrations of the components of the systems were known. In this equation "the influence of the salt only appears by the fact that the values of" "the degree of dissociation of the acid" and "of the salt of the acid together with the ampholyte" "vary with the concentration of the salt." (9, pp. 93 and 101.) In other words, the effect of a neutral salt with a common ion is to depress the dissociation of the acid, and of the salt of the acid with the protein. As a result the acid combining capacity of the protein is increased, and the hydrogen ion concentration decreased. If the protein is ionized as acid on the other side of its isoelectric point and is combined with base, the depression in the dissociation of the base, and of the salt of the protein and the base, causes the hydrogen ion concentration to increase. The effect of salt in depressing the dissociation of the protein compound, however ionized, thus causes the hydrogen ion concentration of protein systems to converge upon the isoelectric point of the protein. As a result Sörensen has concluded:

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* Rona and Michaelis[8] give the isoelectric point of serum globulin as 0.36×10^{-5} and of edestin as 1.3×10^{-7}. These values depend in part, however, upon the points of maximum precipitation of the proteins.

"at hydrogen-ion concentrations which are not situated at or in the neighborhood of the isoelectric point of the ampholyte the capacity to combine with acids

"(*a*) is independent of the ampholyte concentration;

"(*b*) increases with rising concentration of salt;

"(*c*) is positive at hydrogen-ion concentrations superior to that corresponding with the isoelectric reaction of the ampholyte, and

"(*d*) is negative—that is, the ampholyte is combined with surplus base—at hydrogen-ion concentrations inferior to that corresponding with the isoelectric point of the ampholyte.

"At hydrogen-ion concentrations corresponding with or situated in the neighborhood of the isoelectric point of the ampholyte, the conditions are not easily discernible; the capacity to combine with acids is in this case not independent of the concentration of the ampholyte, but is the greater the smaller the latter is." (9, p. 161.)

Whenever complicating colloidal phenomena do not interfere, therefore, the mass law demands that a neutral salt increase the hydrogen ion concentration of systems containing protein when ionized as acid, and decrease the hydrogen ion concentration when ionized as base. Salt should, accordingly, have least effect when the hydrogen ion concentration of the system coincides with the isoelectric point of the protein. Sörensen's studies, therefore, add another property—acid combining capacity—in which the singular point in protein behavior coincides with the isoelectric point, and suggests another method which, in the absence of complicating phenomena, may be used to determine the isoelectric point.

The only apparent effects of sodium chloride upon the hydrogen ion concentration of systems containing tuberin and sodium hydroxide, or tuberin and hydrochloric acid, follow from this theory.* When sodium chloride was added to such systems the acid binding capacity was increased, and the hydrogen ion concentration decreased, if tuberin was ionized as base at acidities greater than the isoelectric point. The hydrogen ion concentration was increased at more nearly neutral reactions. The hydrogen ion concentration of the system whose acid binding capacity was least affected by the addition of sodium chloride was slightly lower than 10^{-4} N, and, therefore, in the neighborhood of the isoelectric point of tuberin.

Dissociation of Tuberin in Salt Solution.—The increased hydrogen ion concentration of tuberin systems less acid than pH 4.3 to which sodium chloride was added shows that the dissociation of the tuberin compound was depressed, and that as a result tuberin combined with surplus base (the amount of base bound by tuberin, and by depression of the dissociation of all electrolytes being measured by the increase in free acid). We have seen that under these circumstances the solubility of tuberin increased.**

Tuberin also combined with base in the systems to which sodium hydroxide was added. An increase in solubility also followed the formation of this tuberin compoound, which may have been similar to that formed by sodium chloride. Tuberin never dissolved as completely, however, in neutral solutions to which no sodium chloride was added. Apparently this compound of tuberin is more soluble when little dissociated. That its dissociation products—like those of many other globulins which appear upon dialysis or dilution of their solutions—are very insoluble, is certain.

Small amounts of hydrochloric acid decomposed the compounds that were formed by the more weakly ionized acid, tuberin, at reactions less acid than its isoelectric point. We have seen that sodium chloride and sodium hydroxide favored the formation of this tuberin compound, and, therefore, increased its solubility. By decomposing it hydrochloric acid decreased the solubility of tuberin in salt solutions.

The observations of Osborne, that have already been referred to, indicate that the decreased solubility of many other globulins in salt solutions, to which small amounts of acid had been added, resulted from their acid isoelectric points, and the decomposition of their compounds with bases. If the isoelectric points of these globulins had not been acid, but had occurred at an alkaline reaction, their acid compounds would have been decomposed by bases, and as a result they would probably have been rendered less soluble by bases rather than by acids.

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* It is immaterial whether the change of hydrogen ion concentration caused or was caused by the change in state of the protein.

** The tuberin preparation was probably not a simple sodium compound. It contained phosphorus, and may have contained calcium. The increase in solubility may, therefore, in part, have been associated with change in the protein compound.

Dissociation of Tuberin in Acid Solution.—In the presence of hydrogen ion concentrations greater than the isoelectric point, tuberin ionized as a base and dissolved as an acid compound. The effect of sodium chloride in precipitating tuberin from such solutions is perhaps less clear. Hardy studied the identical effect upon serum globulin. He believed that proteins formed addition products with neutral salts through their amino groups, and that under these circumstances neutral salts displaced acid from combination with protein. If that were so the acidity of such systems would increase. We have seen that in the case of tuberin quite the contrary occurs. Tuberin chloride binds more rather than less acid in the presence of sodium chloride. For the present, therefore, it is simpler merely to assume that tuberin chloride is little soluble when little dissociated.

Classification of Tuberin as a Globulin.—The classification of tuberin as a globulin depends, as our observations indicate, upon the solubility in salt solutions of the compounds that tuberin forms when ionized as an acid, and upon the insolubility of the products of the hydrolytic dissociation of these same compounds. It would appear from Osborne's observations upon the behavior of many other globulins toward small amounts of acid, that they too have acid isoelectric points, and are, therefore, ionized as acids in neutral salt solutions. If the isoelectric points of these proteins had been alkaline, their solubility in salt solution would have been decreased by bases rather than by acids. But whether the relations that have been described are characteristic of all globulins, or merely of tuberin, it appears from them how intimately the classification of proteins according to solubility involves their isoelectric points.

I am greatly indebted to the Director of the Harriman Research Laboratory, the Roosevelt Hospital, New York, for courteously extending to me the facilities of his laboratory after the research that I had been conducting there for the Surgeon General's Office was concluded. This enabled me to consider these more theoretical aspects of the earlier problem.

[1] Recommendations of the Committee on Protein Nomenclature, *Amer. J. Physiol.*, **21**, 1908 (27).

[2] Osborne, T. B. and Campbell, G. F., *J. Amer. Chem. Soc.*, **18**, 1896 (575).

[3] Cohn, E. J., Gross, J. and Johnson, O. C., *J. Gen. Physiol.*, **1919**, II, (145).

[4] Osborne, T. B. and Campbell, G. F., *J. Amer. Chem. Soc.*, **19**, 1897 (482–487).

[5] Osborne, T. B., *Ibid.*, **24**, (28–39).

[6] Hardy, W. B., *Proc. Roy. Soc., London* (B), **79**, 1907 (413–426).

[7] Hardy, W. B., *J. Physiol.*, **33**, 1905–6 (251–337).

[8] Rona, P. and Michaelis, L., *Biochem. Zs.*, **28**, 1910 (193–199).

[9] Sörensen, S. P. L., Hoyrup, M., Hempel, J. and Palitzsch, S., *C. R. trav. Lab. Carlsberg*, **12**, 1917.

[10] Henderson, L. J., Cohn, E. J., Cathcart, P. H., Wachman, J. D. and Fenn, W. O., *J. Gen. Physiol.*, **1**, 1918–19 (459).

[11] Loeb, J., *Ibid.*, **1**, 1918–19 (237).

THE CARBON DIOXIDE: HEAT RATIO IN CATTLE

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Communicated by F. G. Benedict, March 24, 1920

It was recently suggested to us by Dr. F. G. Benedict that if the ratio of the carbon dioxide excreted to the heat produced by cattle should prove to be sufficiently constant, this fact might be utilized as the basis for determinations of the energy metabolism of this species which would be approximately correct and which would require only a comparatively simple technique. In view of the great value which such a simplified method would have, especially for agricultural investigators, and of the further fact that a new form of respiration apparatus designed to be used in this manner has been constructed by Benedict in coöperation with the New Hampshire Agricultural Experiment Station, we have thought it worth while to study the results of our calorimetric experiments on cattle from this point of view and we present the results of that study herewith.[1]

These experiments were made with the Atwater-Rosa respiration calorimeter which has been in use here since 1901, the heat production as well as the elimination of carbon dioxide being measured, and are, so far as we are aware, the only direct determinations of the heat production of this species. The measured heat production has been compared with the carbon dioxide elimination in 188 24-hour periods.[2] The carbon dioxide included, of course, the considerable amount arising from the methane fermentation in the rumen. The following results were obtained for the heat expressed in kilogram calories divided by the carbon dioxide expressed in grams.

CARBON DIOXIDE : HEAT RATIO OF CATTLE

Range	2.1500 to 3.1500
Mean value	2.4947 ± 0.0085
Standard deviation	0.1713 ± 0.0060
Median	2.4938
Theoretical mode	2.4974
Error of single variate	±0.12
Coefficient of variability	6.87%

In 51 of these periods we have also data for the carbon dioxide and heat production of the animals in the standing and lying positions respectively. A comparison of these shows a somewhat smaller ratio in the lying than in the standing position.

CARBON DIOXIDE : HEAT RATIO OF CATTLE

	STANDING	LYING
Range	1.8600 to 2.9400	1.6900 to 2.5700
Mean value	2.5129 ± 0.0185	2.2529 ± 0.0149
Standard deviation	0.196 ± 0.013	0.158 ± 0.011
Median	2.5020	2.2713
Theoretical mode	2.4802	2.3081
Error of single variate	±0.13	±0.11
Coefficient of variability	7.80%	7.01%

A further study of the results indicated that the rather wide range in the single values of the CO_2 : heat ratio was caused in large degree by variations in the amount of feed consumed, the ratio tending to decrease as the amount of feed increased. This is what would have been expected for two reasons. First, the CO_2 : heat ratio for the methane fermentation, although not very satisfactorily determined, is undoubtedly much lower than that due to the tissue metabolism of the animal and as the feed is increased this constitutes an increasing proportion of the total elimination. Second, it is clear that the formation of fat from carbohydrates which takes place on a heavy ration would tend to lower the ratio; while, on the other hand, the oxidation of body fat on an insufficient ration would tend to increase the ratio. It should be noted also that our subjects were performing no external work and while not absolutely at rest were for the most part standing or lying quietly. Any considerable muscular exertion would doubtless have materially increased the ratio.

In 99 48-hour experiments, the factor of the varying size of the subjects being eliminated by computing all the results per kilogram of live weight, it appeared from a graph in which the abscissas represented the total weights of feed consumed and the ordinates, respectively, the grams of carbon dioxide excreted, the calories of heat measured and the ratio Heat/CO_2, that the individual results grouped themselves quite closely about three straight lines which could be represented by the three following linear equations, in which

x = Air-dry weight of feed in grams per kilogram of live weight
y_1 = Calories of measured heat per kilogram live weight
y_2 = Grams of CO_2 per kilogram live weight

$$y_3 = \frac{\text{Heat}}{CO_2} = \frac{y_1}{y_2}$$

(1) $y_1 = 0.869x + 14.176$
(2) $y_2 = 0.455x + 4.365$
(3) $y_3 = -0.0226x + 2.802.$

A computation of the closeness of fit of the lines represented by the foregoing equations to the individual results gives the following values for the index (Δ) and the probability (P).

$$\Delta_1 = 4.489 \qquad P_1 = 0.9508$$
$$\Delta_2 = 2.467 \qquad P_2 = 0.9640$$
$$\Delta_3 = 0.663 \qquad P_3 = 0.9986$$

It would perhaps have been preferable to make the comparisons with the dry matter of the feed consumed. No succulent feeds, however, were contained in the rations and it does not seem probable that the slight differences in moisture content can have materially affected the conclusions. We have not attempted to distinguish between the effects of different kinds or classes of feeding stuffs and doubt whether our data are sufficiently extensive to warrant it.

It would appear from the foregoing data that within the range of these experiments, i. e., with feed varying from 5 to 27 grams per kilogram live weight, equation (3) may be used for computing the CO_2 : heat ratio of cattle when their live weight and the amount of feed consumed are known, and that by the use of this ratio the heat production may be computed from the observed CO_2 production with a good degree of accuracy, at least for animals on dry feed and not performing work.

It should be noted, however, that the values of y_3 computed from equation (3) differ slightly from the ratios computed from the corresponding values of y_1 and y_2, indicating that y_3 is not a strictly linear function of x. The discrepancy does not appear sufficient to affect materially the computation of the heat production from the CO_2 on the amounts of feed used in these experiments but for the fasting animal, i. e., when $x = 0$, the discrepancy is much more considerable, viz.:

$$\frac{y_1}{y_2} = 3.25 \qquad y_3 = 2.80$$

It appears probable that the values of y_3 would be represented more exactly by a line slightly convex toward the horizontal axis and curving somewhat more sharply upward as the value of x approaches zero, and that for very small amounts of feed the CO_2 : heat ratio computed from the values of y_1 and y_2 is more accurate than that computed from equation (3). This conclusion is strengthened by the fact that the fasting value thus obtained approximates quite closely that observed by Benedict in his experiments on fasting men.

[1] The entire credit for the mathematical discussion of the results as well as for the laborious computations involved is due to Mr. Braman.

[2] In most instances two of these periods constituted the two halves of one of the 48-hour experiments mentioned subsequently.

ON KUMMER'S MEMOIR OF 1857 CONCERNING FERMAT'S LAST THEOREM

By H. S. Vandiver

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Communicated by L. E. Dickson, March 6, 1920

The article referred to in the title appeared in the *Mathematische Abhandlungen* of the Berlin Academy for the year 1857, pp. 41–74. In the present note it will be pointed out that the proofs of the results given by Kummer are inaccurate and incomplete in several respects. The discussion will be confined exclusively to the indication of those parts of the argument which are deficient or incorrect, and in no case will methods for correcting the proofs or supplying the missing analysis be considered here.

In the first place Kummer divides his work, as usual, into the discussion of the two cases arising from

$$x^\lambda + y^\lambda + z^\lambda = 0 \tag{1}$$

where x, y and z are rational integers and λ is an odd prime. The *first case* arises in the consideration of (1) when xyz is prime to λ, and the *second case* when xyz is divisible by λ.

For the first case he shows that if (1) is satisfied then $B_{(\lambda-3)/2}$ and $B_{(\lambda-5)/2}$ are each divisible by λ, the B's being Bernoullian numbers,

$$B_1 = {}^1/_6,\ B_2 = {}^1/_{30}, \text{ etc.}$$

The proof is given on pages 61–65 of the article in question. No criticisms will be made here of this part of the work.

For the second case Kummer concludes that the relation (1) is impossible under the three assumptions:

1. *The first factor of the class number H is divisible by λ but not by λ^2*
2. *If $B_\nu \equiv 0 (\text{mod } \lambda)$, $\nu < {}^1/_2(\lambda - 1)_2$, there exists an ideal in the field $\Omega(\alpha)$, $\alpha^\lambda = 1$, $\alpha \pm 1$, with respect to which as a modulus the unit*

$$E_\nu(\alpha) = \prod_{k=0}^{\mu-1} e(\alpha^{\gamma^k})^{\gamma^{-2k\nu}}$$

is not congruent to the λth power of an integer in $\Omega(\alpha)$.

3. *The Bernoullian number $B_{\nu\lambda}$ is not divisible by λ^3.*

In the above assumptions $H = h_1, h_2$, where

$$h_1 = \frac{P}{(2\lambda)^{\mu-1}},\ h_2 = \frac{D}{\Delta},\ \mu = \frac{\lambda - 1}{2},$$

and P, D and Δ are defined as follows:

Let β be a primitive root of $\beta^{\lambda-1} = 1$ and λ a primitive root of λ. Then

$$P = \prod_{j=1}^{\mu} \phi(\beta^{2j-1}),\ \phi(\beta) = 1 + \gamma_1 \beta + \gamma_2\beta^2 + \ldots . + \gamma_{\lambda-2}\beta^{\lambda-2}$$

where γ_i is the least positive residue of γ^i modulo λ Also

$$e(\alpha) = \sqrt{\frac{(1-\alpha^\gamma)(1-\alpha^{-\gamma})}{(1-\alpha)(1-\alpha^{-1})}},$$

and if lx denotes the real value of log x, then

$$D = \begin{vmatrix} le(\alpha), & le(\alpha^{\gamma}) \ldots\ldots\ldots\ldots & le(\alpha^{\gamma^{\mu-2}}) \\ le(\alpha^{\gamma}), & le(\alpha^{\gamma^2}) \ldots\ldots\ldots\ldots & le(\alpha^{\gamma^{\mu-1}}) \\ & \ldots\ldots\ldots\ldots & \\ & \ldots\ldots\ldots\ldots & \\ le(\alpha^{\gamma^{\mu-2}}), & le(\alpha^{\gamma^{\mu-1}}) \ldots\ldots & le(\alpha^{\gamma^{2\mu-4}}) \end{vmatrix}.$$

If $\epsilon_1(\alpha)$, $\epsilon_2(\alpha), \ldots\ldots \epsilon_{\mu-1}(\alpha)$ are fundamental units in $\Omega(\alpha)$, that is, units which have the property that all units in the field can be expressed as products of powers of them multiplied by $\pm\alpha^m$, then

$$\Delta = \begin{vmatrix} l\epsilon_1(\alpha) \ldots\ldots\ldots\ldots & l\epsilon_{\mu-1}(\alpha) \\ \ldots\ldots\ldots\ldots & \\ \ldots\ldots\ldots\ldots & \\ l\epsilon_1(\alpha^{\gamma^{\mu-2}}) \ldots\ldots & l\epsilon_{\mu-1}(\alpha\gamma_{\mu-2}) \end{vmatrix}.$$

To establish the above result Kummer essays to prove four direct theorems in the theory of the field $\Omega(\alpha)$. These theorems are set forth later in this note and numbered I to IV. As will be shown, the proofs of theorems I and IV are incomplete as several unproved statements are made and the proofs of II and III are inaccurate, since use is made of a formula which is not true in general.

In *Crelle*, **44,** 1852 (134), Kummer gave the relation

$$(\lambda - 1)l\left(\frac{(\varphi(\alpha)}{\varphi(1)}\right) \equiv l\left(\frac{N\varphi(\alpha)}{[\varphi(1)]^{\lambda-1}}\right) + \sum_{i=0}^{\lambda-2} \frac{d_o^{i\lambda^n} l\varphi(e^v)}{dv^{i\lambda^n}} X_i(\alpha) \qquad (2)$$

modulo λ^{n+1}. In this formula $\varphi(\alpha)$ is an algebraic integer in the field $\Omega(\alpha)$, and the symbol

$$l\left(\frac{\varphi(\alpha)}{\varphi(1)}\right)$$

modulo λ^{n+1}, represents that part of the logarithmic series

$$\frac{\varphi(\alpha) - \varphi(1)}{\varphi(1)} - \frac{1}{2}\left(\frac{\varphi(\alpha) - \varphi(1)}{\varphi(1)}\right)^2 + \frac{1}{3}\left(\frac{(\varphi(\alpha) - \varphi(1)}{\varphi(1)}\right)^3 \ldots\ldots\ldots$$

none of whose terms are divisible by λ^{n+1}. The symbol $N\varphi(\alpha)$ stands for the norm of $\varphi(\alpha)$ and

$$\frac{d_o^s l\varphi(e^v)}{dv^s}$$

indicates that log $\varphi(e^v)$ is to be differentiated s times and zero substituted for v in the result. Further,

$$X_i(\alpha) = \alpha + \gamma^{-i\lambda^n}\alpha^{\lambda} + \gamma^{2i\lambda^n}\alpha^{\gamma^2} + \ldots . + \gamma^{-(\lambda-2)i\lambda^n}\alpha^{\gamma^{\lambda-2}}.$$

F. Mertens (*Wiener Berichte*, **126,** 1917, Abt. II*a*, pp. 1337–43) points out that the relation (2) is not true in all cases and shows that an error occurs in Kummer's argument on page 133 of the latter's paper.

We shall now examine Kummer's 1857 memoir in detail and in the light of what has just been mentioned.

Under assumption 1, given above, namely, that h_1 is isdivible by λ but not by λ^2, Kummer proceeds to show that only one of the numbers B_i, $i = 1, 2, \ldots\ldots, \mu - 1$, is divisible by λ. In this connection he states (p. 42, 5th line from bottom) "....denn der erste Faktor der Klassenanzahl muss, wie aus meiner Untersuchung der Teilbarkeit der Klassenanzahl durch λ (*Liouville's Journal*, **16,** 1851 (473)) unmittelbar folgt, den Faktor λ mindestens so viel mal enthalten, als wie viele der $(\lambda - 3)/_2$ Bernoullischen Zahlen durch λ theilbar sind." On examining the paper referred to by Kummer we find that he reduces h_1 modulo λ and obtains a residue which may be expressed in the form

$$m\prod_{i=1}^{\mu-1} B_1$$

where m is an integer prime to λ. At no place in this work however, does he refer to any reduction of h_1, modulo λ^n, $n > 1$. Hence, he is not warranted in making, without additional argument, the statement given above.

Having concluded that only one of the first $\mu - 1$ Bernoullian numbers is divisible by λ, Kummer calls this one B_ν and derives (p. 44), under assumption 1, the following:

Theorem I. *If* $B_\nu \equiv 0 \pmod{\lambda}$, *and* $h_2 \equiv 0 \pmod{\lambda}$ *then* $E_\nu(\alpha)$ *is the* λ^n *power of a unit in* $\Omega(\alpha)$.

On page 45 an attempt is made to prove, under assumption 1, the

Theorem II. *If* $B_\nu \equiv 0 \pmod{\lambda}$ *but* $B_{\nu\lambda} \equiv 0 \pmod{\lambda^3}$ *then a unit in* $\Omega(\alpha)$ *which is congruent modulo* λ^2 *to a rational integer is the* λ_{th} *power of a unit in* $\Omega(\alpha)$.

In the argument covering this (p. 46), there is used the incorrect formula (2) of this paper. Also, on page 50, under assumption I, is given the

Theorem III. *If* $F(\alpha)$ *is an integer in the field* $\Omega(\alpha + \alpha^{-1})$ *and if*

$$\frac{d_o^{\,2\nu} lF(e^v)}{dv^{2\nu}} \equiv 0 \pmod{\lambda}$$

then a unit $E(\alpha)$ *in* $\Omega(\alpha)$ *can be found such that*

$$E(\alpha)F(\alpha) \equiv a \pmod{\lambda}$$

where a is a rational integer.

In proving the above, the incorrect relation (2) is again used.

In paragraphs 4, 5 and 6, pp. 50–61, Kummer goes through a course of reasoning which leads him to

Theorem IV. *If P is an ideal such that* P^λ *is the principal ideal* $(F(\alpha))$, *and* $B_\nu \equiv 0$ (mod. λ) *then P is or is not a principal ideal according as*

$$\frac{d_o^{\,\lambda - 2\nu} lF(e^v)}{dv^{\lambda - 2\nu}} \equiv \text{ or } 0 \;\substack{+\\+\\+}\; \pmod{\lambda}.$$

In this connection note the formula (A) which he gives on page 53. The number $\Psi_r(\alpha)$ which appears here is defined as the product of certain ideal factors (11th line from bottom); but this decomposition holds for the case where $\Psi_r(\alpha)$ contains only ideals of the first degree. Kummer has himself given (*Crelle*, 44, p. 120) the decomposition for the generalized function $\Psi_r(\alpha)$ which may contain ideals of other degrees than the first and said decomposition is of a more general form than that which applies for ideals of the first degree. It would follow from these facts that in theorem IV, $F(\alpha)$ would necessarily contain factors of the first degree only. However, on page 67 of the 1857 memoir the theorem is applied to certain ideals $\Theta_r(\alpha)$ which are not proved to be of the first degree.

Also, on page 54 it is stated without proof or reference that if m is not divisible by λ-1 then

$$\frac{d_o^{m\lambda^r} l\phi(e^v)}{dv^{m\lambda^r}} \equiv \frac{d_o^{m\lambda^r} l\phi_1(e^v)}{dv^{m\lambda^r}} \pmod{\lambda^{r+1}}$$

where $\phi(\alpha)$ and $\phi_1(\alpha)$ are integers in $\Omega(\alpha)$ such that $\phi(\alpha) \equiv \phi_1(\alpha) \pmod{\lambda^{r+1}}$. The writer has not been able to find a proof of this any where in Kummer's papers.

AN IMPROVED FORM OF HIGH TENSION D. C. APPARATUS

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Communicated by E. B. Wilson, March 6, 1920

In a recent paper on X-rays in these PROCEEDINGS,[1] I described a high tension D. C. outfit based on that of A. W. Hull, with certain changes of details. On further investigation it has appeared that one of these changes introduces a disturbing factor, which was not large enough to affect the results in that paper, or to be detected by the tests for fluctuation used at that time, but which might have been detectable if the choke coils in the apparatus had not had extremely high inductances. As I have received several inquiries lately about the best way to build such an outfit and the question is of importance to X-ray research workers, I am writing this note to call attention to the existence of this disturbing factor and the way to eliminate it in outfits with inductances and capacities not so large as those used here.

In the arrangement described in the previous paper the principal A. C. components on the D. C. line had a frequency of 1000 cycles per second, twice that of the primary current, and the currents in both sides of the line increased and decreased in the same phase. The disturbing factor was the capacity of the transformer secondary, relative to the ground, that introduced components of current at 500 cycles, with opposite phases on the two sides of the line. Because of this phase relation, the 500 cycle components are difficult to choke off unless the middle points of the D. C. condenser lines are grounded, in which case the phase relation of each type of current components is unchanged, but the 500 cycle voltage fluctuations on the first D. C. condenser are increased, and its 1000 cycle voltage fluctuations are reduced. To make this change we must remove the ground connection that was on the point A, figure 1, and ground the middle points of all the D. C. condensers as indicated by the grounds, G, and incidentally, we may as well ground the cores of the choke coils also. Since the 500 cycle components have a phase relation opposite to that of the 1000 cycle ones it is well also to reverse the connections on the choke coils on one side of the line. This gives us the arrangement shown in figure 1.

To compute the potential fluctuations, let $I \sin \omega t$ be the current in the lead to either end of C_1, where $\omega = 2\pi \times 500 \text{ sec.}^{-1}$, and we neglect harmonics. This gives potentials as indicated in the diagram, where $(V + V_1)$ is the peak voltage of the transformer,

$$V_1 = \frac{I}{2C_1\omega}, \qquad V_2 = \frac{V_1}{C_2L_1\omega^2} = \frac{I}{2C_1C_2L_1\omega^3}$$

and

$$V_3 = \frac{V_2(1 - C_2M\omega^2)}{C_3L_2\omega^2} = \frac{I(1 - C_2M\omega^2)}{2C_1C_2L_1L_2\omega^5}.$$

Here C_1, C_2 and C_3 are the capacities of the whole series condenser lines, and $L_1 = 2L'_1 + 2M_1$ $L_2 = 2L'_2 + 2M_2$, and M is the mutual inductance between L_1 and L_2. Because of the ω^5 in the denominator of V_3 it is evident that harmonics need not be considered, although for the 1000 cycle harmonic we must replace L_1 and L_2 by $2L'_1 - 2M_1$ and $2L'_2 - 2M_2$, respectively. To find I for a given value of the direct current I' is not easy, and the ratio of the two is not constant. But if the rise of voltage on either half of the first condenser occurred instantaneously it would not be greater than $(2\pi/\omega)I' \div 2C_1$; and, if it took a whole half cycle it would not be less than half this amount. In practice, with heavy loads the rise appears to take a large fraction of a half cycle. The slower the rise is, the less of it we may ascribe to harmonics, and thus for an approximate value of I we may assume the rise to be $2V_1$, and also to be somewhat greater than $\pi I'/2C_1\omega$. Thus I should be about $2I'$ under such conditions, and measurements confirm this relation.

This arrangement has the disadvantage of depending on a 500 cycle charging current in C_1 instead of 1000. On the other hand, if the two sides of the line are symmetrical, there should be no 500 cycle fluctuation in the total potential difference between the ends of the line. In fact with the previous type of connection we might expect that if the ground wire was removed we should have likewise no 500 cycle fluctuation in the potential difference between the ends of the line, although both ends would fluctuate relative to the ground. But this conclusion is based on the assumption of perfect symmetry between the two ends of the line, which is liable not to occur. And although the 500 cycle fluctuation on the first

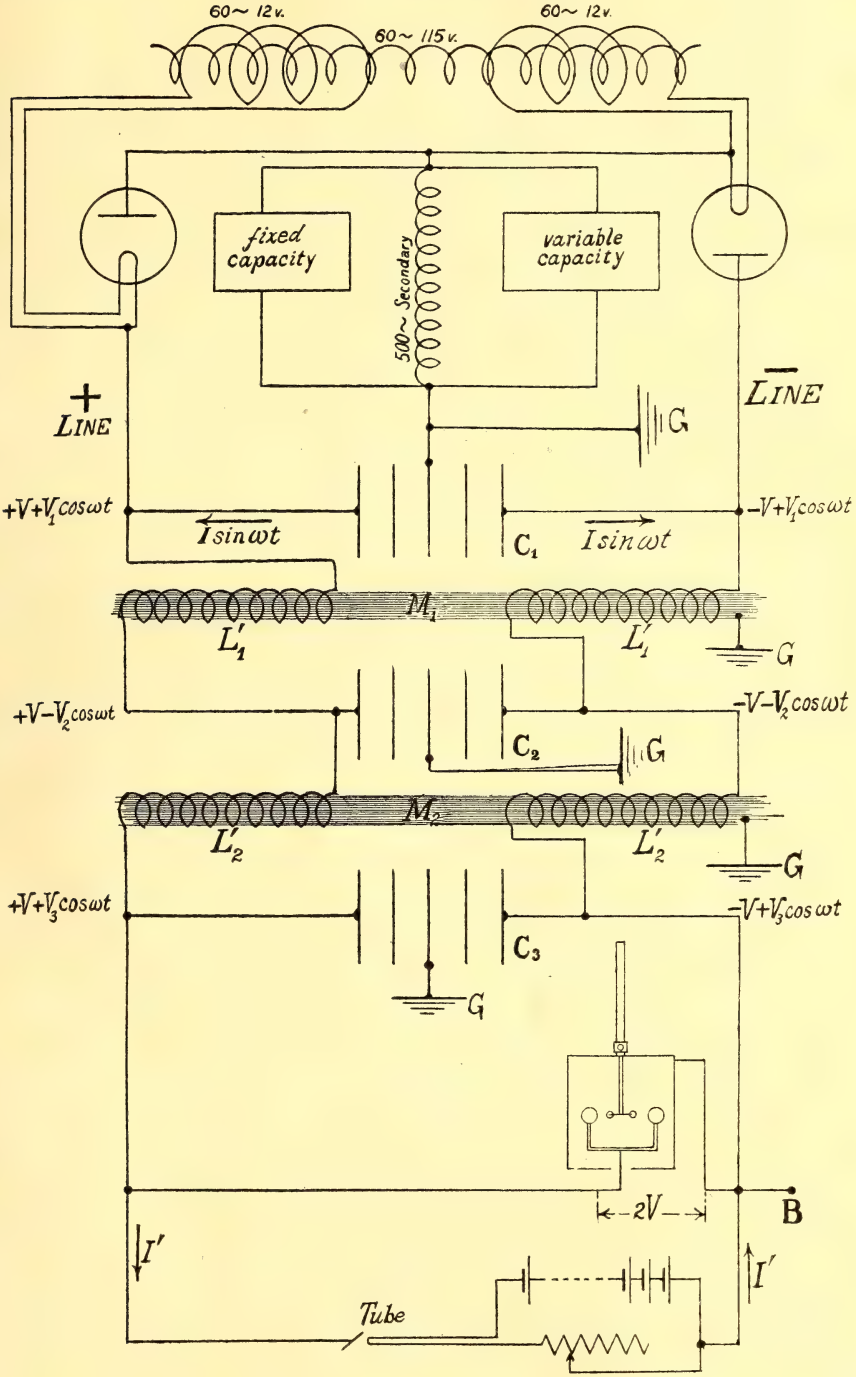
60~ 12v.
60~ 115 v.
60~ 12v.
fixed capacity
500~ Secondary
variable capacity
+ LINE
$\overline{\text{LINE}}$
G
$+V+V_1\cos\omega t$
$I\sin\omega t$
C_1
$I\sin\omega t$
$-V+V_1\cos\omega t$
M_1
L_1'
L_1'
G
$+V-V_2\cos\omega t$
$-V-V_2\cos\omega t$
C_2
G
M_2
L_2'
L_2'
G
$+V+V_3\cos\omega t$
$-V+V_3\cos\omega t$
C_3
G
2V
B
I'
I'
Tube

FIG. 1

condenser is greater in the present arrangement than it would be in the other with no ground wire, that on the last condenser is much less. Hence the present arrangement is the better of the two. The advantage of having the voltage on C_1 almost twice the peak voltage of the transformer is preserved here, because we preserve the feature that is essential for this result, namely, that one end of the transformer is connected to the middle point of the first D. C. condenser line. The resonance condensers, described in the previous article, for eliminating errors due to small changes of the generator speed, can be used as they were there. In cases where the steadiest voltage is not needed and the voltage is not too high, we may cut off the filament current from one kenotron and run the tube between the other side of the line and the ground. This is useful if we need to ground one end of the tube.

For experimental tests of this apparatus I have inserted a current-type voltmeter in each of the ground lines from the middle points of the condensers, and also have used telephones in these lines. The voltmeters not sensitive enough to show the current except in C_1, and the telephones are not good for quantitative work expecially in the presence of stray 500 cycle noises, but the currents and, therefore, the voltage fluctuations are apparently of the right order of magnitude. A point illustrated by these telephone experiments is that if the voltage is raised until brush discharges occur on the line, they produce irregular fluctuations of voltage that can be clearly heard in the telephone. Anyone building an outfit of this type should take all precautions needed to avoid brush discharges and should listen for them with a telephone before relying on the steadiness of the potential.

[1] Webster, D. L., these PROCEEDINGS, 6, 1920, 26-35.

THE EFFECT OF THE CONCENTRATION OF NITRATES ON THE REDUCING POWERS OF BACTERIA[1]

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Communicated by W. M. Wheeler, April 12, 1920

During the course of some investigations on bacteria pathogenic to insects, I frequently used the nitrate-nitrite reduction test as one of a large number of differential characters. Sometimes the tests for nitrites were positive; at other times negative with the same species of microörganism. This led me to believe that the concentration of the salt in the medium was important; in other words, that very definite amounts of salt were necessary in order that the physiological activity of reduction might manifest itself.

As shown in the accompanying table a number of species of microorganisms were selected. Witte's peptone media, containing various molecular concentrations of $NaNO_3$ and KNO_3, were prepared. These media were inoculated with equal amounts of 48-hour cultures of the bac-

TABLE SHOWING RESULTS OF THE NITRATE-NITRITE REDUCTION TESTS

Code: A plus sign indicates reduction of nitrates to nitrites
A cipher indicates the lack of such reduction
WG indicates a weak growth. NG indicates no growth

SPECIES OF ORGANISM		.0002 M	.0005 M	.0008 M	.001 M	.01 M	.1 M	.5 M	1 M	2 M	4 M
Spirillum metchnikovi								W.G.	W.G.	N.G.	N.G.
	$NaNO_3$	0	+	+	+	+	+	0	0	0	0
	KNO_3	0	+	+	+	+	+	0	0	0	0
								W.G.	W.G.	N.G.	N.G.
Bacillus prodigiosus										N.G.	N.G.
	$NaNO_3$	0	0	+	+	+	+	+	+	0	0
	KNO_3	0	0	+	+	+	+	+	+	0	0
										N.G.	N.G.
Bacillus coli communis								W.G.	W.G.	W.G.	N.G.
	$NaNO_3$	0	0	0	0	+	+	+	0	0	0
	KNO_3	0	0	0	0	+	+	+	0	0	0
									W.G.	W.G.	N.G.
Coccobacillus acridiorum. Variety "Souche Sidit"										W.G.	N.G.
	$NaNO_3$	0	0	0	0	+	+	+	+	0	0
	KNO_3	0	0	0	0	+	+	+	+	0	0
										W.G.	N.G.
Coccobacillus acridiorum. Variety "Souche Cham"									W.G.	W.G.	W.G.
	$NaNO_3$	0	0	0	0	+	+	+	0	0	0
	KNO_3	0	0	0	0	+	+	+	0	0	0
									W.G.	W.G.	N.G.
Bacillus anthracis									W.G.	W.G.	W.G.
	$NaNO_3$	0	0	0	0	0	0	0	0	0	0
	KNO_3	0	0	0	0	0	0	0	0	0	0
								W.G.	W.G.	W.G.	N.G.
Staphylococcus pyogenes albus										W.G.	N.G.
	$NaNO_3$	0	0	0	0	0	0	0	0	0	0
	KNO_3	0	0	0	0	0	0	0	0	0	0
										W.G.	N.G.
Streptococcus disparis		W.G.	W.G.	W.G.	W.G.	W.G.	W.G.	W.G.	N.G.	N.G.	N.G.
	$NaNO_3$	0	0	0	0	0	0	0	0	0	0
	KNO_3	0	0	0	0	0	0	0	0	0	0
		W.G.	W.G.	W.G.	W.G.	W.G.	W.G.	W.G.	W.G.	N.G.	N.G.
Checks (No bacteria)	$NaNO_3$	0	0	0	0	0	0	0	0	0	0
	KNO_3	0	0	0	0	0	0	0	0	0	0

teria concerned and incubated for five days. After this time the cultures were tested for the presence of nitrites by the use of the sulphuric acid, potassium iodide, starch reaction and the naphthylamine, sulphanilic acid, acetic acid test. Of course, the absence of nitrites may not indicate non-reduction of nitrates, since the nitrites formed prior to the test may have been reduced to free nitrogen or ammonia. Nevertheless, in that case a nitrate reaction should not be obtained. The presence or absence of nitrates after five days growth was tested in each case by evaporating the culture to dryness and by adding phenolsulphonic acid, diluting with water and transferring to a Nessler jar. Sodium hydroxide solution was added to make it alkaline and the color reactions compared.

The table shows that each species capable of reduction has a distinct range. *Bacillus prodigiosus* has a greater range than *Spirillum Metchnikovi*, but the latter has the power of reducing nitrates in a weaker concentration. *Prodigiosus*, on the other hand, has the power of reduction in greater concentrations of the salts. This inability of *Metchnikovi* to reduce in greater concentrations may be associated with the inhibiting influence of high osmotic pressure on growth, for it will be seen that *Metchnikovi* grew weakly in concentrations greater than 0.1 mol. That growth and ability to reduce are not always associated, however, can be seen in the case of *B. coli communis* which grew poorly in 0.5 mol. concentration and reduced $NaNO_3$.

A distinct range of reduction power in various concentrations can also be noted for some of the other species. Some have a smaller, some a greater range. In so far as the reducing power is concerned no difference was noted between sodium and potassium ions. Nevertheless, a slight difference between sodium and potassium ions in the amount of growth was observed. In 4 mol. $NaNO_3$ *Coccobacillus acridiorum* variety "Souche Cham" grew poorly, but no growth whatever was obtained in the same concentration of KNO_3. The same condition was observed when the Anthrax bacillus was used. This difference in ions was further observed by using *B. coli communis*, where the sodium salt gave a poor growth at 0.5 mol. concentration and the potassium salt a good growth at the same concentration. The difference between ions and growth is not dependent upon variations in the number of bacteria originally introduced into the cultures for the amounts were previously carefully calibrated.

[1] Contribution from the U. S. Bureau of Entomology in coöperation with the Bussey Institution of Harvard University. (Bussey Institution, No. 173.)

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CONTENTS

Page

GENETICS.—CALCULATING ANCESTRAL INFLUENCE IN MAN.................... *By Harry H. Laughlin* 235

PHYSIOLOGY.—THE EFFECT OF ANESTHETICS ON LIVING CELLS................ *By Grace Medes and J. F. McClendon* 243

GEOLOGY.—A GENERAL SINKING OF SEA-LEVEL IN RECENT TIME............ *By Reginald A. Daly* 246

GENETICS.—A NOTE ON THE HUMAN SEX RATIO..............*By C. C. Little* 250

PHYSICS.—THE EXISTENCE OF HOMOGENEOUS GROUPS OF LARGE IONS......... *By O. Blackwood* 253

CHEMISTRY.—THE RELATION BETWEEN THE ISO-ELECTRIC POINT OF A GLOBULIN AND ITS SOLUBILITY AND ACID COMBINING CAPACITY IN SALT SOLUTION...... *By E. J. Cohn* 256

PHYSIOLOGY.—THE CARBON DIOXIDE: HEAT RATIO IN CATTLE................ *By H. P. Armsby, J. August Fries, and Winfred W. Braman* 263

MATHEMATICS.—ON KUMMER'S MEMOIR OF 1857 CONCERNING FERMAT'S LAST THEOREM..*By H. S. Vandiver* 266

PHYSICS.—AN IMPROVED FORM OF HIGH TENSION D. C. APPARATUS............ *By D. L. Webster* 269

ZOÖLOGY.—THE EFFECT OF THE CONCENTRATION OF NITRATES ON THE REDUCING POWERS OF BACTERIA.................................. *By R. W. Glaser* 272

VOLUME 6 JUNE, 1920 NUMBER 6

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ON THE RATE OF GROWTH OF THE POPULATION OF THE UNITED STATES SINCE 1790 AND ITS MATHEMATICAL REPRESENTATION[1]

By Raymond Pearl and Lowell J. Reed

Department of Biometry and Vital Statistics, Johns Hopkins University

Read before the Academy, April 26, 1920

It is obviously possible in any country or community of reasonable size to determine an empirical equation, by ordinary methods of curve fitting, which will describe the normal rate of population growth. Such a determination will not necessarily give any inkling whatever as to the underlying organic laws of population growth in a particular community. It will simply give a rather exact empirical statement of the nature of the changes which have occurred in the past. No process of empirically graduating raw data with a curve can in and of itself demonstrate the fundamental law which causes the occurring change.[2] In spite of the fact that such mathematical expressions of population growth are purely empirical, they have a distinct and considerable usefulness. This usefulness arises out of the fact that actual counts of population by census methods are made at only relatively infrequent intervals, usually 10 years and practically never oftener than 5 years. For many statistical purposes, it is necessary to have as accurate an estimate as possible of the population in inter-censal years. This applies not only to the years following that on which the last census was taken, but also to the inter-censal years lying between prior censuses. For purposes of practical statistics it is highly important to have these inter-censal estimates of population as accurate as possible, particularly for the use of the vital statistician, who must have these figures for the calculation of annual death rates, birth rates, and the like.

The usual method followed by census offices in determining the population in inter-censal years is of one or the other of two sorts, namely, by arithmetic progression or geometric progression. These methods assume that for any given short period of time the population is increasing either in arithmetic or geometric ratio. Neither of these assumptions is ever absolutely accurate even for short intervals of time, and both are grossly inaccurate for the United States, at least, for any considerable period of time. What actually happens is that following any census estimates are made by one or another of these methods of the population for each year up to the next census, on the basis of data given by the last two censuses only. When that next census has been made, the previous estimates of the inter-censal years are corrected and adjusted on the basis of the facts brought out at that census period.

Obviously the best general method of estimating population in inter-censal years is that of fitting an appropriate curve to *all* the available data, and extrapolating for years beyond the last census, and reading off from the curve values for inter-censal years falling between earlier censuses. The methods of arithmetic or geometric progression use only two census counts at the most. Fitting a curve to all the known data regarding population by the method of least squares must obviously give a much sounder and more accurate result. In making this statement, one realizes perfectly, of course, the dangers of extrapolation. These dangers have been well emphasized by Perrin,[3] who used higher order parabolas to predict the future population of Buenos Aires. In keeping sharply before our minds the dangers of extrapolation from a curve, we are apt to forget that the methods of extrapolation by arithmetic or geometric progression have much less general validity than from a curve, and the inaccuracies are found in practice, except by the rarest of accidents, to be actually greater.

The first one to attempt an adequate mathematical representation of the normal rate of growth of the population of the United States was Pritchett.[4] Taking the census data from 1790 to 1880, inclusive, Pritchett fitted by the method of least squares the following equation:

$$P = A + Bt + Ct^2 + Dt^3 \qquad \text{(i)}$$

where P represents the population and t the time from some assumed epoch. As a matter of fact, Pritchett took the origin of the curve at 1840, practically the center of the series. With this third-order parabola Pritchett got a very accurate representation of the population between the dates covered. As will presently appear this curve did not give, even within the period covered, as accurate results as a more adequate curve would have done, and it overestimated the population after a very short interval beyond the last observed ordinate as is shown in table 2.

Some 13 years ago one of the writers[5] demonstrated the applicability of a logarithmic curve of the form

$$y = a + bx + cx^2 + d \log x \qquad \text{(ii)}$$

to the representation of growth changes, using the aquatic plant *Ceratophyllum* as material. Following the application of this curve to growth of this plant it was found equally useful in representing a wide range of other growth and related changes.[6] This list now includes, of matters worked out in the Biological Laboratory of the Maine Experiment Station, such diverse phenomena as change of size of egg with successive layings, change of milk production with age, etc. Donaldson and Hatai[7] have demonstrated the applicability of this type of equation to bodily growth in the white rat and frog.

While the increase in size of a population cannot on *a priori* grounds be regarded, except by rather loose analogy, as the same thing as the growth of an organism in size, nevertheless it is essentially a growth phenomenon. It, therefore, seems entirely reasonable that this type of curve should give a more adequate representation of population increase than a simple third-order parabola. The actual event justifies this assumption, as will presently appear.

Table 1 shows the counted population as determined by the Census Bureau on the dates mentioned from 1790 to 1910. The exact dates were furnished in a personal communication from the present Director of the Census. These figures embody some adjustments and corrections made by the Census Bureau since the original censuses were made.

TABLE 1

SHOWING THE DATES OF THE TAKING OF THE CENSUS AND THE RECORDED POPULATIONS FROM 1790 TO 1910

DATE OF CENSUS		RECORDED POPULATION (REVISED FIGURES FROM STATISTICAL ABST., 1918)
Year	Month and Day	
1790	First Monday in August	3,929,214
1800	First Monday in August	5,308,483
1810	First Monday in August	7,239,881
1820	First Monday in August	9,638,453
1830	June 1	12,866,020
1840	June 1	17,069,453
1850	June 1	23,191,876
1860	June 1	31,443,321
1870	June 1	38,558,371
1880	June 1	50,155,783
1890	June 1	62,947,714
1900	June 1	75,994,575
1910	April 15	91,972,266

To the data of table 1 the following equation was fitted by the method of least squares, taking origin at 1780, and making due allowance in the abscissal intervals for the actual dates of the several censuses:

$$y = a + bx + cx^2 + d \log x$$

where y denotes population and x time. The actual equation deduced was

$$y = 9{,}064{,}900 - 6{,}281{,}430x + 842{,}377x^2 + 19{,}829{,}500 \log x. \qquad \text{(iii)}$$

The results are set forth in table 2, where Pritchett's figures are given for comparison.

TABLE 2

Showing (*a*) the Actual Population[1] on Census Dates, (*b*) Estimated Population from Pritchett's Third-Order Parabola, (*c*) Estimated Population from Logarithmic Parabola, and (*d*) (*e*) Root-Mean Square Errors of Both Methods

CENSUS YEAR	(*a*) OBSERVED POPULATION	(*b*) PRITCHETT ESTIMATE	(*c*) LOGARITHMIC PARABOLA ESTIMATE	(*d*) ERROR OF (*b*)	(*e*) ERROR OF (*c*)
1790	3,929,000	4,012,000	3,693,000	+ 83,000	− 236,000
1800	5,308,000	5,267,000	5,865,000	− 41,000	+ 557,000
1810	7,240,000	7,059,000	7,293,000	− 181,000	+ 53,000
1820	9,638,000	9,571,000	9,404,000	− 67,000	− 234,000
1830	12,866,000	12,985,000	12,577,000	+ 119,000	− 289,000
1840	17,069,000	17,484,000	17,132,000	+ 415,000	+ 63,000
1850	23,192,000	23,250,000	23,129,000	+ 58,000	− 63,000
1860	31,443,000	30,465,000	30,633,000	− 978,000	− 810,000
1870	38,558,000	39,313,000	39,687,000	+ 755,000	+1,129,000
1880	50,156,000	49,975,000	50,318,000	− 181,000	+ 162,000
1890	62,948,000	62,634,000	62,547,000	− 314,000	− 401,000
1900	75,995,000	77,472,000	76,389,000	+1,477,000	+ 394,000
1910	91,972,000	94,673,000	91,647,000	+2,701,000	− 325,000
				935,000[2]	472,000[2]
1920		114,416,000	108,214,000		

[1] To the nearest thousand.

[2] Root-mean square error.

It is obvious from the data of table 2 that, with the same number of constants, the logarithmic parabola gives a distinctly better graduation than a third-order parabola.

The extreme precision of the present graduation is shown graphically in figure 1.

It is evident that as a purely empirical representation of population growth in the United States equation (iii) gives results of a very high degree of accuracy. Indeed, interpolation on this curve for inter-censal years may obviously be relied upon with a greater probability that the estimated figures approximate the unknown true facts than is afforded by any other estimating expedient hitherto applied to the known data. An indication of the general exactness of this curve (iii) for estimating future population by extrapolation may be got in the following way. Suppose a mathematician of the Civil War period had desired to estimate the population of the United States in 1910, and had fitted a curve of the type of (ii), by the method of least squares to the known data available to him, namely, the census counts of 1790, to 1860, inclusive, he would have got this result:

$$y = 8{,}619{,}800 - 5{,}680{,}540x + 822{,}709x^2 + 16{,}987{,}200 \log x \qquad \text{(iv)}$$

If he had calculated from this equation the probable population in 1910, the figure he would have obtained would have been 92,523,000, a result only approximately a half million, or 0.6%, in error, as subsequent events proved. A prophecy less than 1% in error of an event to happen 50 years later is undeniably good predicting.

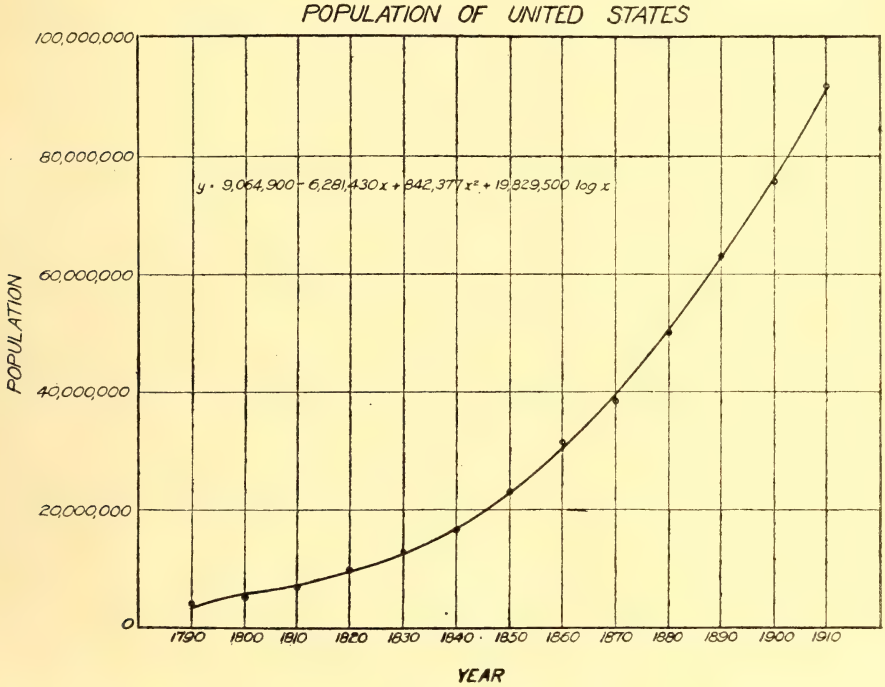

FIG. 1
Diagram showing observed and calculated populations (from logarithmic parabola) from 1790 to 1920.

It is of interest to exhibit the equations and results in predicting the 1910 population obtained by fitting our logarithmic parabola to the data available after the completion of each successive census from 1870 on. We have:

Data for 1790 to 1870, inclusive:

$$y = 8{,}287{,}700 - 5{,}300{,}270x + 795{,}540x^2 + 15{,}778{,}000 \log x \qquad \text{(v)}$$

Predicted population in 1910 = 91,201,000.

Deviation of prediction from actual, 1910 = – 771,000.

Percentage error = 0.8%.

Data for 1790 to 1880 inclusive:

$$y = 7{,}981{,}100 - 4{,}971{,}040x + 764{,}896x^2 + 14{,}993{,}500 \log x \qquad \text{(vi)}$$

Predicted population in 1910 = 89,128,000.
Deviation of prediction from actual, 1910 = – 2,844,000.
Percentage error = 3%.

Data for 1790 to 1890, inclusive:

$$y = 9{,}013{,}800 - 6{,}242{,}170x + 839{,}782x^2 + 19{,}744{,}300 \log x \qquad \text{(vii)}$$

Predicted population in 1910 = 91,573,000.
Deviation of prediction from actual, 1910 = – 399,000.
Percentage error = 0.4 per cent.

Data for 1790 to 1900 inclusive:

$$y = 8{,}748{,}000 - 5{,}880{,}890x + 821{,}001x^2 + 18{,}232{,}100 \log x \qquad \text{(viii)}$$

Predicted population in 1910 = 91,148,000.
Deviation of prediction from actual, 1910 = –824,000.
Percentage error = 0.9%.

Beginning with 1860 (equation (iv)) and coming down to 1900, our hypothetical statistician would have been only once in error as much as 1% in his prediction of the 1910 population by this logarithmic parabola. The one larger error is for the 1880 curve, where apparently the aberrant counts of 1860 and 1870 exert an undue influence.

Altogether it seems justifiable to conclude that:

1. A logarithmic parabola of the type of equation (ii) describes the changes which have occurred in the population of the United States in respect of its gross magnitude, with a higher degree of accuracy than any empirical formula hitherto applied to the purpose.
2. The accuracy of the graduation given by this logarithmic parabola is entirely sufficient for all practical statistical purposes.

II

Satisfactory as the empirical equation above considered is from a practical point of view, it remains the fact that it is an empirical expression solely, and states no general law of population growth. Insofar it is obviously an undesirable point at which to leave the problem of the mathematical expression of the change of population in magnitude.

It is quite clear on *a priori* grounds, as was first pointed out by Malthus in non-mathematical terms, that in any restricted area, such as the United States, a time must eventually come when population will press so closely upon subsistence that its rate of increase per unit of time must be reduced to the vanishing point. In other words, a population curve may start, as does that shown in figure 1, with a convex face to the base, but presently it must develop a point of inflection, and from that point on present a concave face to the x axis, and finally become asymptotic, the asymptote representing the maximum number of people which can be supported on the given fixed area.[8] Now, while an equation like (ii) can, and will in due time, develop a point of inflection and become concave to the base it never can become asymptotic. It, therefore, cannot be regarded as a hopeful line of approach to a true law of population growth.

What we want obviously is a mathematical picture of the *whole* course of population in this country. It is not enough to be able to predict twenty or fifty years ahead as our logarithmic parabola is able to do satisfactorily, *in one portion of the whole curve.* How absurd equation (iii) would be over a really long time range is shown if we attempt to calculate from it the probable population in, say, 3000 A.D. It gives a value of 11,822,000,000. But this is manifestly ridiculous; it would mean a population density of 6.2 persons per acre or 3968 persons per square mile.

It would be the height of presumption to attempt to predict *accurately* the population a thousand years hence. But any real law of population growth ought to give some general and approximate indication of the number of people who would be living at that time within the present area of the United States, provided no cataclysmic alteration of circumstances has in the meantime intervened.

It has seemed worth while to attempt to develop such a law, first by formulating a hypothesis which rigorously meets the logical requirements, and then by seeing whether in fact the hypothesis fits the known facts. The general biological hypothesis which we shall here test embodies as an essential feature the idea that the rate of population increase in a limited area at any instant of time is proportional (*a*) to the magnitude of the population existing at that instant (amount of increase already attained) and (*b*) to the still unutilized potentialities of population support existing in the limited area.

The following conditions should be fulfilled by any equation which is to describe adequately the growth of population in an area of fixed limits.

1. Asymptotic to a line $y = k$ when $x = +\infty$.
2. Asymptotic to a line $y = 0$ when $x = -\infty$.
3. A point of inflection at some point $x = \alpha$ and $y = \beta$.
4. Concave upwards to left of $x = \alpha$ and concave downward to right of $x = \alpha$.
5. No horizontal slope except at $x = \pm\infty$.
6. Values of y varying continuously from 0 to k as x varies from $-\infty$ to $+\infty$.

In these expressions y denotes population, and x denotes time.

An equation which fulfils these requirements is

$$y = \frac{be^{ax}}{1 + ce^{ax}} \tag{ix}$$

when a, b and c have positive values.

In this equation the following relations hold:

$$x = +\infty \qquad y = \frac{b}{c} \tag{x}$$

$$x = -\infty \qquad y = 0 \tag{xi}$$

Relations (x) and (xi) define the asymptotes.

The point of inflection is given by $1-ce^{ax} = 0$, or

$$x = -\frac{1}{a}\log c \qquad y = \frac{b}{2c} \tag{xii}$$

The slope at the point of inflection is $\frac{ab}{4c}$.

Expressing the first derivative of (xi) in terms of y, we have

$$\frac{dy}{dx} = \frac{ay(b-cy)}{b}. \tag{xiii}$$

Putting the equation in this form shows at once that it is identical with that describing an autocatalyzed chemical reaction, a point to which we shall return later.

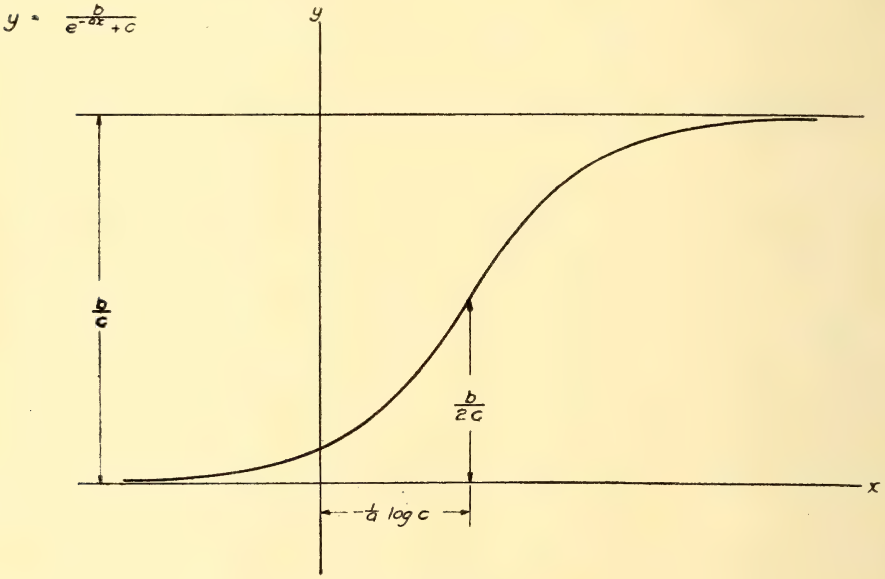

FIG. 2

General form of curve given by equation (ix).

The general form of the curve is shown in figure 2.

The question now is how well does (ix) represent the known historical facts as to the growth in population of the United States, and to what legitimate deductions as to the future course of population in this country does it lead?

It is obvious that equation (ix) as it stands cannot be fitted to observational data by the method of least squares. It is possible to write momental equations and fit by the method of moments, but at this tim we do not care to develop that method because, as will presently appear, we do not regard equation (ix) as the final development of this type of equation for representing population, and we have no desire to encumber the literature with a mathematical discussion which we expect later to discard.

For present purposes it will be sufficient to fit (ix) to the observations by passing it through three points. Given three equally spaced ordinates, y_1, y_2 and y_3, the necessary equations are:

$$\frac{b}{c} = \frac{2y_1y_2y_3 - y_2^2(y_1 + y_3)}{y_1y_3 - y_2^2} \qquad \text{(xiv)}$$

$$a = \log_{10} \frac{y_2\left(\frac{b}{c} - y_1\right)}{y_1\left(\frac{b}{c} - y_2\right)} \div h \log_{10} e \qquad \text{(xv)}$$

where h is the abscissal distance in years between y_1 and y_2, or y_2 and y_3.

$$c = \frac{1}{y_2 - y}\left(\frac{y_1}{e^{a\alpha}} - \frac{y_2}{e^{a(\alpha+h)}}\right) \qquad \text{(xvi)}$$

where α is the abscissal distance in years from the origin to y_1.

Putting x_1 at 1790, x_2 at 1850, and x_3 at 1910, and taking origin at 1780 we have

$y_1 = 3,929$[9]

$\alpha = 10$

$y_2 = 23,192$

$h = 60$

$y_3 = 91,972$

and taking (ix) in the form

$$y = \frac{b}{e^{-ax} + c}, \qquad \text{(xvii)}$$

we find these values for the constants:

$$y = \frac{2,930.3009}{e^{-.0313395x} + 0.014854} \qquad \text{(xviii)}$$

The closeness with which this curve fits the known facts is shown in table 3.

The closeness of fit of this curve is shown graphically in figure 3.

Though empirically arrived at this is a fairly good fit of theory to observations. The root-mean square error from the last column is 463,000, or slightly smaller than that from the logarithmic parabola in table 2. It must not be forgotten, however, that the root-mean square error is reduced in the present case by virtue of the fact that in three out of the 13 ordinates theory and observation are made, by the procrustean method of fitting, to coincide exactly. The most that can be asserted is that

TABLE 3

RESULTS OF FITTING POPULATION DATA 1790 TO 1910 BY EQUATION (XVIII)

YEAR	OBSERVED POPULATION	CALCULATED POPULATION BY EQUATION (XVIII)	ERROR
1790	3,929,000	3,929,000	0
1800	5,308,000	5,336,000	+ 28,000
1810	7,240,000	7,228,000	— 12,000
1820	9,638,000	9,757,000	+119,000
1830	12,866,000	13,109,000	+243,000
1840	17,069,000	17,506,000	+437,000
1850	23,192,000	23,192,000	0
1860	31,443,000	30,412,000	—1,031,000
1870	38,558,000	39,372,000	+ 814,000
1880	50,156,000	50,177,000	+ 21,000
1890	62,948,000	62,769,000	— 179,000
1900	75,995,000	76,870,000	+ 875,000
1910	91,972,000	91,972,000	0

equation (xviii) gives nearly or quite as good a fit to the observations as does the logarithmic parabola. If we properly graduated the data, by the method of moments, we should probably get a result measurably better than that from equation (iii).

The significance of the result lies in this consideration. A curve which on *a priori* grounds meets the conditions which must be satisfied by a true law of population growth, actually describes with a substantial degree of accuracy what is now known of the population history of this country.

Let us examine some further consequences which flow from equation (xviii). The first question which interests one is this: when did or will the population curve of this country pass the point of inflection, and exhibit a progressively diminishing instead of increasing rate of growth? From (xii) it is easily determined that this point occurred about *April 1, 1914*, on the assumption that the numerical values of (xviii) reliably represent the law of population growth in this country. In other words, so far as we may rely upon present numerical values, the United States has already passed its period of most rapid population growth, unless there comes into play some factor not now known and which has never operated during the past history of the country to make the rate of growth more rapid. This latter contingency is improbable. While prophecy is a dangerous pastime, we believe, from the fragmentary results already announced, that the 1920 census will confirm the result indicated by our curve, that the period of most rapid population growth was passed somewhere in the last decade. The population at the point of inflection works out to have been 98,637,000, which was in fact about the population of the country in 1914.

The upper asymptote given by (xviii) has the value 197,274,000 roughly. This means that according to equation (xviii) the maximum population which continental United States, as now areally limited, will ever have will be roughly twice the present population. We fear that some will condemn at once the whole theory because the magnitude of this number is not sufficiently imposing. It is so easy, and most writers on population have been so prone, to extrapolate population by geometric series, or by a parabola or some such purely empirical curve, and arrive at stupendous figures, that calm consideration of real probabilities is most difficult to obtain. While, as will appear from the next section of this paper, we have no desire to defend the numerical results of this section, and indeed ourselves regard them only as a rough first approximation, it remains a fact that if anyone will soberly think of every city, every village, every town in this country having its present population multiplied by 2, and will further think of twice as many persons on the land in agricultural pursuits, he will be bound, we think, to conclude that the country would be fairly densely populated. It would have about 66 persons per square mile of land area.

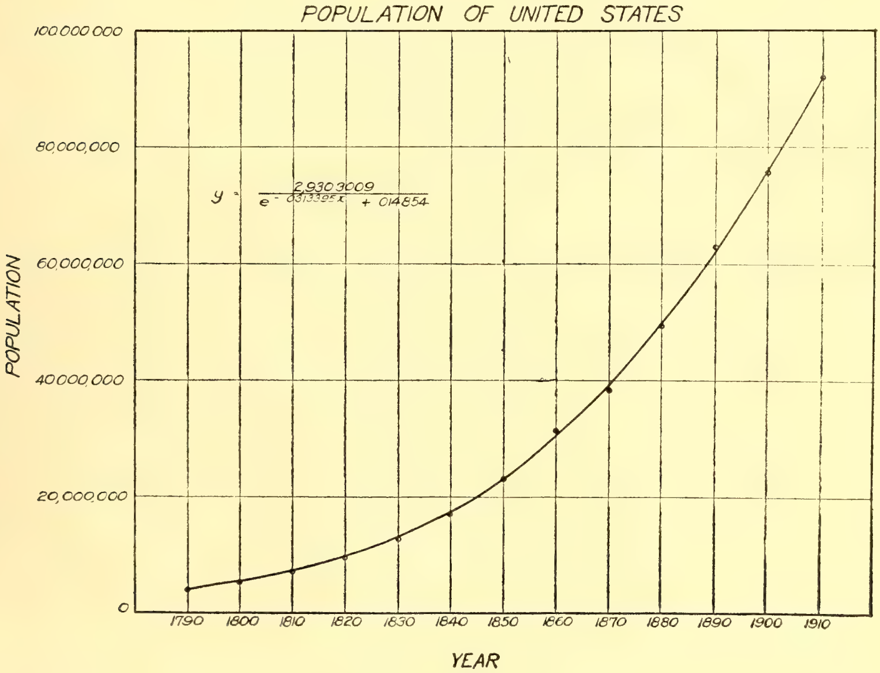

FIG. 3

Showing result of fitting equation (xviii) to population data.

It will at once be pointed out that many European countries have a much greater density of population than 66 persons to the square mile, as for example Belgium with 673, Netherlands with 499, etc. But it must not be forgotten that these countries are far from self-supporting in respect of physical means of subsistence. They are economically self-supporting, which is a very different thing, because by their industrial development at home and in their colonies they produce money enough to buy physical means of subsistence from less densely populated portions of the world. We can, of course, do the same thing, provided that by the time our population gets so dense as to make it necessary there still remain portions of the globe where food, clothing material, and fuel are produced in excess of the needs of their home population. But in this, and in any other scientific discussion of population, it is necessary to limit sharply the *area* one is to talk about. This paper deals with population, and by direct implication the production of physical means of subsistence, within the present area of continental United States.

Now 197,000,000 people will require, on the basis of our present food habits,[10] about 260,000,000,000,000 calories per annum. The United States, during the seven years 1911–1918, produced as an annual average, in the form of human food, *both primary and secondary* (i.e., broadly vegetable and animal), only 137,163,606,000,000 calories per year.[11] So that unless our food habits radically change, and a man is able to do with less than 3000 to 3500 calories per day, or unless our agricultural production radically increases,[12] it will be necessary when our modest figure for the asymptotic population is reached, to import nearly or quite one-half of the calories necessary for that population. It seems improbable that the population will go on increasing at any very rapid rate after such a condition is reached. And is it at all reasonable to suppose that at such time, with all the competition for means of subsistence which the already densely populated countries of Europe will be putting up, there can be found any portion of the globe producing food in excess of its own needs to an extent to make it possible for us to find the calories we shall need to import?

Altogether, we believe it will be the part of wisdom for anyone disposed to criticise out asymptotic value of a hundred and ninety-seven and a quarter millions because it is thought too small, to look further into all the relevant facts.

III

With the above numerical results in hand it is desirable to discuss a little further the general theory of population growth set forth in the preceding section. At the outstart let it be said that we are convinced that equation (ix) represents no more than a first approximation to a true law of population growth. There are several characteristics of this curve which are too rigid and inelastic to meet the requirements of such a law. In (ix) the point of inflection must of necessity lie exactly half-way between the two asymptotes. Furthermore the half of the curve lying to the right of the point of inflection is an exact reversal of the half lying to the left of that point. This implies that the forces which during the latter part of the population history of an area act to inhibit the rate of population growth are equal in magnitude, and exactly similarly distributed in time, to the forces which in the first half of the history operate to accelerate growth. We do not believe that such rigid and inelastic postulates as these are, in fact, realized in population growth.

The same objections apply to the use of the equation of an autocatalytic reaction to the representation of organic growth in the individual. This fact has been noted by Robertson[13] who was the first to discover that, in general, growth follows much the same curve as that of autocatalysis. What needs to be done is to generalize (ix) in some such form as will free it from the two restrictive features (location of point of inflection and symmetry) we have mentioned, and will at the same time retain its other essential features. We are working along this line now and hope presently to reach a satisfactory solution.

We attach no particular significance to the *numerical* results of the preceding section. They obviously can give only the roughest approximation to probable future values of the population of the United States. Our only purpose in presenting them at all at this time is to demonstrate that the hypothesis here advanced as to the law of population growth, even when fitted by a rough and inadequate method, so closely describes the known facts regarding the past history of that growth, as to make it potentially profitable to continue the mathematical development and refinement of this hypothesis further. There is much that appeals to the reason in the hypothesis that growth of population is fundamentally a phenomenon like autocatalysis. In a new and thinly populated country the population already existing there, being impressed with the apparently boundless opportunities, tends to reproduce freely, to urge friends to come from older countries, and by the example of their well-being, actual or potential, to induce strangers to immigrate. As the population becomes more dense and passes into a phase where the still unutilized potentialties of subsistence, measured in terms of population, are measurably smaller than those which have already been utilized, all of these forces tending to the increase of population will become reduced.

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[1] Papers from the Department of Biometry and Bital Statistics, School of Hygiene and Public Health, Johns Hopkins University, No. 13.

[2] Cf. for a discussion on the relation of curve fitting to true organic laws of change, Pearl, R. "Some Recent Studies on Growth," *Amer. Nat.*, **43,** 1909 (302–316).

[3] Perrin, E., "On Some Dangers of Extrapolation," *Biometrika,* **3,** 1904 (99–103).

[4] Pritchett, A. S., "A Formula for Predicting the Population of the United States," *Quart. Publ. Amer. Statistical Assoc.*, **2,** 1891 (278–286).

[5] Pearl, R., "Variation and Differentiation in Ceratophyllum," *Carnegie Inst. Washington, Publ.* **58,** 1907 (136).

[6] Cf. the following papers:

Curtis, M. R., 1914, "A Biometrical Study of Egg Production in the Domestic Fowl. IV. Factors Influencing the Size, Shape, and Physical Constitution of Eggs," *Arch. Entwicklungsmech. Organ.*, B. **39,** Heft 2/3, pp. 217–327.

Pearl, R., 1909, "Studies on the Physiology of Reproduction in the Domestic Fowl. I. Regulation in the Morphogenetic Activity of the Oviduct," *J. Exp. Zool.*, **6,** No. 3, pp. 339–359.

Pearl, R., 1914, "On the Law Relating Milk Flow to Age in Dairy Cattle," *Proc. Soc. Expt. Biol. Med.*, **12,** No. 1, pp. 18–19.

[7] Donaldson, H. H., 1908, "A Comparison of the Albino Rat with Man in Respect to the Growth of the Brain and of the Spinal Cord," *J. Compar. Neurol. Psych.*, **18,** No. 4, pp. 345–389.

Donaldson, H. H., 1909, "On the Relation of the Body Length to the Body Weight and to the Weight of the Brain and of the Spinal Cord in the Albino Rat (*Mus norvegicus var. albus*)," *Ibid.*, **19,** No. 2, pp. 155–167.

Donaldson, H. H., 1910, "On the Percentage of Water in the Brain and in the Spinal Cord of the Albino Rat," *Ibid.*, **20,** No. 3, pp. 119–144.

Donaldson, H. H., 1911, "On the Regular Seasonal Changes in the Relative Weight of the Central Nervous System of the Leopard Frog," *J. Morph.*, **22,** pp. 663–694.

Donaldson and Hatai, Shinkishi, 1911, "A Comparison of the Norway Rat with the Albino Rat in Respect to Body Length, Brain Weight, Spinal Cord Weight, and the Percentage of Water in Both the Brain and the Spinal Cord," *J. Compar. Neurol. Psych.*, **21,** pp. 417–458.

Hatai, Shinkishi, 1909, "Note on the Formulas Used for Calculating the Weight of the Brain in the Albino Rats," *Ibid.*, **19,** No. 2, pp. 169–173.

Hatai, Shinkishi, 1911, "A Formula for Determining the Total Length of the Leopard Frog (*R. pipiens*) for a Given Body Weight," *Anat. Rec.*, **5,** No. 6, pp. 309–312.

Hatai, Shinkishi, 1911, "An Interpretation of Growth Curves from a Dynamical Standpoint," *Ibid.*, **5,** No. 8, pp. 373–382.

[8] Always, be it clearly understood, on the assumption that the average standard of living, method of agricultural production, etc., either do not further change at all in the period between the end of historical past record of fact and the time when dy/dx becomes negligibly small as the asymptote is approached, or that the net effective magnitude of any such changes as do occur will be relatively so small as to be negligible in comparison with the effect of such factors as reproduction and immigration in determining the relation between population and time in an area of fixed limits. In any mathematical treatment of the subject these factors of standard of living, methods of agriculture, etc., represent essentially constant (and hence omitted) parameters of any assumed functional relation between population and time.

[9] Omitting 000 here and in the subsequent calculations till the end.

[10] Cf. Pearl, R., *The Nation's Food,* Philadelphia (W. B. Saunders Company), 1920 (247).

[11] Pearl, R., *loc. cit.*, p. 76.

[12] As a matter of fact East, in his able presidential address on "Population," before the American Society of Naturalists has shown that the United States has already entered upon the era of diminishing returns in agriculture in this country.

[13] Robertson, T. Brailsford, "On the Normal Rate of Growth of an Individual and Its Biochemical Significance," *Arch. Entwickmech. Organ.*, **25,** pp. 581–614.

Robertson, T. Brailsford, "Further Remarks on the Normal Rate of Growth of an Individual, and Its Biochemical Significance," *Ibid.*, **26,** pp. 108–118.

ON THE SPRINGFIELD RIFLE AND THE LEDUC FORMULA*

By ARTHUR GORDON WEBSTER

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Read before the Academy, April 26, 1920

One of the formulae of interior ballistics found most useful, at any rate by practical ballisticians in the United States, is the formula of Leduc, which says that the velocity of a shot in the bore of a gun is graphically represented in terms of the distance travelled by a rectangular hyperbola, giving the formula:

$$v = \frac{as}{b + s}$$

where v is the velocity, s the distance travelled by the shot.

The writer had the curiosity to try this formula on the Springfield rifle. The observations were made by the gauge described in these PROCEEDINGS, **5,** July, 1919 (259–263). The mode of deducing the Leduc formula is decidedly open to criticism since it assumes the combustion to be instantaneous, and the expansion adiabatic, neither of which is true and it is perfectly obvious that powders giving curves so different as those published in the paper quoted cannot possibly give v, s curves of the same shape. Nevertheless, it turns out that for the rifle the Leduc formula answers very well indeed.

Observations were made on the time of reaching different points in the barrel by Mr. H. C. Parker, assistant in the Ballistic Institute of Clark University. The method was to put a fine wire insulated with enamel down the barrel of the rifle to a certain distance. When the shot reached it a circuit was made which made a current passing through the oscillograph. The motion of the oscillograph was photographed on a rotating drum and thus the times were obtained. The curve obtained for the actual times of reaching ten or a dozen distances was exactly similar to that shown in the paper cited above, obtained by a double integration of the pressure-time curve. The velocities were obtained from Mr. Parker's curve by a graphic differentiation, and from the known velocities and distances the formula was obtained. Even without the method of least squares the formula $v = \dfrac{1133s}{19.8 + s}$ answers very well where the distances are given in centimeters and the velocities in meters per second.

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* Contribution from the Ballistic Institute, No. 7.

ALKYL AMIDES OF ISOTHIOCYANACETIC ACID

By Treat B. Johnson, Arthur J. Hill and Erwin B. Kelsey

Department of Chemistry, Yale University

Communicated April 10, 1920

Theoretically, anilides of chloracetic acid, of which chloracetanilide I is the simplest representative, might be expected to interact with potassium thiocyanate in two ways, namely, to form either a normal thiocyanate or its structural isomer, an isothiocyanate, as is represented by formulas II and III, respectively. A careful study of this reaction, however, has revealed the fact that it is productive only of the thiocyanate

$$ClCH_2CONHC_6H_5$$

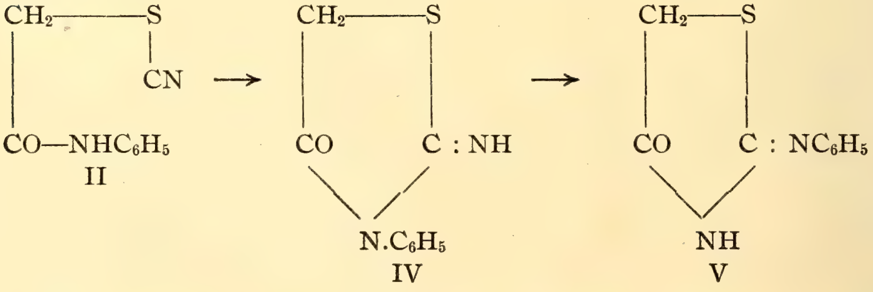
I

$$NCS.CH_2CONHC_6H_5 \qquad\qquad SCN.CH_2CONHC_6H_5$$

II   III

form of these two isomers, or that combination in which sulphur is joined to carbon as represented in formula II. In every case, where the reaction has been represented as proceeding in the other manner, a careful examination of the properties of the reaction-product has revealed an incorret interpretation of the change, and shown that the primary reaction-product possesses a different constitution than that of an isothiocyanate. In other words, we may conclude from the evidence thus far presented, that no anilide of isothiocyanacetic acid has yet been prepared.[1]

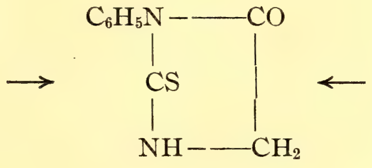
Wheeler and Johnson[2] submitted this reaction between potassium thiocyanate and chloracetanilides to a very careful investigation and showed that the primary product of reaction is always a normal thiocyanate. They found, however, that these combinations are not always stable and in some cases it was extremely difficult to isolate them in a state of purity. This instability is explained by the fact that they easily undergo an unique rearrangement and are transformed into pseudothiohydantoins. In no case were normal thiohydantoins obtained by rearrangement. The isomerization of thiocyanacetanilide may be represented as follows:



The conclusions recorded in a recent publication by Beckurts and Frerichs[3] have now forced us to turn our attention again to this field of research. These investigators have included in their recent work several new anilides of chloracetic acid, and in the case of three of them they interpret the reaction with potassium thiocyanate as leading to the formation of isothiocyanates. The anilides which interact with potassium thiocyanate in this unique manner are the *m*-toluide, *p*-chloranilide and *p*-anisidide of chloracetic acid.

In the light of these interesting results, it has become necessary for us to take up again this question of constitution from an entirely new viewpoint, and apply the synthetical method of investigation. The question of tautomeric behavior of potassium thiocyanate must be excluded from further consideration, and a fundamental method of synthesis be developed whereby only true isothiocyanate combinations corresponding to formula III can be formed. At present such a method of synthesis is not known. These isothiocyanates if formed would not revert by rearrangement to normal thiocyanates, and should exhibit a chemical behavior quite as characteristic as that of the isomeric thiocyanates. Theoretically combinations corresponding to formula III should easily undergo isomerization with formation of normal thiohydantoins. In other words, if the isothiocyanate III is a product of reaction when potassium thiocyanate interacts with chloracetanilide as Frerich and Beckurts assume, it should act in an entirely different manner than the thiocyanate II and isomerize to the same hydantoin VI, as is obtained by interaction of phenylisothiocyanate with glycine[4] or its ethyl ester. These changes are represented as follows:

$$ClCH_2CONHC_6H_5 \longrightarrow \underset{\text{III}}{SCN.CH_2CONHC_6H_5}$$



$$\longrightarrow \begin{array}{ccc} C_6H_5N & - & CO \\ | & & | \\ CS & & | \\ | & & | \\ NH & - & CH_2 \end{array} \longleftarrow$$

VI

$$C_6H_5NCS + NH_2CH_2COOH \longrightarrow C_6H_5NHCSNHCH_{2}3COOH$$

We are now in position to report a practical method of synthesis which enables us to obtain anilides of isothiocyanacetic acid. Starting with chloracetanilide for illustration the complete operation may be outlined as follows: Chloracetanilide is first converted into the anilide of glycocoll (glycine) VII, and this then combined with carbonbisulphide when the corresponding dithiocarbamate VIII is obtained in excellent yield. This salt VIII is a crystalline substance melting at 145° and its formation from chloracetanilide is expressed as follows:

$$ClCH_2CONHC_6H_5 + 2NH_3 = NH_4Cl + \underset{\text{VII}}{NH_2CH_2CONHC_6H_5}$$

$$2NH_2CH_2CONHC_6H_5 + CS_2 = C_6H_5NH.COCH_2NHCSSH.NH_2CH_2CO\text{-}NHC_6H_5$$

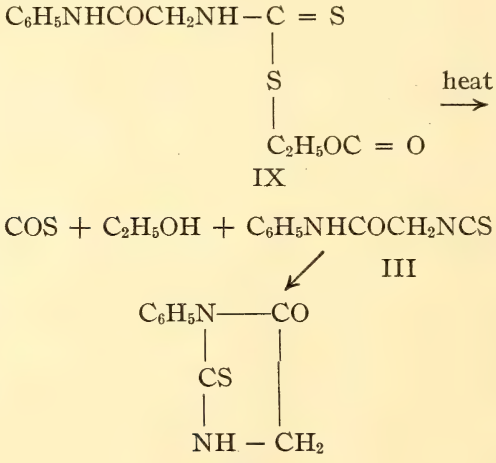
VIII

The dithiocarbamate VIII interacts smoothly with ethyl chlorformate to give the corresponding ester represented by formula IX. This ester IX is the first representative of its type to be described in the chemical

$$C_6H_5NHCOCH_2NH.CSSH.NH_2CH_2CONHC_6H_5 + Cl.COOC_2H_5 = C_6H_5NHCOCH_2NH.CSS.COOC_2H_5 + C_6H_5NH.CO.CH_2NH_2HCl$$

IX

literature, and is a crystalline substance melting at 100°. This ester can be crystallized without decomposition, but undergoes the normal decomposition, characteristic of its constitution, when heated above its melting point temperature. It then breaks down into carbonoxysulphide, ethyl alcohol and the unknown isothiocyanate III. This characteristic decomposition reaction is expressed as follows:



IX

$$COS + C_2H_5OH + C_6H_5NHCOCH_2NCS$$

III

VI

Thus far we have not succeeded in stopping our reaction at the stage where the isothiocyanate III is formed. To our surprise this substance is so unstable that the heat applied to decompose the dithiocarbamic ester IX is sufficient to produce immediate rearrangement of the resulting isothiocyanate III into the normal phenylthiohydantoin VI. The yield of this cyclic compound is excellent, and as we anticipated it proved to be identical with the thiohydantoin previously described in the literature.[5] We obtained no evidence of the formation of either of the two pseudothiohydantoins IV and V which result by rearrangement of thiocyanacetanilide II. In other words, our method of synthesis not only excludes the possibility of formation of a normal thiocyanate, but has also revealed to us for the first time the striking dissimilarity in chemical behavior between these two types of isomeric compounds.

It seems safe to conclude, therefore, from the results of this new work that any isothiocyanate formed by interaction of potassium thiocyanate with a secondary chloracetanilide will be found to be unstable and easily transformed into its corresponding thiohydantoin. Whether Beckurts and Frerichs were actually dealing in their work with such normal thiohydantoins instead of the pseudo modifications must be decided by further investigation. In the case of tertiary anilides of isothiocyanacetic $SCN.CH_2CO.NR_2$, such cyclic rearrangements cannot take place, and in such cases we expect to obtain acyclic combinations containing the grouping NCS functioning as a true isothiocyanate. Investigations dealing with various phases of this interesting problem are now in progress and the results will be published in the *Journal of the American Chemical Society.*

[1] Wheeler and Merriam, *J. Amer. Chem. Soc.*, **23,** 1901 (283).

[2] *Amer. Chem. J.*, **28,** 1902 (121); Johnson, *J. Amer. Chem. Soc.*, **25,** 1903 (483).

[3] *Arch. Pharm.*, **253** (233); *Chem. Zentrbl.*, **2,** 1915 (614); *Chem. Abstracts*, **10,** 1916, (888).

[4] Aschan, *Ber. chem. Ges.*, **17,** 1884 (420); Marckwald, Neumark and Stelzner, *Ibid.*, **24,** 1891 (3278).

[5] *Loc. cit.*

STUDIES OF MAGNITUDES IN STAR CLUSTERS. XI. FREQUENCY OF CURVES THE ABSOLUTE MAGNITUDE AND COLOR INDEX FOR 1152 GIANT STARS.

By Harlow Shapley

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Communicated by G. E. Hale, April 8, 1920

Kapteyn, Schwarzschild, Parkhurst, Russell, and other investigators of statistical problems in stellar astronomy have paid special attention to the relative frequency of successive values of the absolute brightness of stars (the luminosity law) and the relative frequency of different spectral or color types. The laws of the frequencies of absolute magnitude and spectrum are indeed fundamental in studies of stellar evolution and the arrangement of stars in space; but to determine these laws, at the same time keeping the errors due to unavoidable and vitiating selection of data at a minimum, is by no means a simple process. Insufficient knowledge of stellar distances, and frequently of apparent magnitudes and spectra as well, presents a serious obstacle. In particular, it is difficult to obtain satisfactory luminosity curves for each spectral type, or representative spectral curves for small and clearly defined intervals of absolute magnitude. The luminosity curve that does not differentiate

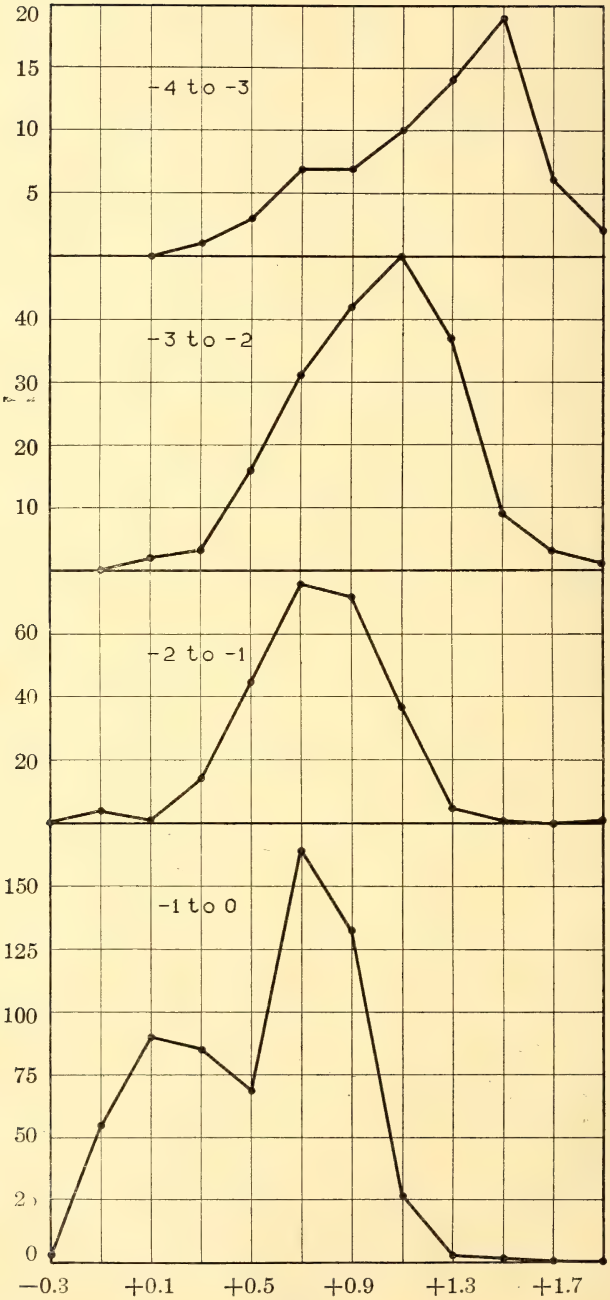

FIG. 1

Curves of the frequency of color index for successive intervals of absolute magnitude. Ordinates are numbers of stars; abscissae are color indices.

spectral types conceals the important differences for red and blue stars of the giant-dwarf phenomena; and the spectral (or color) curve that includes a great range of absolute magnitude is of limited value because it integrates indiscriminately the various masses, densities, ages, and other factors that appear to change with luminosity.

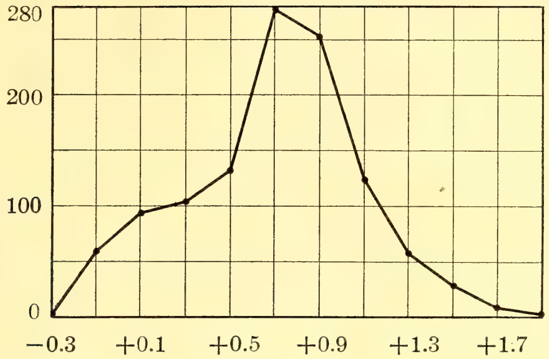

FIG. 2

Integrated color curve for 1145 giant stars of all absolute magnitudes brighter than zero. Coördinates as in Fig. 1.

The study of magnitudes and colors in star clusters affords, however, for the brightest of giant stars, a fairly accurate determination of the frequencies of magnitudes and colors for small intervals of color and magnitude, respectively. The relative apparent magnitudes and distances, which in most cases are prerequisite to a knowledge of absolute brightness, are sufficiently known in nine systems to permit a combination of all observations for a statistical investigation of the above-mentioned astrophysical laws.

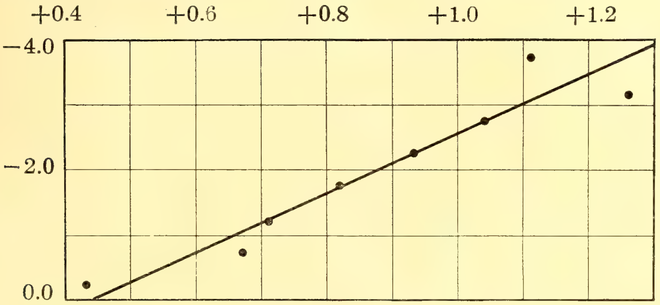

FIG. 3

Change of mean color index with brightness for 1143 giant stars in clusters. Ordinates are absolute photovisual magnitudes; abscissae are color indices. (See last three columns of Table I.)

The discussion of the observational data for 1152 stars brighter than absolute photovisual magnitude zero is summarized in the tabular results and illustrative curves of the present note.

The clusters whose stars are included in this synopsis of absolute magnitude are Messier 3, 5, 11, 13, 15, 30, 68; N. G. C. 4147, 7006. Their distances range between seven and seventy thousand parsecs; the apparent magnitudes that correspond to zero absolute magnitude range between 14.3 and 19.2.

The available absolute magnitudes fainter than zero are less accurate than those for the brighter giants, and for many of the most distant clusters the data for the fainter stars are very incomplete. The present study therefore is limited to stars brighter than zero magnitude, and for the luminosity curves it is restricted to the clusters Messier 3, 11, and 13, for which we have published catalogues of magnitudes and colors complete to the adopted fainter limit.

TABLE I

DATA FOR CURVES OF COLOR FREQUENCY

(Tabulated quantities are numbers of stars)

ABSOLUTE PHOTOVISUAL MAGNITUDE	COLOR INDEX												ALL COLORS	MEAN MAGNITUDE	MEAN COLOR INDEX
	−0.4 to −0.2	−0.2 to 0.0	0.0 to +0.2	+0.2 to +0.4	+0.4 to +0.6	+0.6 to +0.8	+0.8 to +1.0	+1.0 to +1.2	+1.2 to +1.4	+1.4 to +1.6	+1.6 to +1.8	+1.8 to +2.0			
−5.0 to −4.5	0	0	0	0	0	0	0	0	0	0	0	0	0		
−4.5 to −4.0	0	0	0	1	0	0	0	1	0	0	0	0	2	−4.38	+0.77
−4.0 to −3.5	0	0	0	1	2	2	2	3	3	6	1	0	20	−3.74	+1.11
−3.5 to −3.0	0	0	0	0	1	5	5	7	11	13	5	2	49	−3.18	+1.26
−3.0 to −2.5	0	0	1	1	7	14	13	21	25	6	1	1	90	−2.77	+1.04
−2.5 to −2.0	0	0	1	2	9	17	29	29	12	1	2	0	102	−2.25	+0.93
−2.0 to −1.5	0	0	1	4	22	16	39	24	3	1	0	0	110	−1.76	+0.82
−1.5 to −1.0	0	4	0	10	23	60	33	13	2	0	0	1	146	−1.22	+0.71
−1.0 to −0.5	1	1	16	34	18	60	77	18	1	1	0	0	227	−0.72	+0.67
−0.5 to 0.0	2	54	74	51	50	104	55	8	1	0	0	0	399	−0.23	+0.43
All magnitudes	3	59	93	104	132	278	253	124	58	28	9	4	1145	−1.18	+0.69

For intervals of 0.2 mag. in color index and of 0.5 mag. in absolute photovisual magnitude table I contains the number of stars from all nine clusters. Only seven of the 1152 stars fall outside the limits of this table. Their absolute magnitude and color indices are:

−5.45	+1.01	−2.77	+2.06
−4.14	+1.23	−2.19	−0.52
−5.09	+1.00	−1.03	−0.60
−5.01	+1.83		

It should be especially noted that only six stars (four of those above and two in table I) are brighter than −4.0, and the brightest recorded star is fainter than —5.5. We are able, moreover, to go much farther with this inference relative to an upper limit for stellar luminosity. The investigations carried on at Mount Wilson during the last six years have yielded provisional but sufficiently reliable absolute magnitudes in more than fifty clusters; they permit the statement that, *of more than a million stars, less than a tenth of 1% are brighter than −4, photovisually.* Since the brightest giants are red, the corresponding upper limit for absolute photographic magnitude may be placed at −3. There appears to be scarcely a single star brighter than photovisual magnitude −6.

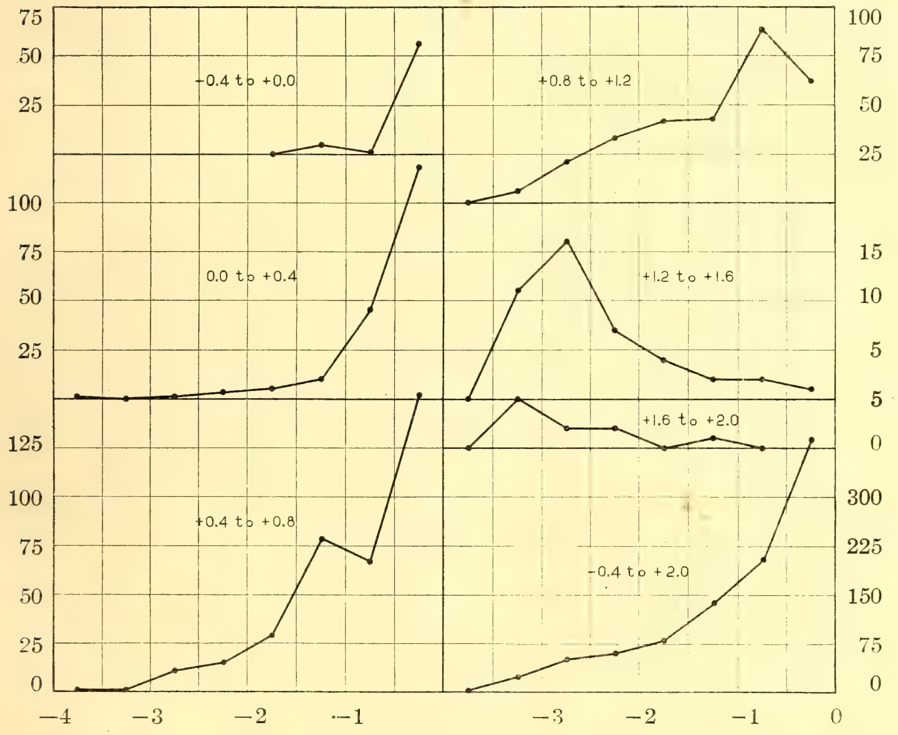

FIG. 4

Luminosity curves for different intervals of color index (Table II). Ordinates are numbers of stars; abscissae are absolute photovisual magnitudes.

The existence of a maximum luminosity, indicated by these results, is probably closely related to the upper limits of stellar mass suggested by Eddington's researches on giant stars; it is highly significant in considerations of external galactic systems.

It is also of interest that of the 1152 stars only three may be considered slightly abnormal in color, in that they fall outside the limits of table I. This speaks for the uniformity of stars throughout the universe, and for the comparability of the spectral phenomena in globular clusters with those of the solar neighborhood.

Color frequency-curves for intervals of one magnitude are shown in figures 1 and 2, based on the data of table I. The striking progression of maximum frequency toward the blue, with decreasing magnitude, again emphasizes the law of average color for giant stars in clusters, viz., *the brighter the giant star the greater its color index.* The progression of average color with luminosity is also clearly shown by the means in the last two columns of table I, which are plotted in figure 3, the six stars brighter than −4.0 being omitted.

A double maximum appears in the color curve for stars between magnitudes 0.0 and −1.0. It is not the result of the combination of observations from clusters at different stages of development, because it is shown separately for Messier 3 and 13. It may be due to unequal evolutionary values for the different intervals of color—to a relatively speedier development, that is, throughout the interval of color index +0.2 to +0.6, for stars of a certain mass.

TABLE II

DATA FOR LUMINOSITY CURVES

(Tabulated quantities are numbers of stars)

ABSOLUTE PHOTOVISUAL MAGNITUDE	COLOR INDEX												ALL COLORS
	−0.4 to −0.2	−0.2 to 0.0	0.0 to +0.2	+0.2 to +0.4	+0.4 to +0.6	+0.6 to +0.8	+0.8 to +1.0	+1.0 to +1.2	+1.2 to +1.4	+1.4 to +1.6	+1.6 to +1.8	+1.8 to +2.0	
−5.0 to −4.5	0	0	0	0	0	0	0	0	0	0	0	0	0
−4.5 to −4.0	0	0	0	0	0	0	0	0	0	0	0	0	0
−4.0 to −3.5	0	0	0	1	0	1	0	0	0	0	0	0	2
−3.5 to −3.0	0	0	0	0	0	1	3	3	6	5	3	2	23
−3.0 to −2.5	0	0	0	1	1	10	8	13	11	5	1	1	51
−2.5 to −2.0	0	0	1	2	7	8	17	16	6	1	2	0	60
−2.0 to −1.5	0	0	1	4	20	9	25	17	3	1	0	0	80
−1.5 to −1.0	0	4	0	10	22	56	32	11	2	0	0	1	138
−1.0 to −0.5	0	1	15	30	13	54	71	17	1	1	0	0	203
−0.5 to 0.0	2	54	71	47	49	103	54	8	1	0	0	0	389
All magnitudes	2	59	88	95	112	242	210	85	30	13	6	4	946

Table II, similar in form to table I, contains data for the luminosity curves (fig. 4), showing the distribution in color and absolute magnitude of 946 stars from Messier 3, 11, and 13. These curves, for intervals of 0.4 mag. in color index, indicate that our material gives the complete distribution of luminosity for the red giants, but that for the blue stars the maximum frequency falls below zero absolute magnitude.

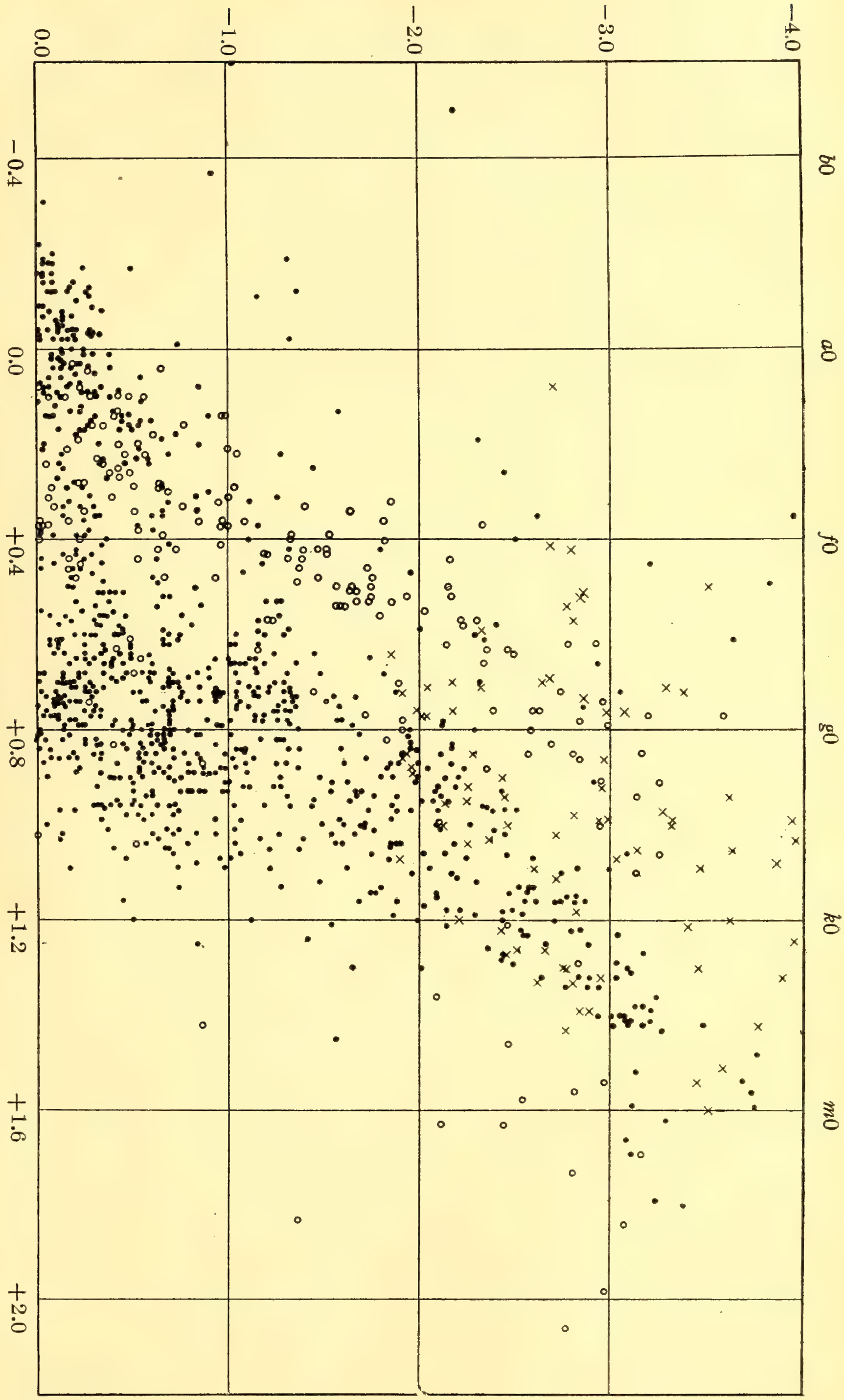

FIG. 5

Distribution of giant stars in color and brightness. Ordinates are absolute photovisual magnitudes; abscissae are color indices with the corresponding color class indicated at the top of the diagram. (See last paragraph of text for further explanation.)

It is believed that the average probable error of the paralaxes of these nine clusters is much less than 15%. Suppose, however, that for one of the clusters the true distance differs by 30% from the adopted value. The adopted absolute magnitudes for that cluster would be systematically in error by about 0.6 mag. The position of the maxima of the luminosity curves for the nine clusters together would be slightly altered, therefore, if the supposed error were uncompensated; but the general form of none of the curves would be materially changed.

Figure 5 represents the distribution in color and luminosity of the individual stars, the circles representing data from Messier 11, the dots giving results for the most accurately measured clusters (Messier 3, 5, 13, 15, and 68), and the crosses representing the stars for the clusters for which the magnitudes depend upon less extensive investigations. Together with the luminosity and color curves, this diagram illustrates the present state of our information concerning the giant stars in clusters.

ON THE DISTORTION IN CONFORMAL MAPPING WHEN THE SECOND COEFFICIENT IN THE MAPPING FUNCTION HAS AN ASSIGNED VALUE

By T. H. GRONWALL

TECHNICAL STAFF, OFFICE OF THE CHIEF OF ORDNANCE, WASHINGTON, D. C.

Communicated by E. H. Moore, April 27, 1920

Note III On Conformal Mapping Under Aid of Grant No. 207 From the Bache Fund

Let $w = z + a_2z^2 + \ldots + a_nz^n + \ldots$ be a power series in z convergent for $z| < |1$ and such that the circle $|z| < 1$ is mapped conformally on a simple (that is, simply connected and nowhere overlapping) region in the w-plane. Koebe[1] has shown that on the circumference $|z| = r$, where $0 < r < 1$, the distortion $|dw/dz|$ and also $|z|$ lie between positive bounds depending on r alone, and the writer[2] has determined the exact values of these bounds. A far more difficult problem arises when some of the coefficients of the power series are given *a priori*. The simplest case where $a_2 = ae^{\gamma i}$ $(a \geq 0)$ is given was investigated by the writer,[3] the method employed failing, however, to furnish the upper bound of $|z|$ in the case $0 \leq a < 1$. This defect has now been remedied, and denoting by $r(a)$, for $0 \leq a < 1$, the root between zero and unity of the equation

$$\frac{2r}{1 + 2(a-1)r + r^2} - \log\frac{1+r}{1-r} = 0,$$

and by $\cos\beta$, for $0 < r \leq r(a)$, the positive root of the equation

$$\frac{2r}{1-2r\cos\beta + r^2} - \log\frac{1+r}{1-r} = 0,$$

we have the following:

THEOREM: *When the analytic function*

$$w = z + ae^{\gamma i}z^2 + a_3z^3 + \ldots + a_nz^n + \ldots$$

(*where* $a \geq 0$ *and* γ *real*) *maps the circle* $|z| < 1$ *on the interior of a simple region D in the w-plane, then* $a \leq 2$, *and we have, for* $|z| = r$ *and* $0 < r < 1$,

$$\frac{1 - r^2}{(1 + ar + r^2)^2} < \left|\frac{dw}{dz}\right| < \left\{\begin{array}{l} \dfrac{1 - 2r\cos\beta + r^2}{(1 - r)^{2+\cos\beta+a}(1 + r)^{2-\cos\beta-a}} \\ \text{[for } a < 1 \text{ and } r \leq r(a)\text{]}, \\ \dfrac{1 + 2(a - 1)r + r^2}{(1 - r)^3(1 + r)} \\ \text{[for } a < 1 \text{ and } r \geq r(a)\text{, and for } 1 \leq a \leq 2\text{]}, \end{array}\right.$$

$$\frac{r}{1 + ar + r^2} < |w| < \left\{\begin{array}{l} \dfrac{1}{4(1+a)}\left[\left(\dfrac{1+r}{1-r}\right)^{1+a} - 1\right] + \dfrac{1}{4(1-a)}\left[1 - \left(\dfrac{1-r}{1+r}\right)^{1-a}\right] \\ \text{[for } a < 1\text{]}, \\ \dfrac{2 - a}{4}\log\dfrac{1 + r}{1 - r} + \dfrac{a}{2}\dfrac{r}{(1 - r)^2} \\ \text{[for } 1 \leq a \leq 2\text{]}, \end{array}\right.$$

except that these bounds are reached for the functions w obtained upon replacing r by $ze^{\gamma i}$ *and* $-ze^{\gamma i}$ *in the upper and lower bounds, respectively. When the region D is* CONVEX, *then* $a \leq 1$, *and we have*

$$\frac{1}{1 + 2ar + r^2} < \left|\frac{dw}{dz}\right| < \frac{1}{(1 + r)^{1+a}(1 + r)^{1-a}},$$

$$\left.\begin{array}{l} \dfrac{1}{\sqrt{1 - a^2}}\arctan\dfrac{\sqrt{1 - a^2}.r}{1 + ar} \\ \text{[for } a < 1\text{]}, \\ \dfrac{r}{1 - r} \\ \text{[for } a = 1\text{]}, \end{array}\right\} < |w| < \left\{\begin{array}{l} \dfrac{1}{2}\log\dfrac{1 + r}{1 - r} \\ \text{[for } a = 0\text{]}, \\ \dfrac{1}{2a}\left[\left(\dfrac{1 + r}{1 - r}\right)^a - 1\right] \\ \text{[for } 0 < a \leq 1\text{]}, \end{array}\right.$$

except for the functions w obtained as above.

As an application, let us suppose that the function

$$w = 1/z + c_1z + \ldots + c_nz^n + \ldots$$

(without constant term) maps $|z| < 1$ on a simple region in the w-plane (this region containing, of course, $w = \infty$ in its interior). Inequalities of the form $|w| < k/r$ for $|z| = r$, $0 < r < 1$, k = constant, were obtained by Fricke[4] and the writer.[5] From the assumption, it follows that

$$W = 1/w = z + a_3z^3 + \ldots + a_nz^n + \ldots$$

maps $|z| < 1$ on a simple region in the W-plane, and the application of our theorem with $a = 0$ gives

$$\frac{r}{1 + r^2} < |W| < \frac{r}{1 - r^2},$$

or

$$\frac{1}{r} - r < |w| < \frac{1}{r} + r$$

for $|z| = r$, $0 < r < 1$, except when

$$w = \frac{1}{z} + e^{2\alpha i} z$$

with α real, the upper and lower bounds of $|w|$ being then reached when $z = \pm e^{-\alpha i} r$ and $z = \pm i e^{-\alpha i} r$, respectively. This particular function maps $|z| < 1$ on the w-plane cut along the straight line from $w = -2e^{\alpha i}$ to $w = 2e^{\alpha i}$.

[1] Koebe, *Göttingen, Nachr. Ges. Wiss.*, 1909 (73).

[2] Gronwall, *Paris, C. R. Acad. Sci.*, **162,** 1916 (249).

[3] Gronwall, *Ibid.*, **162,** 1916 (316).

[4] Fricke and Klein, *Vorlesungen über die Theorie der automorphen Functionen,* II, Leipzig, 1912 (497).

[5] Gronwall, *Annals Math., Princeton,* Ser. 2, **16,** 1914 (74).

ON THE CONNECTION OF THE SPECIFIC HEATS WITH THE EQUATION OF STATE OF A GAS*

By Arthur Gordon Webster

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Read before the Academy, April 26, 1920

One would suppose that this subject was already exhausted, but I am disposed to believe that such is not the case. In these Proceedings, **5,** July, 1919 (286–288) I published a paper "On the Possible Form of the Equation of State of Powder Gases." In a private letter from M. LeChatelier, in commenting on this paper, he says: "Votre objection au sujet de nos chaleurs spécifiques nous était venue à l'esprit, mais nous pensions avoir démontré, en partant des principes de la thermodynamique, que toutes les fois qu'un fluide obéit a une équation caracteristique de la forme

$$V = F(P/T)$$

les chaleurs spécifiques sont indépendantes de la pression."

I do not find where, if anywhere, M. LeChatelier has published this conclusion. Certainly it is not in the great paper by Mallard and LeChatelier, *Journal de Physique,* Ser. 2, **4** (59–84), "Recherches sur la Combustion des Mélanges Gazeux Explosifs," where the matter is not mentioned. In a paper published in the *Physical Review,* **29,** No. 3, September, 1909, "On the Definition of an Ideal Gas," written by me, but suggested by my then colleague Professor Rosanoff, and published under our joint names, I have investigated the properties of a gas having the equation of state mentioned by M. LeChatelier, but have said nothing about the specific heats. Inasmuch as the matter seems to be not quite clear and inasmuch as in the five-foot shelf of treatises on thermodynamics in my library I cannot find the answer to the question that I have in mind, I think it worth while to take the matter up *ab initio.* To be sure my colleague Professor Charles A. Kraus has pointed out to me that van der Waals, in his excellent Lehrbuch der Thermodynamik, Kap. II, has partially treated the subject, and Professor Gilbert N. Lewis, whom I met yesterday on the Avenue, referred me to the same "well-known formula," and intimated that there was nothing more to be added. I am not quite convinced. After teaching thermodynamics for thirty years I find it is only now that my ideas on some points are becoming clarified.

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* Contribution from the Ballistic Institute, Clark University, No. 8.

Let us suppose any system to be specified by two coördinates, x and y, in terms of which all thermal data may be expressed, neither being the temperature. If we deal with unit of mass of a substance, the specific heat at constant x is defined by the equation

$$dQ_x = C_x dT_x,$$

where dT_x is the change of temperature made when x is constant and dQ is the amount of heat required for a change of state. Similarly for the specific heat at constant y:

$$dQ_y = C_y dT_y,$$

Now we have in any change

$$dT = \frac{\partial T}{\partial x} dx + \frac{\partial T}{\partial y} dy,$$

and consequently:

$$dQ_x = C_x \frac{\partial T}{\partial y}, \quad dQ_y = C_y \frac{\partial T}{\partial x} dx.$$

Since for an infinitesimal change the total amount of heat required is the sum of the two preceding, we have for any change whatever

$$dQ = C_y \frac{\partial T}{\partial x} dx + C_x \frac{\partial T}{\partial y} dy.$$

But, by the first law of thermodynamics, when we are dealing with a system under a uniform normal pressure p we have

$$dQ = dU + pdv$$

where U is the intrinsic energy of the substance. Writing out the perfect differentials dU and dv in terms of dx and dy, which are independent, we have the two partial differential equations:

$$\frac{\partial U}{\partial x} = C_y \frac{\partial T}{\partial x} - p \frac{\partial v}{\partial x},$$

$$\frac{\partial U}{\partial y} = C_x \frac{\partial T}{\partial y} - p \frac{\partial v}{\partial y}.$$

From these, eliminating U by cross differentiation, we get:

$$\frac{\partial C_x}{\partial x}\frac{\partial T}{\partial y} - \frac{\partial C_y}{\partial y}\frac{\partial T}{\partial x} + (C_x - C_y)\frac{\partial^2 T}{\partial x \partial y} = \frac{\partial(p,v)}{\partial(x,y)} \qquad (1)$$

From the second law of thermodynamics, introducing the integrating factor $1/T$ and the entropy S, we have

$$\frac{dQ}{T} = dS = \frac{\partial S}{\partial x}\,dx + \frac{\partial S}{\partial y}\,dy = \frac{C_y}{T}\frac{\partial T}{\partial x}\,dx + \frac{C_x}{T}\frac{\partial T}{\partial y}\,dy$$

from which we get in a similar manner:

$$\frac{\partial C_x}{\partial x}\frac{\partial T}{\partial y} - \frac{\partial C_y}{\partial y}\frac{\partial T}{\partial x} + (C_x - C_y)\frac{\partial^2 T}{\partial x \partial y} = \frac{(C_x - C_y)}{T}\frac{\partial T}{\partial x}\frac{\partial T}{\partial y} \qquad (2)$$

By a combination of these two equations we get

$$(C_x - C_y)\frac{\partial T}{\partial x}\frac{\partial T}{\partial y} = T\frac{\partial(p,v)}{\partial(x,y)}$$

which is equation (3) of my paper quoted above.

These are all the equations that we can get from the laws of thermodynamics and since any function has two partial derivatives for C_x and C_y, there will be four, which to be sure are connected by two equations. It is, I think, obvious that two equations are not sufficient. At any rate, let us proceed to an examination of the equation of state suggested by M. LeChatelier. If we have:

$$x = p,\ y = v, \qquad T = pf(v) = xf(y),$$

we at once obtain

$$\frac{\partial T}{\partial x} = f(y), \quad \frac{\partial T}{\partial y} = xf'(y), \quad \frac{\partial^2 T}{\partial x \partial y} = f'(y)$$

And accordingly in the equation of the first law we have

$$xf'(y)\frac{\partial C_x}{\partial x} - f(y)\frac{\partial C_y}{\partial y} + (C_x - C_y)\,f'(y) = 1. \qquad (3)$$

and in the equation of the second law

$$xf'(y)\,\frac{\partial C_x}{\partial x} - f(y)\,\frac{\partial C_y}{\partial y} + (C_x - C_y)f'(y) = (C_x - C_y)f'(y). \qquad (4)$$

We accordingly obtain the relations between the two specific heats:

$$xf'(y)\,\frac{\partial C_x}{\partial x} - f(y)\,\frac{\partial C_y}{\partial y} = 0, \qquad (5)$$

$$C_x - C_y = \frac{1}{f'(y)}. \qquad (6)$$

From (6) we get

$$\frac{\partial C_y}{\partial y} = \frac{\partial C_x}{\partial y} + \frac{f''}{f'^2}. \qquad (7)$$

Substituting this value in equation (5) we obtain,

$$x\frac{f'}{f}\frac{\partial C_x}{\partial x}-\frac{\partial C_x}{\partial y}=\frac{f''}{f'^2}, \tag{8}$$

a partial differential equation of the first order for C_x, which is of the form

$$P\frac{\partial z}{\partial x}+Q\frac{\partial z}{\partial y}=R$$

if we put,

$$C_x=z,\quad P=x\frac{d\log f(y)}{dy},\quad Q=-1,\quad R=-\frac{d}{dy}\left(\frac{1}{f'}\right).$$

This is immediately integrated by Lagrange's method, if we write

$$\frac{dx}{P}=\frac{dy}{Q}=\frac{dz}{R},\quad \frac{dx}{x\dfrac{d\log f}{dy}}=-dy=-\frac{dz}{\dfrac{d}{dy}\left(\dfrac{1}{f'}\right)}.$$

We obtain two particular integrals, in which we are to put

$$u\equiv xf=T=\text{const.},\quad v\equiv z-\frac{1}{f'}=\text{const.},\quad v=\varphi(u),$$

where φ is an arbitrary function. We thus obtain finally

$$C_p=1/f'(v)+\varphi(T),\quad C_v=\varphi(T) \tag{9}$$

and we find that neither of the specific heats is constant or independent of the pressure, nor is their difference constant, although it is independent of the pressure. Such a gas has no cohesion pressure, but does not in general have a zero Joule-Kelvin effect. If, on the contrary, we take a gas subject to the equation,

$$T=vg(p)$$

which has a zero Joule-Kelvin effect, we have a similar conclusion, interchanging p and v.

If, on the other hand, one of our variables is the absolute temperature, we are to write,

$$dQ=C_xdT+L_xdx \tag{10}$$

where L_x is the latent heat for constant x. We then obtain from the first law

$$\frac{\partial U}{\partial x}=L_x-p\frac{\partial v}{\partial x},\quad \frac{\partial U}{\partial T}=C_x-p\frac{\partial v}{\partial T},$$

$$\frac{\partial C_x}{\partial x}-\frac{\partial L_x}{\partial T}=\frac{\partial(p,v)}{\partial(x,T)}, \tag{11}$$

and from the second,

$$\frac{\partial S}{\partial x}=\frac{L_x}{T},\quad \frac{dS}{dT}=\frac{C_x}{T}$$

$$\frac{\partial C_x}{\partial x}-\frac{\partial L_x}{\partial T}=-\frac{L_x}{T}, \tag{12}$$

and combining the two,

$$\frac{L_x}{T} = -\frac{\partial(p, v)}{\partial(x, T)} \tag{13}$$

which is the generalized Clapeyron-Clausius equation. Making use of this in (11) we obtain the "well-known equation" mentioned by Professor Lewis, and given by van der Waals, p. 52:

$$\frac{\partial C_v}{\partial v} = T\frac{\partial^2 p}{\partial T^2}$$

Applying this to the case mentioned by Professor Lewis,

$$p = \varphi_1(v) + T\varphi_2(v)$$

which is the case under which van der Waals' familiar equation comes, we find, to be sure, that C_v is independent of the *volume*, but on solving for T, and writing

$$T = pV_1(v) + V_2(v)$$

and applying equations (1) and (2), we do not find anything particularly simple for C_p.

All this is very simple, but what I wish to call attention to is that in any case, the specific heats being defined by a partial differential equation of the first order, there is one perfectly arbitrary function, by no means necessarily of the temperature. In any case I believe that the teaching of thermodynamics by means of the equation of the so-called ideal gas or by means of van der Waals' equation is by no means conducive to clearness.

ANOMALOUS OSMOSE

By F. E. Bartell

Chemical Laboratory, University of Michigan*

It has repeatedly been noted that solutions of electrolytes with different types of "semi-permeable" membranes give osmotic pressure values quite different in magnitude from the calculated maximum values derived from the gas law formulations. We may define *normal osmotic pressure values* as those which conform to the gas law formulations. We have an example of this type when a solution of cane sugar is used with a semi-permeable membrane such as copper ferrocyanide. *Anomalous osmotic pressure values* are those which do not conform to the gas law formulations; they may be greater or less than the normal values. The abnormality of the effects may be of considerable magnitude and may even result in so-called negative osmose, in which case the direction of flow of liquid is from the concentrated to the more dilute solution.

* Communicated by M. Gomberg, April 20, 1920

Anomalous osmose has been observed with membranes of inorganic materials, such as unglazed porcelain [1] and finely divided metals;[2] with membranes of plant tissue, such as parchment paper;[3] with membranes of animal tissue, such as gold beaters skin,[4] and with membranes of organic gelatinous materials, such as gelatine and collodion.[5] The writer has investigated each of these types of membranes and has noted their behavior when used with different types of solutions. Measurements have included osmotic pressure, the sign of the charge which exists between the membrane material and the solution, and the difference in potential which exists between the two solutions in contact with the faces of the membrane.

In order to account for abnormal osmotic effects, the following postulates have been made:

1. Capillary pores connect the two faces of an osmotic membrane. These pores become filled with liquid from the solutions bathing the membrane faces.

2. An electrical double layer may exist between the walls of the capillary pores and the liquid within them (i.e., the Helmholtz electrical layer). The magnitude of the electrical double layer depends upon the extent of the selective adsorption of the ions.

3. A difference of potential may exist between the two faces of the membrane. The magnitude of this value is primarily dependent upon diffusion, relative ionic migration velocities, and relative ionic adsorption.

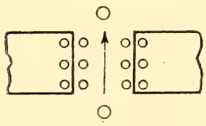
4. The forces tending to produce anomalous osmose operate in practically the same manner as do the forces in the phenomena of electrical osmose. The passage of liquid through the membrane is, in each of these cases, due to the electrical potential which is operative between the two faces of the membrane and which acts upon the charged liquid layer along the capillary wall.

5. In an osmotic system there is always operative a force tending to produce normal positive osmose. When solutions of electrolytes are used there may exist, in addition to this normal force, another force acting either with or against it, with the result that observed osmotic pressures may be either abnormally great or abnormally small.

Electrical States Which May Be Associated with the Membrane System.—According to the postulates above outlined, nine different states of electrification may exist within the osmotic system. Each of the following diagrams represents the electrical influences associated with a single pore extending through a membrane; this includes the sign of the electrostatic charge on the pore wall with the corresponding opposite charge of the liquid layer bathing this pore wall, together with the orientation of the electrical potential existing between the two faces of the membrane. The concentrated solution is in every case represented on the upper side. In each case the arrow at the left pointing upward indicates the direction of the tendency to produce normal osmose, while the arrow on the right indicates the direction of the superimposed effect tending to produce abnormal osmose. With electrical orientations represented in cases I, II, III, IV and VII, normal osmose would be obtained; with electrical orientations represented in cases V and IX, abnormally great positive osmose would be obtained; with electrical orientations represented in cases VI and VIII, abnormally small (or negative) osmose would result.

An indication of the orientation of the electrical charge, and the magnitude of the charge, on the capillary wall system has been obtained by reducing the material of the membrane to the form of a fine suspension and then determining the electrical sign of these particles by cataphoresis, i. e., by noting the direction of movement of the fine particles when subjected to the driving force of a difference of potential.

I



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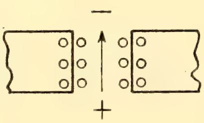

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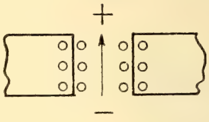

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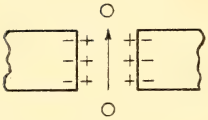

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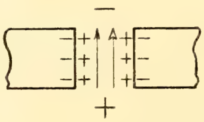

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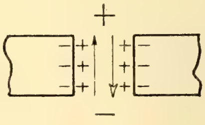

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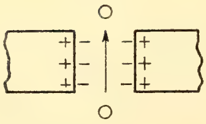

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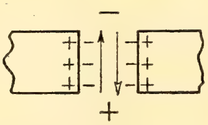
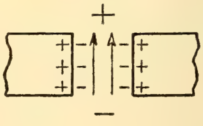


IX



The difference in potential between the two solutions bathing the faces of the membrane has been measured in practically all our osmotic experiments. This value has been assumed to represent the difference in potential which exists between the two faces of the membrane and to indicate at least the direction of the driving force tending to produce abnormal osmose.

It has been noted that the value of the electromotive force observed is quite different from the value calculated by assuming it to be due to contact potential. In some cases it is even opposite in sign to such calculated values.

In the following tables, the sign preceding the potential value refers to the sign of the charge on the solution side of the membrane. The (−)

sign preceding the osmose value indicates negative osmose. Some typical results are given in the following tables.

Results Obtained with Porcelain Membrane

SOLUTION	POTENTIAL OF CONCENTRATED SOLUTION SIDE	OSMOSE	SIGN OF CAPILLARY WALL	SIGN OF LIQUID IN CAPILLARY
0.1 M KNO_3	−0.006	+64.5	−	+
0.1 $LiNO_3$	+0.007	−31	−	+
0.1 $Al(NO_3)_3$	+0.310	+447	+	−
0.005 M HCl		+27		
0.01 M HCl	−0.011	+15	+	−
0.2 M HCl	+0.013	+119	+	−
0.005 M NaOH		+26		
0.01 M NaOH	−0.022	+81	—	+
0.2 M NaOH	+0.016	−45	−	+

Results Obtained with Gold Beaters Skin

SOLUTION	POTENTIAL OF CONCENTRATED SOLUTION SIDE	OSMOSE	SIGN OF CAPILLARY WALL	SIGN OF LIQUID IN CAPILLARY
0.05M KCl	+0.002	+ 8.5	+	−
0.05 M LiCl	+0.046	+ 11	+	−
0.05 M $AlCl_3$	+0.067	+550	+	−
0.1 HCl	−0.09	− 2	+	−
0.5 H_2SO_4	−0.094	− 40	+	−
0.5 H_3PO_4	−0.103	− 45	+	−
0.01 NaOH	+0.040	+ 5	−	+
0.5 NaOH	+0.04	− 5.5	−	+

Results Obtained with Potassium Salts with Porcelain Membranes

SOLUTION	POTENTIAL OF CONCENTRATED SOLUTION SIDE	OSMOSE	SIGN OF CAPILLARY WALL	SIGN OF LIQUID IN CAPILLARY
KCNS	−0.0185	+100.5	−	+
KCl	−0.0105	+ 80	−	+
KBr	−0.0090	+ 71	−	+
KNO_3	−0.0060	+ 64.5	−	+
KI	−0.0050	+ 55	−	+

The results obtained show that the magnitude of the osmose varies with the magnitude of the potential. The magnitude of osmose will not vary directly with the magnitude of potential inasmuch as the electromotive force values must overcome the tendency toward positive osmose, and, also, the value of the double electric layer of the capillary wall system is different in every solution used.

Gold beaters skin in water is slightly electro-negative. Salts with univalent cations may cause the membrane material to become positively charged. The magnitude of the charge on the capillary walls increases with increase in valency of the cation. This explains the abnormally great osmose with $AlCl_3$ solution.

It is noted that the direction and the extent of osmose in the above experiments conform to predictions which follow from the application of the postulates given above. In those cases in which the direction of flow is not that actually indicated by the sign of the charge of the system, it will be noted that the osmose is abnormal and that the tendency is operative in the direction predicted.

The experiments with various types of membranes have been extended considerably beyond those cited. Parchment paper, gold beaters skin, collodion, and gelatine membranes have been studied. The generalizations outlined above have been found to apply very satisfactorily in the case of each of these types of membranes. The results will be published in the near future.

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[1] Bartell, F. E. and Hocker, C. D., *J. Amer. Chem. Soc.*, **38,** 1916 (1036).

[2] Robinson, C. S., *J. Phys. Chem.*, **22,** 1918 (99); **22,** 1918 (153).

[3] Unpublished data.

[4] Girard, P., *Rev. Gen. Sci.*, **20,** 1909 (694); Lillie, R. S., *Amer. J. Physiol.*, **28,** 1911 (194).

[5] Loeb, J., *J. Gen. Physiol.*, **2,** 1919 (87).

A PHOTOGRAPHIC METHOD OF FINDING THE INSTANTANEOUS VELOCITY OF SOUND WAVES AT POINTS NEAR THE SOURCE

By ARTHUR L. FOLEY

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Communicated by R. A. Millikan, April 10, 1920

Sparks from the condensers L, L (Leyden jars, charged by an electric influence machine not shown in the figure) pass across the gaps G, G, the sound gap S, and the illuminating gap I. The light from the spark at I casts a shadow (caused by refraction) of the sound wave produced by the spark at S upon the photographic dry plate P, provided the spark at I occurs at the proper time. The gaps I and S are in series, the spark at S occurring first, the spark at I being retarded the proper time interval by means of an adjustable condenser K.

D is a steel disk 76 cm. in diameter mounted to rotate at high speed just in front of and near the lower edge of the dry plate P. The rim of the disk was ground to a chisel edge and very small teeth were cut in it at intervals of a cm. or so. The intervals and teeth were made irregular to insure the identification of their shadows on the photographic plate. The rotational speed of the disk was measured by means of a calibrated Van Sicklen-Elgin tachometer. A light-tight box encloses the spark gaps I and S, the dry plate P and the disk D.

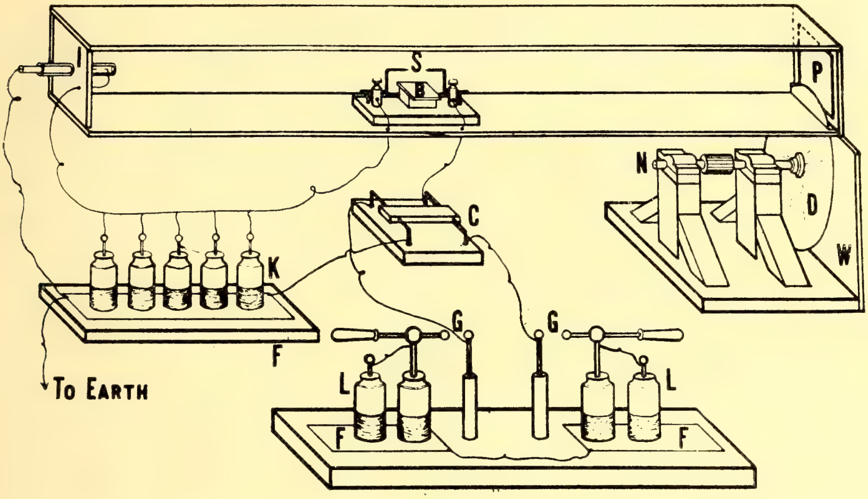

FIG. 1

When a spark occurs at S a cylindrical sound wave is generated and a shadow of the saw teeth is thrown on the photographic plate. Assuming that the time interval between S and I has been adjusted properly the spark at I casts on the dry plate a shadow of the sound wave and the teeth

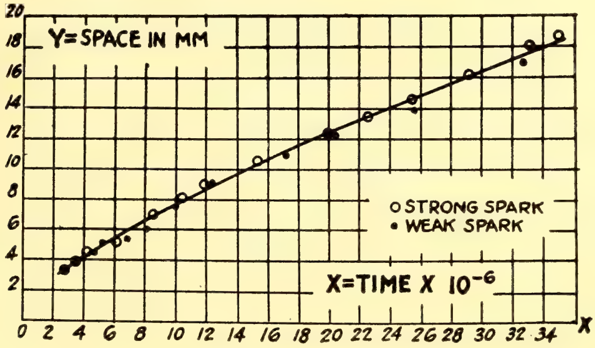

FIG. 2

of the disk, the latter having moved into a new position during the interval between the sparks. The true radius of the sound wave is calculated from the geometry of the figure and the radius as measured on the photographic plate. The time interval is calculated from the measured distance through which any given tooth has turned, together with the speed of rotation and the distances from the plate to I, S, and the disk D. Plotting wave radii as ordinates and the corresponding time intervals as abscissas, the tangent to the curve at any point gives the instantaneous velocity of sound at that distance from the source. The accompanying curve shows that the velocity varies from about 660 meters per sec. at a distance of 3.2 mm. from the sound source to 380 meters per second at a distance of 1.8 cm. Results have been obtained since the above curve was plotted for points much closer to the source and for distances up to a half meter.

The writer designates as "weak sparks" those produced when each condenser, L, consists of two Leyden jars; as "strong sparks," seven jars. The curve appears to show but little difference in the velocity of the resulting sound waves. However, considering the shortness of the sound pulses produced by such electric sparks, all are really intense waves at points near the source.

This investigation, made under a grant from the American Association for the Advancement of Science, will be published in full in the *Physical Review.*

CONFORMAL MAPPING OF A FAMILY OF REAL CONICS ON ANOTHER

BY T. H. GRONWALL

TECHNICAL STAFF, OFFICE OF THE CHIEF OF ORDNANCE, WASHINGTON, D. C.

Communicated by E. H. Moore, April 27, 1920

Note IV On Conformal Mapping Under Aid of Grant No. 207 From the Bache Fund

Let $z = x + yi$ and $w = u + vi$ be two complex variables, and let the analytic function $w = w(z)$ define a conformal map of the z-plane upon the w-plane. It is the purpose of the present note to determine all functions $w(z)$ such that there exists a family (containing at least one real parameter) of real conics in the z-plane which is mapped upon a family (obviously with an equal number of parameters) of real conics in the w-plane. The particular case when the conics in the w-plane are straight lines parallel to the real axis has been investigated by Von der Mühll[1] and Meyer.[2]

It is convenient to use the isometric coördinate $z = x + yi$ and $\bar{z} = x - yi$, and we begin by establishing the following general results:

When $\bar{z} = f(z, t)$, where f is analytic in both its arguments and t is a real parameter, represents a family of real curves in the z-plane, and similarly $\bar{w} = F(w, t)$ is a family of real curves in the w-plane, the necessary and sufficient condition that $w = w(z)$ shall map these two families upon each other is that the relation

$$\frac{\frac{\partial F}{\partial w}}{\frac{\partial F}{\partial t}}\frac{dw}{dz} = \frac{\frac{\partial f}{\partial z}}{\frac{\partial f}{\partial t}}$$

shall become an identity in z and t upon making $w = w(z)$.

In order that the z- and w-planes may be mapped upon an auxiliary Z-plane in such a manner that our two families of curves are mapped into the parallels to the real Z-axis, it is necessary and sufficient that the right-hand member in the relation above equals the product of a function of z by a function of t. When this is not the case, and our two families of curves are both algebraic, then $w(z)$ is necessarily an algebraic function of z.

Applying these general propositions to the various kinds of real conics, we obtain all the conformal maps having the required property, and replacing z and w by $az + b$ and $\alpha w + \beta$, respectively, or by $(az + b)/(cz + d)$ and $(\alpha z + \beta)/(\gamma z + \delta)$ when the conics involved are circles, and interchanging z and w when necessary, we may reduce our conformal maps to the following types, arranged according to the number of parameters in the families of conics:

Five parameters (one type):

$$w = z; \tag{I}$$

any conic in the z-plane is transformed into the same conic in the w-plane.

Three parameters (two types):

$$w = 1/z; \tag{II}$$

any circle or straight line is transformed into a circle or straight line

$$w = z^2; \tag{III}$$

denoting the parameters by k, r and θ, any ellipse $(k>1)$ or hyperbola $(1>k>-1)$

$$\bar{z} = k\frac{\bar{z}_1}{z_1}z + \frac{\bar{z}_1}{z_1}\sqrt{(k^2 - 1)(z^2 - z_1^2)}$$

with center at the origin and foci at

$$z_1 = re^{\theta i}, z_2 = -z_1$$

is transformed into the ellipse $(k>1)$ or hyperbola $(1>k>-1)$

$$\bar{w} = K\frac{\bar{w}_1}{w_1}w + \frac{1 - K}{z}\bar{w}_1 + \frac{\bar{w}_1}{w_1}\sqrt{(K^2 - 1)w(w - w_1)}$$

with $K = 2k^2-1$ and foci at

$$w_1 = z_1^2 = r^2e^{2\theta i}, w_2 = 0.$$

In the special case $k = 0$, the equilateral hyperbolas

$$e^{-2\theta i}z^2 + e^{2\theta i}\bar{z}^2 = r^2$$

with center at the origin go into the straight lines

$$e^{-2\theta i}w + e^{2\theta i}\bar{w} = r^2$$

Two parameters, denoted by r, θ or p, θ (four types):

$$w^2 = z^2 - 1; \qquad \text{(IV)}$$

any equilateral hyperbola

$$e^{-2\theta i}z^2 + e^{2\theta i}\bar{z}^2 = r^2$$

with center at the origin and foci at $z_1 = re^{\theta i}$, $z_2 = -z_1$ is transformed into the equilateral hyperbola

$$e^{-2\theta i}w^2 + e^{2\theta i}\bar{w}^2 = r^2 - 2\cos 2\theta$$

with center at the origin and foci at

$$w_1 = \sqrt{r^2 - 2\cos 2\theta}\, e^{\theta i},\ w_2 = -w_1.$$

In the special case $r^2 = 2\cos 2\theta$, we have a pair of perpendicular straight lines through the origin in the w-plane.

$$w = z^2; \qquad \text{(V)}$$

any straight line

$$e^{1/_2\theta i}\,\bar{z} - e^{-1/_2\theta i}z = i\sqrt{p}$$

is transformed into the parabola

$$e^{1/_2\theta i}\sqrt{\bar{w}} - e\sqrt{\,}^{1/_2\theta i}w = i\sqrt{p}$$

with focus at the origin and at a distance of $^1/_2p$ from the directrix, θ being the angle between the axis of the parabola and the real axis in the w-plane.

$$w = (z^2 - c_1)^2,\ \text{where}\ c_1 = 0\ \text{or}\ 1; \qquad \text{(VI)}$$

any equilateral hyperbola

$$e^{1/_2\theta i}\,\bar{z}^2 - e^{-1/_2\theta i}z^2 = i(\sqrt{p} + 2c_1 \sin {}^1/_2\theta)$$

with center at the origin and foci at

$$z_1 = e^{1/_4(\theta - \pi)i}(\sqrt{p} + 2c_1 \sin {}^1/_2\theta)^{1/_2},\ z_2 = -z_1$$

is transformed into the parabola

$$e^{1/_2\theta i}\sqrt{\bar{w}} - e^{-1/_2\theta i}\sqrt{w} = i\sqrt{p}$$

with focus at the origin.

$$w = (\sqrt{z} - 1)^2; \qquad \text{(VII)}$$

any parabola

$$e^{1/_2\theta i}\sqrt{\bar{z}} - e^{-1/_2\theta i}\sqrt{z} = i(\sqrt{p} + 2\sin {}^1/_2\theta)$$

with focus at the origin is transformed into the parabola

$$e^{1/_2\theta i}\sqrt{\bar{w}} - e^{-1/_2\theta i}\sqrt{w} = i\sqrt{p}$$

with focus at the origin.

One parameter, denoted by r, θ or p (nine principal types, the linear composition of each of these with itself or with another giving a total of forty-five types). There are tabulated below the nine functions which map the one parameter families of conics specified on the parallels to the real axis in the Z-plane. When $\varphi_1(z)$ and $\varphi_2(z)$ are any two of these functions (identical or different), all maps transforming one parameter families of conics into others are found by making

$$\varphi_2(w) = A\varphi_1(z) + B,$$

where A and B are constants and A real, and then making the linear transformations on z and w indicated at the beginning of this summary.

$$Z = z; \qquad \text{(VIII)}$$

all parallels to the real axis (special case of I).

$$Z = \log z; \qquad \text{(IX)}$$

all straight lines through $z = 0$ (the circles with center at $z = 0$ go into parallels to the imaginary Z-axis).

$$Z = i \log z; \qquad \text{(X)}$$

all circles with center at $z = 0$ (the straight lines through $z = 0$ go into parallels to the imaginary Z-axis).

$$Z = z^2; \qquad \text{(XI)}$$

all equilateral hyperbolas $z^2 + \bar{z}^2 = r^2$ with center at the origin and foci on the real axis (special case of III).

$$Z = \log (z^2-1); \qquad \text{(XII)}$$

all equilateral hyperbolas

$$e^{-2\theta i}z^2 + e^{2\theta i}\bar{z}^2 = 2 \cos 2\theta$$

with center at the origin and foci $\pm\sqrt{2 \cos 2\theta}\, e^{\theta i}$ (which are the end-points of the diameter with angle of slope θ of the lemniscate with foci at $+1$ and -1). These hyperbolas all pass through the foci of the lemniscate.

$$Z = \log (z + \sqrt{z^2-1}) \text{ or } z = {}^1/_2(e^Z + e^{-Z}); \qquad \text{(XIII)}$$

all hyperbolas with foci at $+1$ and -1 (all ellipses with these foci go into parallels to the imaginary Z-axis).

$$Z = 1/i \log (z + \sqrt{z^2-1}) \text{ or } z = \cos Z; \qquad \text{(XIV)}$$

all ellipses with foci at $+1$ and -1 (all hyperbolas with these foci go into the parallels to the imaginary Z-axis).

$$Z = \sqrt{z}; \qquad \text{(XV)}$$

all parabolas $\sqrt{\bar{z}}-\sqrt{z} = i\sqrt{p}$ with focus at the origin and the real axis as axis (special case of V).

$$Z = \log (\sqrt{z}-1); \qquad \text{(XVI)}$$

all parabolas

$$e^{{}^1/_2\theta i}\sqrt{\bar{z}} - e^{-{}^1/_2\theta i}\sqrt{z} = 2i \sin {}^1/_2\theta$$

with focus at the origin and passing through the point $z = 1$.

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[1] Von der Mühll, *J. Math., Berlin*, **69**, 1868 (264).

[2] Meyer, "Ueber die . . . Isothermen," *Dissertation*, Göttingen, 1879.

EXPERIMENTS ON THE VIBRATION OF AIR IN CONICAL HORNS

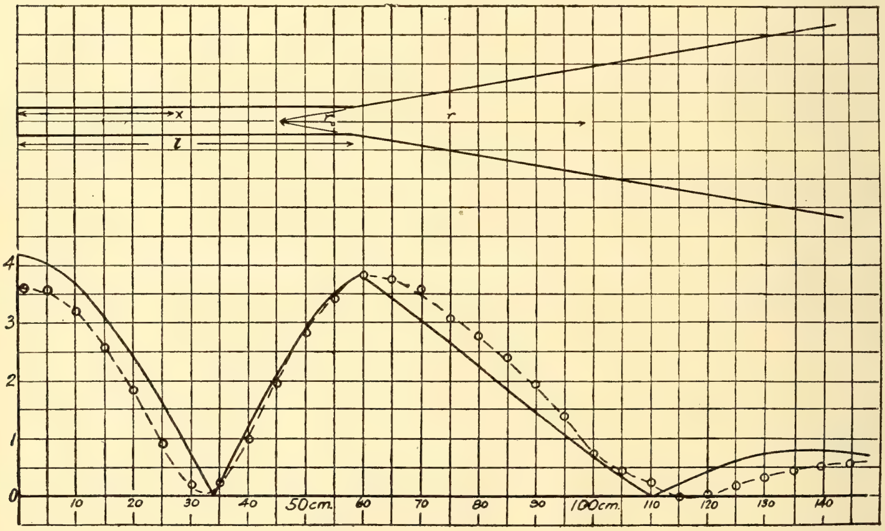
By Arthur Gordon Webster

Clark University, Worcester, Mass.

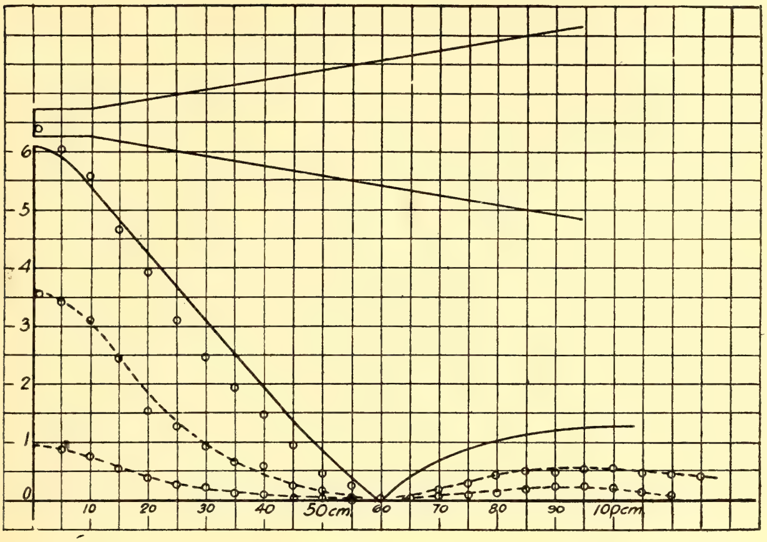
Communicated March 30, 1920

At the meeting of the Committee on Sound of the National Research Council recently held at Geneva, Illinois, it was represented to me that my habit of not publishing results had been a serious detriment to the progress of certain investigators in sound and I was strenuously urged to reform my habits. I shall, therefore, take the liberty of bringing out of my drawers a certain number of papers some of which I have had for many years but which for one reason or another have not been published.

The curves presented herewith were shown at a meeting of the American Association for the Advancement of Science in Columbus in 1915, but I believe have not been published. They were made by means of the phonometer and phone described in my paper in these Proceedings. **5**, May, 1919 (163–166), and they are intended to verify the theory of horns given by me in these Proceedings, **5**, July, 1919 (275–283).

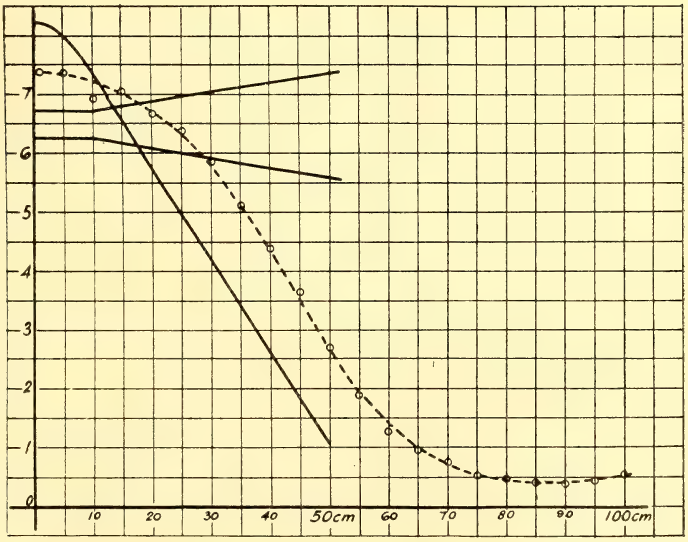


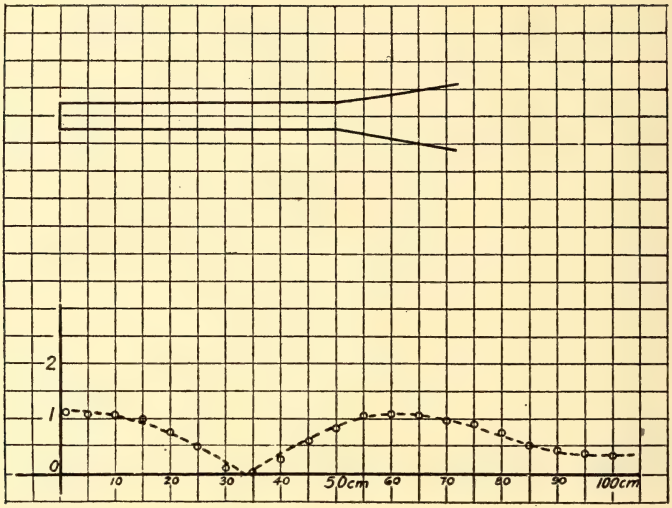
The horn is always a portion of a circular cone mounted on one end of a cylindrical tube, the other end of which is closed. The sound was made by the phone emitting a constant sound of a pitch of 256 per second. The standing waves inside the horn were explored by an antenna consisting of a glass tube of three millimeters internal diameter and several feet long attached to a disk closing the end of a cylindrical tube screwed into the opening of the phonometer. This could be put into any part of



the space to be explored. To be sure, the pressure measured by the phonometer is not the same as that of the antenna, but inasmuch as standing waves are formed in the antenna the pressure measured by the phonometer is proportional to that at the end of the antenna and, therefore, to that in the place to be observed.

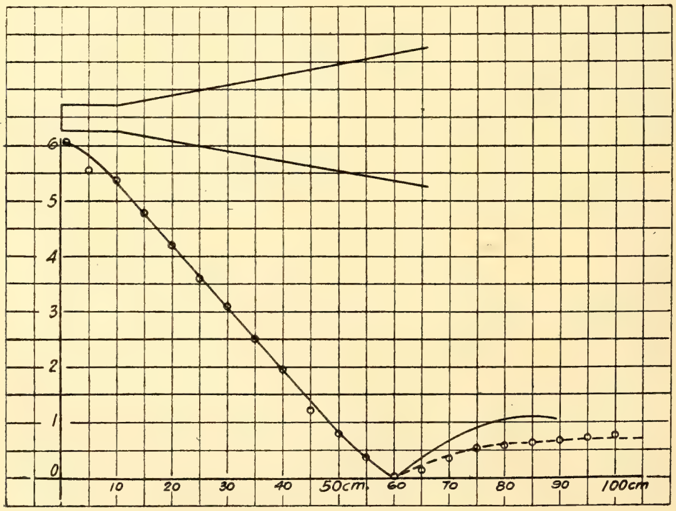
The figures printed herewith show on the dotted lines the actual observations and on the full lines the theory. The shape and dimensions of the

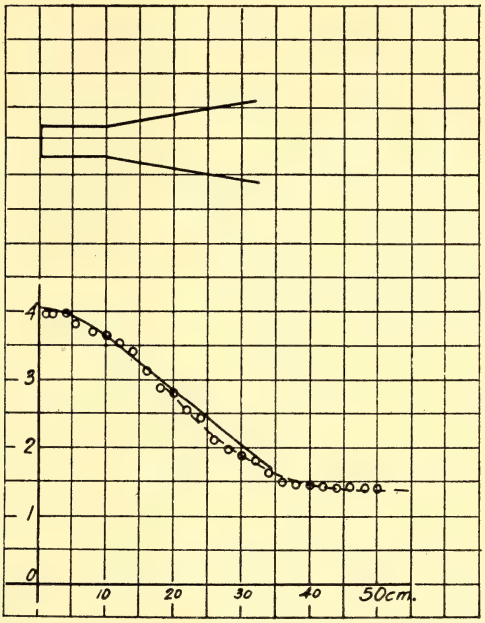




horns are also shown on the figures. The experiments were made by my then assistant, Dr. H. F. Stimson, to whom I am indebted for his careful work.

It will be seen that the theory is very substantially verified, the defect in it being due to the very rough estimate made about the correction of the open end which, as I stated in my paper on horns, is far from being accurate. But inasmuch as it is the only theory that has ever been given I feel that the results constitute a distinct advance. Prof. G. E. Stewart has done a large amount of work on horns and he also confirms my theory

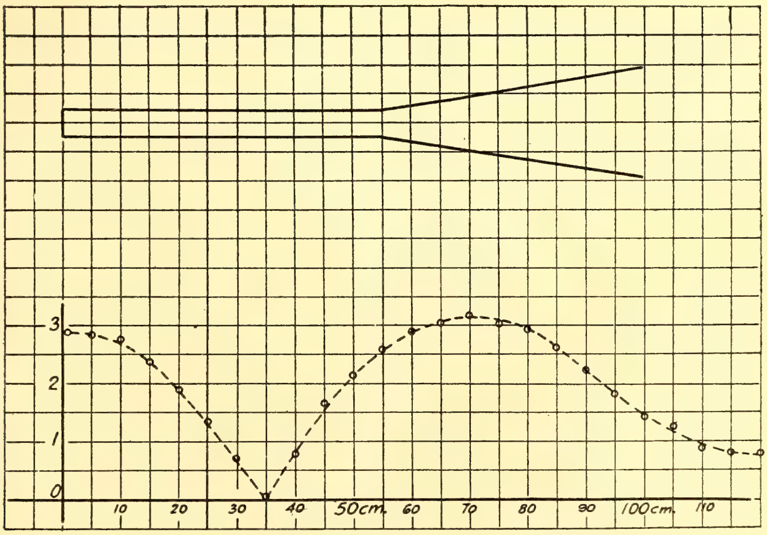




with about the same degree of accuracy as it was confirmed by these results more than four years ago. Professor Stewart has also given a theoretical treatment that is an improvement on mine. We shall see whether it agrees better with the observations. The formulae I have used in calculating these curves are:

In cylinder
$$p = \frac{P \sin \beta \cos kx}{r_0 \cos kl},$$

In cone
$$p = P \sin k(r - r_0 + \beta)/r,$$



$$P = p_o r_o / \sin\beta, \quad \tan k\beta = 1/(\text{ctn}\, k\alpha - \tan kl), \quad \tan k\alpha = kr_o.$$

I enclose the following errata in my article on horns cited above:

PAGE	EQUATION	FOR	READ
276	4	$\frac{k}{2V}i$	$\frac{k}{2\pi}i$
277	13	uv	uv
278	17	$uv, \alpha v$	$uv, \alpha v$
278	19–20	d	∂
281	above 36	$Z_1 = ck^2\{$	$Z_1 = ek^2\{$
281	after 37	$H.K.$	$H.F.$
282		$\frac{d^2p}{vn^2} + m\frac{dp}{vn} + p = 0,$	$\frac{\partial^2 p}{\partial x^2} - m\frac{\partial p}{\partial x} + k^2 a = 0$
		$p = e^{-\sqrt{4-k^2x}}\|A\cos kx + B\sin kx\|$	$p = e^{\frac{mx}{2}}\{A\cos k\sqrt{1-(m/2k)^2}.x$ $+ B\sin k\sqrt{1-(m/2k)^2}.x\}$
		$x = e^{-\sqrt{4-k^{22}}}\|C\cos kn + Dmk^2\|$	$X = e^{\frac{mx}{2}}\{C\cos k\sqrt{1-(m/2k)^{2}.x}$ $+ D\sin k\sqrt{1-(m/2k)^2}.x\}$

THE RELATIVE IMPORTANCE OF HEREDITY AND ENVIRONMENT IN DETERMINING THE PIEBALD PATTERN OF GUINEA-PIGS

By Sewall Wright

Bureau of Animal Industry, United States Department of Agriculture

Communicated by R. Pearl, March 17, 1920

The Bureau of Animal Industry has been carrying on an experiment on the effects of inbreeding on guinea-pigs since 1906. Twenty-three families were started successfully from as many pairs and were maintained wholly by matings of brother with sister. Another stock from the same source has been maintained as a control, without mating even second cousins.

A number of color variations were present in the original stock and most of them kept appearing during the early history of each family as the result of Mendelian segregation. After a number of generations, however, a certain color became fixed automatically within each inbred line. In some cases an entire family came to produce only one color. In most cases, the families became broken into a number of sub-families, each characterized by a particular color.

The original stock consisted largely of tricolors. Conspicuous differences in the average amounts of white and yellow appeared among the inbred families from the first, proving the existence of hereditary differences in pattern. Contrary, however, to the case of the qualitative differences in color, no types of pattern ever became at all well fixed. Variations covering almost the entire range from solid color to solid white can be found in each line. Even after twenty generations of inbreeding, a guinea-pig with 20% of white in the coat may have a litter mate with as much as 90% of white.

As a supplement to more direct attempts to determine the cause of these variations in the pattern, it seemed desirable to find the relative importance of heredity and environmental factors.

The pattern of tricolors is really a combination of two patterns which are, in the main, inherited independently of each other, although obviously associated in development. These are the piebald pattern, consisting of colored spots on a white ground, and the tortoiseshell pattern consisting of black spots on a yellow ground. In a tricolor, a certain pattern of color and white is determined irrespective of the quality of the color. Another group of factors determines which spots shall be black (or one of the modifications of black, such as agouti, sepia or brown) and which shall be yellow or red.

A cross between a homozygous solid black and a tricolor of one of the inbred families produces black young with no yellow spots and only a very small amount of white, if any. On back-crossing with the tricolor stock, four kinds of young are produced in approximately equal numbers, viz., solid black, black and yellow tortoiseshells, black and white piebalds, and black, yellow and white tricolors. As in the first generation, there is apt to be a small amount of white in the first two classes. The nearly solid black type has been back-crossed for seven generations with pure tricolor stock, producing young with as much as 255/256 of the blood of the latter stock, with the same results as in the first back-cross, four classes of young in equal numbers. It is clear that two independent Mendelian factors are involved. One of the factors is completely dominant over that for tortoiseshell, the other is nearly but not quite fully dominant over that for piebald. In the stocks considered in the present paper both of these dominant factors were absent, leaving all of the animals tricolor except for the occasional somatic bicolors and black-eyed whites to be expected in a stock with a high average amount of white in the coat. The hereditary factors to be considered here, those by which one tricolor differs from another, are of the kind frequently known as modifiers. Crosses between inbred families at opposite extremes in amount of white have resulted in offspring which are almost exactly intermediate on the average. In the second generation, there is only a little more variability than in the first; much less than would be present if there were segregation of a single factor.

The minor as well as the major variations of the piebald and tortoiseshell patterns appear to be largely independent of each other, although at least one factor has been discovered which affects both, viz., sex. The males of all stocks have on the average some 7% more color than the females and also some 4% more black within the colored spots. In the present paper only variations in the piebald pattern will be considered and the sexes will be dealt with separately.

A record of the patterns of the guinea-pigs has been kept from the first by drawing the spots within a rubber-stamp outline. Only as much of the pattern as is visible from the back was recorded in this way before 1915. Since that date, the patterns of both the back and belly have been recorded, but separately, making comparison with the earlier records possible. The amount of white in each case has been estimated by means of a piece of tracing cloth on which the area in the stamped outline is divided into ten equal dorsal parts and ten equal ventral parts, including the legs. Each guinea-pig is given a record such as B-R-W 2–5–3/X–5–5, meaning 20% black, 50% red and 30% white on the back, a trace of black, 50% red and 50% white on the belly. In the present paper only the dorsal areas are considered. As the correlation between the amount of white on the back and belly is 0.86 and the back contains the most persistent center of color (the ear region) and almost the most persistent white (the nose) the grades, based on the dorsal part only, are believed to be practically as satisfactory as grades based on the entire coat.

Figure I shows the distribution of grades of white in three of the inbred families and in the control stock in 1916 to 1918. The representatives of

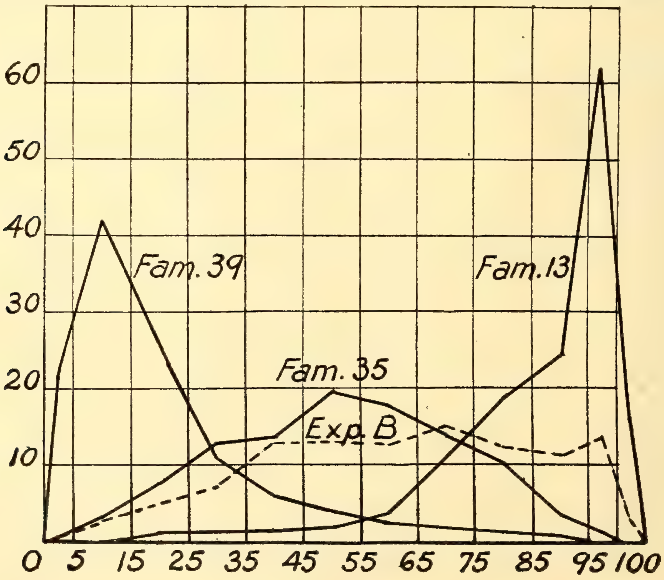


FIG. 1

Percentage distribution of grades of white in three inbred families (39, 35, 13) and in the control stock (B) during 1916–1918. The percentages with less than 5 per cent white and with 95 to 100 per cent white are given double value, the class ranges being of half size. The few solid whites in Family 13 and B are arbitrarily given double value.

family 39 during this period were all descended from a single mating in the fourth generation of inbreeding, and with a few exceptions, from one in the eighth. Family 35 was descended wholly from one mating in the seventh generation and largely from a single one in the twelfth. Family 13 was descended wholly from one mating in the second generation and largely from one in the seventh.

It will be seen that each family varies almost through the entire range, but that the mean values are markedly different. Family 39 has only about 20% white on the average, family 13 at the opposite extreme has about 85%, while family 35 has its average near 50%. On attempting to compare the variability of the families, a difficulty at once becomes apparent. The distribution curve of family 35 is practically symmetrical, but those of families 39 and 13 are markedly skew in opposite senses. This is not because of overstepping of the physiological limits, as there were no solid colored guinea-pigs in family 39, and only 8% black-eyed white in family 13. The curves appear to be compressed at the ends of the range, indicating that a unit of variation in these regions means more than in the middle of the range. It is especially difficult, so to speak, to remove the last bit of color, or the last bit of white. This phenomenon is shown by all of the stocks in which there is not an approximately equal average amount of color and white.

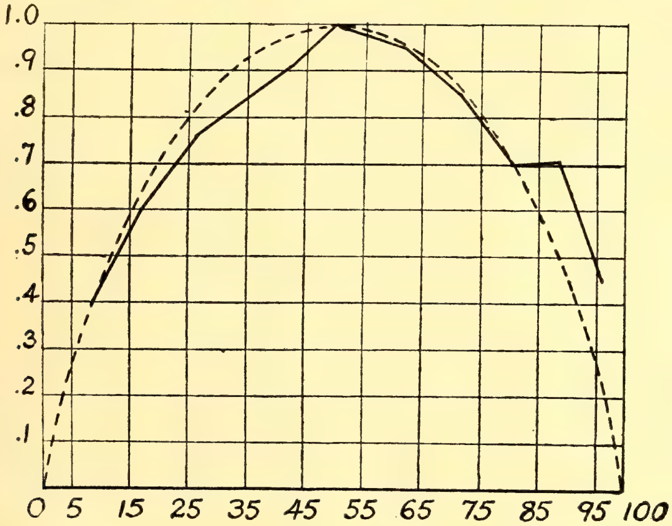


FIG. 2

The solid line shows the average distance between quartile and median in distribution curves from different inbred families in which the midpoint between quartile and median falls at different percentages of white. Based on 68 quartiles. The average quartile distance near 50 per cent white is taken as the unit. The broken line shows the expected quartile distances according to the hypothesis discussed in the text.

It is clear that measures of variability have little value unless the grades are properly corrected. Two methods have been tried for making this correction, one empirical, the other theoretical. The distribution of grades of white was tabulated, males and females separately, in the 17 families which were still on hand during 1916 and 1917. With a properly corrected system of grades the variability should be approximately equal in all cases. The points were found which divided each distribution into four equal parts, i.e., the median and two quartiles. The distance between the median and each quartile was then correlated with the grade midway between the two points. The average of the values which fall within one grade is shown in the solid line in figure 2. It will be seen that a given variation near the middle of the range corresponds to one about half as great near the ends.

An effect of this kind is to be expected on theoretical grounds. Although the skin of a piebald guinea-pig is divided sharply into areas in which pigment is either produced to the full amount characteristic of the animal, or is wholly absent, it is not to be supposed that the influences which at some critical period in ontogeny determine whether a region is to be colored or white, are so sharply alternative in themselves. It seems more reasonable to suppose that the sums of the favorable and unfavorable influences in different parts of the skin could be arranged in a graded series. Doubtless in certain white regions a slight difference in the conditions would have enabled color to develop, while in others, a great change would have been necessary. Similarly with colored areas. Suppose, then, that the skin is divided into a large number of equal areas and that it were possible

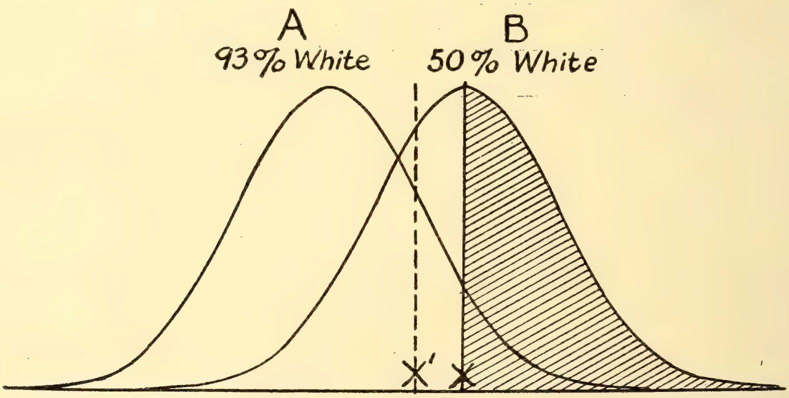


FIG. 3

The two curves are intended to represent the distribution of color potentialities among areas in the skins of two guinea-pigs. X is the critical potentiality, above which color is developed. The same change in average potentiality increases the amount of color in the guinea-pig with a large amount of white (A) only half as much as in the guinea-pig which is half white and half colored (B).

to determine the true potentiality of each area at the critical period in development. Let us assume that these potentialities are distributed, at least roughly, according to the normal probability curve.

In figure 3 variation in the piebald potentiality is measured along the horizontal axis. Curve A is intended to represent the number of areas with each potentiality in one guinea-pig, while curve B represents the same for another guinea-pig in which the tendency toward color production is higher as a whole. The critical potentiality is at X. All areas which exceed this produce color, while those which fall below, however slightly, remain white. Guinea-pig A is thus about 93% white, while B is about 50% white. It is easy to see that any cause which increases the potentiality of A and B by a given amount makes a greater change in the area of color in B than in A. From a table of probability integrals, one can find at once the change in the area of the curve, brought about by a unit change in the value of X, in the neighborhood of any given area. These results should be comparable with those obtained empirically for physiologically equivalent variations at different areas of white.

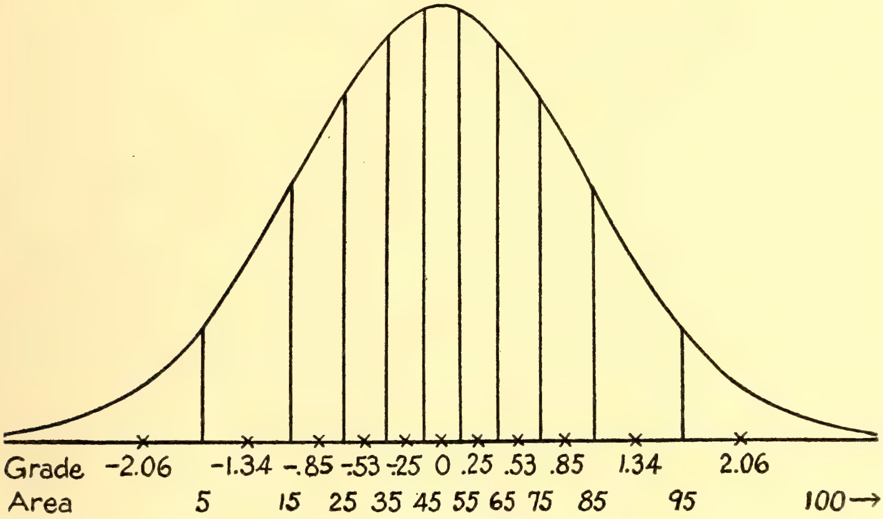


FIG. 4

A normal probability curve, the area of which is divided into tenths with the exception of the end areas which are twentieths. The deviations from the middle of the curve to the middle of each area (mean of the end areas), measured in terms of the standard deviation, are used as a corrected series of grades in place of the actual percentages of white in piebald guinea-pigs.

The theoretical curve, as shown by the dotted line in figure 2, does not fit the empirical curve perfectly, but appears to give a sufficiently accurate correction of the grades for practical purposes. Aside from its greater smoothness, the theoretical curve has the advantage that by its use the variability in the amount of color in a stock of guinea-pigs can be compared with the theoretical variability in the potentialities within a single average guinea-pig.

The relative importance of unit variations in area in different parts of the range is as the reciprocal of the variations which are physiologically equivalent. In figure 4 the area of a probability curve is divided into equal tenths except for the end areas, which are twentieths. The difference in potentiality corresponding to any difference in area can easily be estimated. The corrected series of grades as given in figure 4 has been made by finding the average of the deviations from the median of the curve to each of the class limits, with the exception that the deviation of the mean has been used for the end classes. A little consideration will show that the proper centering of the grades varies slightly with the mean of the population with which one is dealing. The grades as corrected here, are, however, sufficiently accurate for the present purpose.

TABLE 1

NUMBERS, MEANS AND STANDARD DEVIATIONS IN RANDOM-BRED STOCK AND INBRED FAMILY NO. 35 DURING 1916, 1917 AND 1918

	EXP. B (RANDOM-BRED)			FAMILY 35 (INBRED)		
	No.	Mean	σ	No.	Mean	σ
Sire.......	105	+0.337 ± 0.050	0.756 ± 0.035	73	−0.071 ± 0.044	0.559 ± 0.031
Dam......	107	+0.390 ± 0.054	0.824 ± 0.038	73	+0.220 ± 0.045	0.570 ± 0.032
Son.......	498	+0.273 ± 0.024	0.808 ± 0.017	235	−0.017 ± 0.026	0.593 ± 0.019
Daughter	488	+0.543 ± 0.024	0.796 ± 0.017	236	+0.140 ± 0.027	0.613 ± 0.019
Total Offspring	986	+0.407 ± 0.017	0.802 ± 0.012	471	+0.062 ± 0.019	0.603 ± 0.013

Compare with figure 4 for conversion of grades into percentages of white. The standard deviation of 0.603 in the inbred family means about 22% of the total dorsal area of the coat.

Two stocks were chosen for study of the relative importance of heredity and environment. One of these was the control stock (*B*) in which matings were made at random, except that even the mating of second cousins was avoided. The other was family 35, which since 1915 has been descended entirely from a single mating in the seventh generation of inbreeding and largely from a single mating in the twelfth generation. The records for 1916, 1917 and 1918 were used in both cases. Table 1 gives the number in each experiment, the means and standard deviations. Table 2 gives the correlations between parent and offspring, between litter mates and between the parents, weighted by the number of young. Inspection of the tables shows that sex makes no significant difference in the correlations. This is in agreement with the results of direct experiments. Reciprocal crosses between inbred families at the opposite extremes, as families 39 and 13 in figure 1, produced practically the same result, the progeny in both cases being almost exactly intermediate. Sex also makes no significant difference in variability. In the case of the means, however, there is in all cases the sex difference already mentioned. The mean and standard deviations are measured on a scale in which unity is the standard deviation of color potentialities for areas within the skin of a single average guinea-pig. It is interesting to find that even the random-bred guinea-pigs differ less among themselves in average color potentiality than do the spearate skin areas of a single animal.

TABLE 2

CORRELATIONS BETWEEN PARENTS, BETWEEN PARENT AND OFFSPRING AND BETWEEN LITTER MATES IN RANDOM-BRED STOCK AND INBRED FAMILY NO. 35, DURING 1916, 1917 AND 1918

	EXPERIMENT B (RANDOM-BRED)		FAMILY 35 (INBRED)	
	No.	Correlation	No.	Correlation
Sire-Dam	105	+0.019 ± 0.066	73	+0.029 ± 0.079
Sire-Son	492	+0.231 ± 0.029	235	+0.013 ± 0.044
Sire-Daughter	484	+0.194 ± 0.030	236	+0.082 ± 0.044
Dam-Son	498	+0.251 ± 0.028	235	+0.042 ± 0.044
Dam-Daughter	488	+0.165 ± 0.030	236	—0.080 ± 0.044
Average, Parent-Offspring	1962	+0.211 ± 0.015	942	+0.014 ± 0.022
Brother-Brother	390	+0.219 ± 0.033	182	+0.090 ± 0.050
Brother-Sister	437	+0.228 ± 0.031	203	+0.062 ± 0.047
Sister-Sister	406	+0.180 ± 0.032	194	+0.064 ± 0.048
Average, Litter Mates	1233	+0.214 ± 0.018	579	+0.069 ± 0.028

The matings in the correlation between sire and dam are each weighted by the number of offspring. The probable errors in this case are based merely on the number of matings. The probable errors in the other cases are based on the number of entries in the tables. Owing to repetition of individuals, they are probably somewhat too small. The correlation between litter mates in experiment B is based on 894 individuals in litters in which two or more were graded. The probable error based on this number is ±0.022 instead of ±0.018 as given in the table. Similarly there were 426 individuals in such litters in family 35, giving a probable error of ±0.031 instead of ±0.028.

The negligible correlations between the parents (+0.019 in the control stock and +0.029 in the inbred family) show that matings actually were made at random as far as color is concerned. In the control stock, both the average correlation between parent and offspring (+0.211) and that between litter mates (+0.214) are unquestionably significant. In the inbred family, there is virtually no correlation between parent and offspring (0.014) while that between litter mates is of doubtful significance (+0.069).

There is evidently very little genetic variability left in family 35, but is a good deal in the control stock. A closer analysis can be made by considering the relations between parent and offspring as shown diagrammatically in figure 5.

In a broad sense, the peculiarities of an individual are entirely determined by heredity and environment. In the present and doubtless most other cases, the latter should be divided into two elements. Nearly all tangible environmental conditions—feed, weather, health of dam, etc., are identical for litter mates. There may, indeed, be some differences in the blood supply, but in the main, differences which are not genetic must be due to irregularities in development due to the intangible sort of causes to which the word chance is applied. Variations which are due to inaccuracy in grading and are, therefore, merely apparent, cannot, however, be distinguished from those of this class. Checks have been made which indicate that apparent variability due to this cause is small in comparison with the large amount of variability which is found to be due neither to heredity nor to tangible environmental conditions.

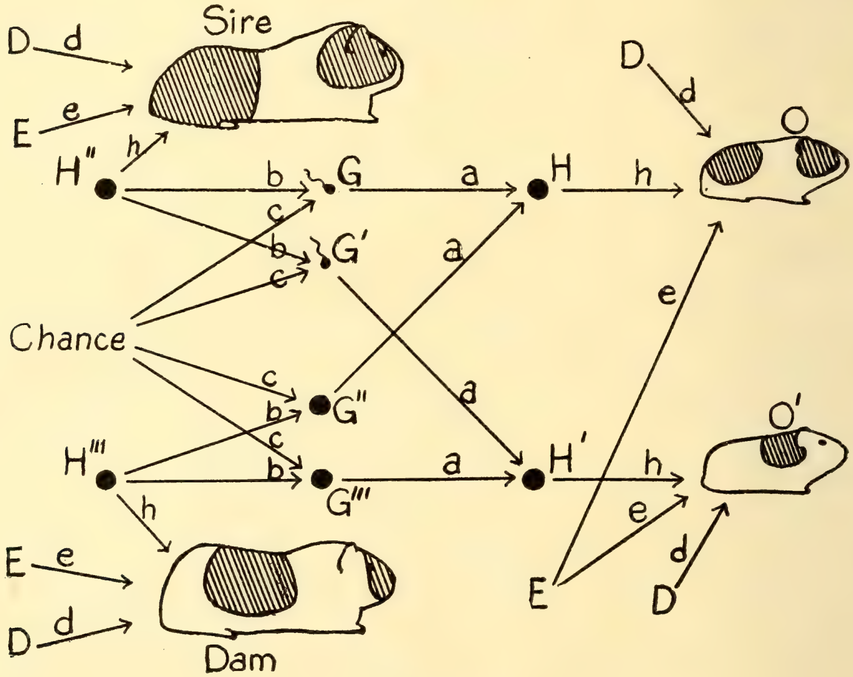


FIG. 5.

Diagram illustrating the casual relations between litter mates (O, O′) and between each of them and their parents. H, H′, H″, H,‴ represent the genetic constitutions of the four individuals, G, G′, G″, and G‴ that of four germ cells. E represents such environmental factors as are common to litter mates. D represents other factors, largely ontogenetic irregularity. The small letters stand for the various path coefficients.

In the diagram, the pattern of each guinea-pig is represented as determined by three factors, H (heredity), E (environment common to litter mates before birth) and D (the residue, largely irregularity in development). Our problem is to determine the degree of determination by each of these factors.

In a forthcoming paper, a method of estimating the degree to which a given effect is determined by each of a number of causes will be discussed at some length.

Figure 6 is meant to illustrate a system in which the variations of two quantities X and Y are determined in part by independent causes, such as A and D, respectively, and in part by common causes such as B and C. These common causes may be correlated with each other as in the figure. It is assumed that all of the relations are approximately linear and that the influence of the various causes are combined approximately by addition. The path coefficient, measuring the importance of a given path of influence from cause to effect, is defined as the ratio of the variability of the effect to be found when all causes are constant except the one in question, the variability of which is kept unchanged, to the total variability. Variability is measured by the standard deviation. The path coefficients in the figure are represented by small letters.

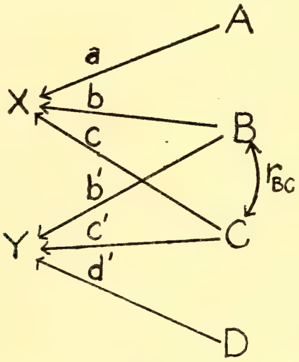


FIG. 6

Diagram illustrating two effects (XY) which are determined in part by the same correlated causes (BC).

It can be shown that the squares of the path coefficients measure the degree of determination by each cause. If the causes are independent of each other, the sum of the squared path coefficients is unity. If the causes are correlated, terms representing joint determination must be recognized. The complete determination of X in figure 6 by factor A and the correlated factors B and C, can be expressed by the equation:

$$a^2 + b^2 + c^2 + 2bcr_{BC} = 1 \qquad (1)$$

The squared path coefficients and the expressions for joint determination measure the portion of the mean square deviation of the effect due to the causes singly and jointly, respectively.

The correlation between two variables can be shown to equal the sum of the products of the chains of path coefficients along all of the paths by which they are connected. In figure 6

$$r_{XY} = bb' + cc' + br_{BC}\,c' + cr_{BC}b'. \tag{2}$$

The path coefficients in a system of causes and effects can be calculated, if a sufficient number of simultaneous equations can be made, expressing the known correlations in terms of the unknown path coefficients as in the last equation (2), and expressing complete determination of the effects by their causes as in equation (1).

The correlation between characteristics of parent and offspring are represented in figure 5 as tracing back to a single cause, the genetic constitution of the parent. Litter mates on the other hand are correlated for three reasons, the genetic constitutions of the sire and dam, respectively, and the common environment. In the absence of assortative mating, these causes are not correlated with each other. In such a system, the path coefficients are merely coefficients of correlation.

That the square of the coefficient of correlation measures the degree of determination of one variable by another in such cases may be seen from Pearson's fomula for the standard deviation of one variable (X) when another (A) is constant.

$$_A\sigma_X = \sigma_X\sqrt{1 - r^2_{XA}} \tag{3}$$

$$r^2_{XA} = 1 - \frac{_A\sigma^2_X}{\sigma^2_X} \tag{4}$$

The expression $1 - {_A\sigma^2_x}/\sigma^2_x$ is obviously the portion of the mean square deviation of X which is directly due to A if the latter is a cause of variation in X. In the present case the degree to which pattern is determined by the three independent factors, heredity (H), tangible environment (E) and irregularity in development (D) are measured by r^2_{OH}, $r^2_{\sigma E}$ and r^2_{OD}. Using the letters of figure 5 for the correlations, we have

$$h^2 + e^2 + d^2 = 1 \quad \text{from (1).} \tag{5}$$

The correlation between parent and offspring is the product of the correlations along the single chain of factors by which they are connected.

$$r_{PO} = hbah = abh^2 \tag{6}$$

The correlation between litter mates is the sum of the products of the correlations along the three connecting paths,

$$r_{OO^1} = habbah + habbah + ee = 2a^2b^2h^2 + e^2. \tag{7}$$

In a sense, the genetic constitution of the fertilized egg is simply the sum of the constitutions of the egg and sperm. The latter are independent of each other in a population which is mating at random. They are of equal importance, as indicated by the equality of inheritance from sire and dam. The degree of determination in each case must, therefore, be one-half.

$$r^2_{GH} = a_2 = {}^1/_2 \qquad a = \sqrt{{}^1/_2} \tag{8}$$

The relation between the germ-cells and the genetic constitution of one of the parents is not quite so obvious. It is well known that in a population which is mating at random, the proportion of homozygous and heterozygous classes come to an equilibrium such that the number of those heterozygous in a given factor equals twice the square root of the product of the numbers in the two homozygous classes. For factors A and a, the proportion in the three classes can be expressed in the following form:

$$x^2AA + 2xyAa + y^2aa$$

In the production of this population by the preceding generation, there were $x^2 + y^2$ cases in which similar germ-cells united at fertilization and $2xy$ cases in which the egg and sperm were dissimilar.

When this population produces germ-cells, there are $x^2 + y^2$ cases in which gametogenesis results in similar ones, and $2xy$ cases in which they are dissimilar.

Thus in a population of this kind, the correlation between the germ-cells produced by gametogenesis is exactly the same as that between two germ-cells which unite at fertilization, the correlation in which case, of course, is zero. Consequently, the relation between a germ-cell and the genetic constitution of the parent is the same as its relation to that of the offspring. Thus we may look upon the constitution of the germ-cell as half determined by that of the parent and half by chance at segregation.

$$r_{H'G} = b = a = \sqrt{{}^1/_2} \tag{9}$$

In considering the genetic constitution of the fertilized egg as simply the sum of the constitutions of the egg and sperm, each heterozygous class is rated as if exactly intermediate between the two corresponding homozygous classes. This is not, however, the correct way to grade the genetic constitution as a factor in development, unless dominance is lacking. The correlation between the two methods of grading depends on the degree of dominance and on the ratio of dominant to recessive factors in the population. For perfect dominance and a distribution of all pairs of allelomorphs according to the proportion $1AA : 2Aa : 1aa$, the correlation is $\sqrt{{}^2/_3}$. The correlation between the genetic constitution of parent and offspring is thus about ${}^1/_3$ if there is perfect dominance, instead of ${}^1/_2$.

As already noted, the progeny of a cross between two grades of piebald are almost exactly intermediate on the average and the second generation is only slightly more variable than the first. These results might be due to two groups of dominant factors with opposed effects. The most probable explanation, however, is that dominance is lacking and this view will be adopted provisionally.

With this assumption, our three equations for determining the values of h^2, e^2 and d^2 are as follows in the two stocks under consideration.

	EXP. B	FAM. 35
(5) $h^2 + e^2 + d^2 = 1$		
(10) from (6), $r_{PO} = {}^1/_2h^2 =$	0.211	0.014
(11) from (7), $r_{OO^1} = {}^1/_2h^2 + e^2 =$	0.214	0.069
$h^2 =$	0.422	0.028
$e^2 =$	0.003	0.055
$d^2 =$	0.575	0.917
	1.000	1.000

Thus in the control stock, variations in pattern are determined about 42% by heredity, and 58% by irregularity in development, leaving nothing for tangible environmental factors. In the inbred family the corresponding figures are 3% for heredity, 5% for tangible environment and 92% for irregularity in development. We can find the mean squared deviation due to each kind of factor by applying these percentages to the total squared standard deviation of each stock. (Equation 3 and table 1.)

	CONTROL (B)	IMBRED (35)
Heredity, $\sigma_{O.H}^2$	0.271	0.010
Tangible environment, $\sigma_{O.E}^2$	0.002	0.020
Development, $\sigma_{O.D}^2$	0.370	0.334
Total, σ_O^2	0.643	0.364

If these results had been obtained in the control stock before the inbreeding commenced, the extent to which the variability of the stock should be reduced by inbreeding could have been predicted, assuming dominance to be lacking. Inbreeding automatically renders all Mendelian factors homozygous and, therefore, eliminates all genetic variability. There is no reason for supposing that variability due to environmental or ontogenetic causes would be affected. The sum of the mean square deviations due to the latter causes is 0.372 in the control stock. This, then, is the mean square deviation to be expected after prolonged inbreeding, while its square root, 0.610, is the expected standard deviation. The actual figures in the inbred family, 0.364 for the mean square deviation, 0.603 for the standard deviation, are an even better check than could reasonably be expected on the accuracy of the assumptions on which the degrees of determination were calculated.

FOSSIL PLANTS FROM THE LATE CRETACEOUS OF TENNESSEE

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Communicated by Harry Fielding Reid, April 14, 1920

That part of the Atlantic Coastal Plain known as the Mississippi Embayment is one of strategic paleobotanical interest. It is a region unacquainted with orogenesis and remote from vulcanism, a region on the threshold as it were of the present-day tropics, far removed from glaciation, and with a relatively simple geological history. Hence the Gulf Coastal Plain affords a unique opportunity for studying the almost unbroken evolution of floras from the Cretaceous to the present. Many of these floras have been described in recent years, the two large unknown gaps in the record having been a knowledge of the later Upper Cretaceous and the Miocene floras. The latter still remains to be discovered, but information regarding the former will now be available through the fortunate discoveries by Dr. Bruce Wade during the summer of 1919. In working near the boundary between the Coastal Plain and the Paleozoic rocks which form its margin and basement in western Tennessee several clay lenses with an abundant varied and well-preserved representation of the flora of the Ripley formation were discovered. The Ripley formation is the latest Cretaceous known in this region and is overlain unconformably by the basal Eocene. Its sediments, in general shallow water marine sands, with many suggestions of plants in their carbonaceous sdum have heretofore yielded but few and fragmentary representatives of the forests which clothed the shores of the Ripley sea—such things as leaves or broken twigs of Araucarian or other conifers, and a few of the more coriaceous dicotyledonous leaves that chanced to settle in its waters. The clay lenses in Henry and Carroll counties are of fine-grained and pure material, evidently the sediments of quiet estuarine or lagoonal waters close to a forested shore and perhaps the distributaries of slow streams or bayous traversing a richly forested country. The collected representatives of this flora contain some surprises in the survival of what have been considered earlier forms, and have an important bearing on correlation as well as affording a fair picture of the general facies. These considerations warrant a preliminary announcement.

The most extensive best-preserved and earliest studied Upper Cretaceous floras, both in this country and abroad, have been those in the continental deposits immediately preceding, or in the initial deposits of the transgressing Upper Cretaceous sea, such for example as the Dakota sandstone of Kansas and Nebraska, the Raritan clays of New Jersey, the Perucer beds of Bohemia or the Niederschoena deposits of Saxony. Coming upward in all of the sections the plant evidence has rapidly diminished in both variety and quality. A knowledge of the Ripley flora not only serves to offset this condition and furnish a basis for more precisely evaluating the passing of Cretaceous time, but it introduces us to many of the smaller and more delicate forms that almost invariably fail of preservation in coarse deposits.

Examples of the relative paucity of the later Cretaceous floras of the Atlantic Coastal Plain are furnished by noting that the Tuscaloosa, Raritan and Magothy floras comprise about 150 species each whereas when I published the Upper Cretaceous Floras of the World in 1916 I listed but 9 species from the Ripley formation in Georgia, two from beds of that age in Alabama and three from Tennessee. A foreign parallel may be taken from the Bohemia area. Here the Perucer beds (Cenomanian) have furnished 184 species, the Turonian 47 and the Chlomeker beds (Emscherian) but 32.

The present discoveries disclose the remains of 124 species complete enough for descriptive purposes, of which 86 are new to science. They are referred to 62 genera in 38 families and 25 orders. Thirty of the genera or 49% are extinct, and all but one of the species and about a dozen of the genera are not found in the Eocene.

Contrasts with the earlier Upper Cretaceous floras are the scanty representation of the gymnosperms, here reduced to 6 species, and the absence of traces Sequoia or cycads. Familiar types not present are Magnolia, and Diospyros, and there are no certainly identified Proteaceae. In a general way the flora suggests slightly warmer climates than those that preceded it during Upper Cretaceous times, and there are a considerable number of types that appear to be ancestral to members of the lower Eocene flora of the Mississippi Embayment region.

The flora contains but 5 ferns, 7 monocotyledons and 105 dicotyledons, of which last 94 are Choripetalae and 11 Gamopetalae. The largest alliances are the Leguminosae with 14 species, the Lauraceae with 12, the Myricaceae with 11 and the Moraceae with 10. The largest genera are Myrica with 11 species, Ficus with 9 and Celastrophyllum with 6.

Compared with floras in other areas the Ripley flora of Western Tennessee shows little in common with the Laramie flora of the West. It does contain a number of elements common to the Montana Group floras, particularly the flora of the Vermejo formation of Colorado and New Mexico. There are also similarities with the flora of the Patoot beds of Greenland and the upper Emscherian and lower Aturian (Campanian) floras of Western Europe.

A complete account of the Ripley flora has been prepared which will be published by the U. S. Geological Survey.

NATIONAL RESEARCH COUNCIL

NOTES FROM THE MINUTES OF THE MEETINGS IN JANUARY AND FEBRUARY, 1920, OF THE EXECUTIVE BOARD AND INTERIM COMMITTEE OF THE NATIONAL RESEARCH COUNCIL.

At the meeting of the Executive Board, February 14, the Chairman of the Committee on Publication reported that the first number of the Bulletin Series had been issued and that five numbers of the Reprint and Circular Series are in the hands of the printers. A considerable number of subscriptions have already been received for the Bulletin Series and requests from several libraries and individuals have been received for the Reprint and Circular Series as the numbers become available.

The Chairman of the Research Information Service reported the status of several projects which are being undertaken by that Division; first, the preparation of a catalogue of research laboratories or organizations to include all laboratories throughout the country in which research is fostered; second, the preparation of information concerning current investigations which will involve securing data from five or six hundred educational institutions and from thousands of independent workers and laboratories; and, third, a file of the scientific personnel of the United States, for which the names of about twelve thousand investigators have already been collected.

The Chairman of the Division of Physical Sciences presented a resolution passed by the Mathematical Association of America requesting representation in the Council through a separate Division of Mathematics. The matter was discussed at some length and referred to the Division of Physical Sciences for recommendation.

The Chairman of the Division of Chemistry and Chemical Technology stated that the annual dues of the United States to the International Chemical Union are 4500 francs, and that, at the request of the constituent societies of the Division that they be permitted to share in this expense, an agreement has been reached whereby the Division of Foreign Relations shall pay one-half the amount, the balance to be paid by the Division of Chemistry and Chemical Technology through its constituent societies.

The American Geophysical Union was made a committee of the Executive Board of the National Research Council.

LECTURE AND EXHIBIT

Mr. J. J. Carty, Vice-President of the American Telephone and Telegraph Company and member of the Executive Board and of the Division of Research Extension of the National Research Council, gave a lecture, under the auspices of the Council, February 6, in the auditorium of the new National Museum on "Science and the Industries." This lecture was illustrated by an exhibition of a number of pieces of apparatus of great historical interest, among them being the electromagnet and galvanometer used by Joseph Henry and the original telephone used by Alexander Graham Bell and Thomas A. Watson. The lecture was also illustrated by talking motion pictures.

On February 9, an exhibit of apparatus and motion pictures telling the story of the historical development of wireless telegraphy and telephony was opened to the public in the exhibit room of the Council in Washington. This exhibit was prepared and installed by the American Telephone and Telegraph Company and the Western Electric Company, Incorporated, in coöperation with the U. S. Signal Corps and Air Service.

DIVISION AND COMMITTEE APPOINTMENTS

Government Division.—Vice-Chairman, Carl L. Alsberg, Chief, Bureau of Chemistry, Department of Agriculture; Executive Committee, Chairman, Charles D. Walcott, Secretary, Smithsonian Institution, Vice-Chairman, Carl L. Alsberg, Secretary, R. M. Yerkes, National Research Council, Col. Colden L'H. Ruggles, Ordnance Department, U. S. A., Rear Admiral Ralph Earle, Bureau of Ordnance, U. S. N., S. W. Stratton, Chief, U. S Bureau of Standards, and George Otis Smith, U. S. Geological Survey.

Representatives of the Departments of the Government.—Department of State: Julius G. Lay, Acting Foreign Trade Advisor.

Department of Treasury: Assistant Surgeon General J. W. Schereschewsky, Public Health Service.

Department of War: Maj. General William L. Sibert, Chemical Warfare Service, Maj. General George O. Squier, Signal Corps, Brig. General Marlborough Churchill, Military Intelligence Division, Col. Colden L'H. Ruggles, Ordnance Department, Col. Frederick F. Russell, Medical Corps, Col. Clarence O. Sherrill, Corps of Engineers, Lieut. Col. Robert E. Callan, Coast Artillery, Lieut. Col. Byron Q. Jones, Air Service, Major William M. Fassett, General Staff.

Department of Justice: John T. Creighton, Bureau of Investigation.

Post Office Department: John T. Miller, Chief of the Division of Correspondence.

Department of the Navy: Rear Admiral A. P. Niblack, Office of Naval Intelligence of Office of Naval Operations, Rear Admiral J. A. Hoogewerff, Bureau of Navigation, Rear Admiral D. W. Taylor, Bureau of Construction and Repair, Rear Admiral R. S. Griffin, Bureau of Steam Engineering, Rear Admiral W. C. Braisted, Medical Corps, Bureau of Medicine and Surgery, Rear Admiral C. W. Parks, Bureau of Yards and Docks.

Department of the Interior: M. H. Coulston, Assistant Commissioner of Patents, S. P. Capen, Specialist in Higher Education, Bureau of Education, George Otis Smith, Director, U. S. Geological Survey, A. P. Davis, Director and Chief Engineer, U. S. Reclamation Service, F. G. Cottrell, Assistant Director, Bureau of Mines.

Department of Agriculture: C. F. Marvin, Chief of Weather Bureau, J. R. Mohler, Bureau of Animal Industry, K. F. Kellerman, Bureau of Plant Industry, H. S. Graves, Forest Service, C. L. Alsberg, Bureau of Chemistry, Milton Whitney, Bureau of Soils, L. O. Howard, Bureau of Entomology, E. W. Nelson, Bureau of Biological Survey, T. H. MacDonald, Bureau of Public Roads.

Department of Commerce: S. W. Stratton, Director, Bureau of Standards, Hugh M. Smith, Commissioner of Fisheries, Joseph A. Hill, Chief Statistician, Division of Revision and Results, Bureau of Census, William Bowie, Hydrographic and Geodetic Engineer, Coast and Geodetic Survey.

Department of Labor: Royal Meeker, Commissioner of Labor Statistics.

Smithsonian Institution: Charles D. Walcott, Secretary.

Division of States Relations.—Dr. T. S. Palmer, Expert in Game Conservation, Bureau of Biological Survey, as member at large.

Division of Research Extension.—An advisory Committee on Alloys Research was established composed of the following members: Chairman, W. M. Corse, Monel Metal Products Corporation, Bayonne, N. J., Lt. Commander William Campbell, Columbia University, G. H. Clamer, First Vice-President and Secretary, The Ajax Metal Company, Philadelphia, H. W. Gillett, Chief Alloy Chemist, Department of the Interior, Bureau of Mines, Ithaca, Zay Jeffries, Lynite Laboratories of the Aluminum Castings Company, Cleveland, Jesse L. Jones, Westinghouse Electric Company, Paul D. Merica, The International Nickel Company, Bayonne, Paul E. McKinney, U. S. Navy Yard, John A. Mathews, President, Halcomb Steel Company, Syracuse, and lately Vice-President of the Crucible Steel Company, C. H. Mathewson, Sheffield Scientific School, Yale University, Richard Moldenke, Watchung, New Jersey, William B. Price, Waterbury, Connecticut, George C. Stone, Chief Metallurgist, The New Jersey Zinc Company, New York City, John F. Thompson, International Nickel Company, New York City, W. R. Webster, Vice-President, Bridgeport Brass Company, Bridgeport, Albert E. White, Consulting Metallurgical Engineer, Professor of Chemical Engineering, University of Michigan.

Division of Engineering.—Additional members have been appointed as follows: To represent the Institute of Mining and Metallurgical Engineers, Allen H. Rogers, New York City; to represent the American Society of Heating and Ventilating Engineers, John R. Allen, Dean of the School of Engineering and Architecture, University of Minnesota; and as a member at large representing the Association of State Highway Officials, T. R. Agg, Iowa State College.

The following committee appointments have been made: A. E. Kennelly, Harvard University, to be Chairman of the Committee on Core

Losses; T. R. Agg, Iowa State College, to be Chairman of the Committee on Economic Theory of Highway Improvement; A. N. Goldbeck, Engineer of Tests, Bureau of Public Roads, to be Chairman of the Committee on Structural Design of Roads; H. S. Mattimore, Engineer of Tests, Pennsylvania State Highway Department, to be Chairman of the Committee on Tests and Properties of Road Materials and their Combinations; J. V. Emmons, Cleveland Twist Drill Company, R. P. Devries, Lublum Steel Company, E. L. French, Crucible Steel Company of America, Henry M. Howe, Bedford Hills, New York, C. M. Johnson, Crucible Steel Company of America, Frank Lounsberry, Atlas Crucible Steel Company, A. F. MacFarland, Vanadium Alloys Company, J. A. Mathews, Crucible Steel Company of America, and Howard Scott, Bureau of Standards, to be members of the Committee on High Speed Steels.

Division of Biology and Agriculture.—A Committee on Eugenics to make arrangements for the International Eugenics Congress in New York in 1921 has been appointed, composed of the following members: L. F. Barker, Johns Hopkins Hospital, representing Medicine, E. A. Hooton, Peabody Museum, Cambridge, representing Anthropology, R. M. Yerkes, National Research Council, representing Psychology, Stewart Paton, Princeton University, representing Psychiatry, Raymond Pearl, School of Hygiene and Public Health, Johns Hopkins University, representing Eugenics and Biometry, and Alexander G. Bell, Washington.

Committees on Coöperation have been appointed by the following Societies to coöperate with the Division of Biology and Agriculture and to represent the Societies in the Division in the same manner as the present Forestry Committee: American Society of Naturalists, American Society of Agronomy, American Society of Zoölogists, and Botanical Society of America.

Division of Geology and Geography.—Additions to the Committee on Sedimentation have been made as follows: G. D. Louderback, University of California, H. E. Merwin, Geophysical Laboratory, W. H. Twenhofel, University of Wisconsin, representing the Association of State Geologists, and J. A. Udden, Director, Bureau of Economic Geology and Technology, University of Texas.

Division of Physical Sciences.—Dr. Augustus Trowbridge, Professor of Physics, Princeton University, has been made Vice-Chairman of the Division of Physical Sciences.

The following have been appointed as members of the American Geophysical Union: D. L. Hazard, U. S. Coast and Geodetic Survey, J. A. Fleming, Department of Research in Terrestrial Magnetism, S. J. Mauchly, Department of Research in Terrestrial Magnetism, T. A. Jaggar, Hawaiian Volcano Observatory, U. S. Weather Bureau, F. A. Perret, Posillipo, Naples, W. M. Davis, Sturgis-Hooper Professor of Geology, emeritus, Harvard University, and George P. Paine, Blue Hill Observatory, Harvard University.

Division of Anthropology and Psychology.—The resignation of Dr. Franz Boas from membership in the Division has been accepted.

The following committees have been appointed:

Advisory Committee on Problems of Military Psychology: Chairman, Col. W. D. Scott, Northwestern University and the Scott Company, Philadelphia, Major G. F. Arps, Ohio State University, Major C. S. Yoakum, Carnegie Institute of Technology.

Committee on Anthropological and Psychological Study of Peoples of the United States: Chairman, *ex-officio,* Dr. W. V. Bingham, Professor of Psychology, and Director of the Division of Applied Psychology, Carnegie Institute of Technology, and Chairman of the Division of Anthropology and Psychology, National Research Council, Dr. E. L. Thorndike, Professor of Educational Psychology, Columbia University, and Dr. Clark Wissler, Curator, Department of Anthropology, American Museum of Natural History.

Committee to Formulate a Program of Research on Accumulated Army Data: Chairman, Dr. R. M. Yerkes, Chairman, Research Information Service, National Research Council, Dr. W. V. Bingham, Professor of Psychology, and Director of the Division of Applied Psychology, Carnegie Institute of Technology, and Dr. Clark Wissler, Curator, Department of Anthropology, American Museum of Natural History.

Committee on Specific Projects Outside the United States: Chairman, Dr. J. Walter Fewkes, Bureau of American Ethnology, Smithsonian Institution, Dr. William C. Farabee, Curator, University Museum, University of Pennsylvania, and Dr. R. B. Dixon, Professor of Anthropology, Harvard University.

Committee on Personnel in Research and Industry: Chairman, Beardsley Ruml, The Scott Company, Philadelphia, and Dr. James R. Angell, Dean of the Faculties of Arts, Literature and Science, and Head of the Department of Psychology, University of Chicago, and Chairman, National Research Council.

REPORT OF THE MEETINGS OF THE INTERNATIONAL RESEARCH COUNCIL AND OF THE AFFILIATED UNIONS HELD AT BRUSSELS, JULY 18–28, 1919

Presented to the Division of Foreign Relations, National Research Council

By W. W. Campbell, *Chairman,* American Delegation to the International Research Council

In response to the call of the Executive Committee of the International Research Council, the third conference of representatives of the sciences in the allied and associated nations was held in Brussels on July 18 to July 28, inclusive. The purpose of the conference was to complete the organization of the International Research Council, and to form international unions, affiliated with the Council, representing the individual sciences. Duly authorized delegates from the following countries took part in the conference: Belgium, Canada, France, Great Britain and Ireland, Italy, Japan, New Zealand, Poland, Roumania, Serbia, and the United States of America. The total number of delegates from the eleven countries was slightly over two hundred. The American delegation, formed under the auspices of the National Research Council, numbered twenty-one, and was as follows:

International Research Council

W. W. Campbell, *Chairman,* Chairman of American Delegation to International Astronomical Union.

C. E. Mendenhall, Scientific Attaché at London, also representing Physics.

H. S. Washington, Scientific Attaché at Rome, also representing Geology.

Major Wm. Bowie, Chairman of American Delegation to International Geophysical Union.

E. W. Washburn, Chairman of American Delegation to International Chemical Union.

W. S. Thayer, Representing Medicine.

John C. Penny, Representing Patents.

Major Douglas W. Johnson, Representing Geography.

Astronomical Union

W. W. Campbell, *Chairman*
W. S. Adams
Benjamin Boss
Major Philip Fox
S. A. Mitchell
Frank Schlesinger
C. E. St. John
F. H. Seares
Joel Stebbins

Geophysical Union

Major Wm. Bowie, *Chairman*
L. A. Bauer
G. W. Littlehales
Admiral Edward Simpson

Chemical Union

E. W. Washburn, *Chairman*
F. G. Cottrell
Chas. L. Parsons
H. S. Washington

Medicine

W. S. Thayer, *Chairman*

Geology

H. S. WASHINGTON, *Chairman*

Geography

MAJOR DOUGLAS W. JOHNSON, *Chairman*

Physics

C. E. MENDENHALL, *Chairman*

Patents

JOHN C. PENNY, *Chairman*

All sessions of the Council and of the affiliated unions and all of the related committee meetings were held in the splendid Palais des Académies, under the patronage of the Royal Belgian Academy of Sciences. The Palais, spacious in plan and sumptuous in appointments, occupies an isolated and commanding position *vis à vis* the Palais-Royal and the great Parc of the city.

Illness prevented the attendance of Professor Picard, Chairman of the Executive Committee of the Council. This position was filled for the occasion by Professor Lacroix, who presided admirably over the sessions of the Council and of the Executive Committee. Professor Hale, member of the Committee, was unable to attend the Conference, and Mr. Campbell served in his stead. Professor Schuster, Secretary of the Royal Society and General Secretary of the Executive Committee, served ably as the secretary of the Council. Other members of the Executive Committee were M. Lecointe, Director of the Royal Observatory of Belgium, and Professor Volterra, of the University of Rome.

The opening session of the Conference was honored by the presence of His Majesty, Albert, King of the Belgians, who occupied the royal box in the great assembly room. He had previously received the members of the Executive Committee in an adjoining room. An address of congratulation to the King was read by M. Hermignie, Belgian Minister of Sciences and Arts, and a cordial welcome to the delegates was pronounced by the Vice-President of the Class of Sciences of the Royal Belgian Academy. Upon the motion of the President of the Belgian Academy, the Executive Committee, as described above, served as the administrative Bureau of the Research Council throughout the Conference.

The several plenary sessions of the Research Council were devoted largely to the formulation of a Constitution which should define the purposes and govern the proceedings of the Council. The Executive Committee in special session at Paris in May had drawn up a proposed constitution, and this, in printed form, served as the basis for our deliberations. The modifications were accomplished by general consent, or by oral ballot in which each country had one vote, and final adoptions rested also upon this form of ballot.

The Conference was able to complete the structure of the Constitution, to the satisfaction of all the delegations, except in one particular: that relating to the number of members composing the Executive Committee of the Council, and certain possible conditions governing their choice. For example, many delegates were apparently in favor of the proposal that each affiliated union should have one representative on the Executive Committee of the Council, either *ex-officio* or by the choice of the union. There was not time to consider the probable effects of the various proposals, and the Committee was fixed temporarily at five members, with the understanding that this provision should be subject to discussion and modification at the next general meeting of the Council, in 1922.

The Constitution as adopted was in French text, and it was formally agreed that interpretations of its provisions should be based upon that text. The Constitution follows herewith, the text being that issued from the office of the General Secretary of the I. R. C.

INTERNATIONAL RESEARCH COUNCIL

Statutes of Convention

I—OBJECTS OF INTERNATIONAL COUNCIL AND CONDITIONS OF ADMISSION

1. The purpose of the International Research Council is:

(*a*) To coördinate international efforts in the different branches of science and its applications.

(*b*) To initiate the formation of international Associations or Unions deemed to be useful to the progress of science in accordance with Article I of the resolutions adopted at the Conference of London, October, 1918.[1]

(*c*) To direct international scientific activity in subjects which do not fall within the purview of any existing international associations.

(*d*) To enter through the proper channels into relation with the Governments of the countries adhering to the International Research Council in order to promote investigations falling within the competence of the Council.

II—DOMICILE

2. The legal domicile of the International Research Council shall be at Brussels where the General Assemblies will be held and the archives kept.

Donations and legacies will be received and administered according to Belgian law.

III—CONDITIONS OF ADMISSION

3. The countries in the following list may participate in the foundation of the International Research Council, and of any scientific Union connected with it, or join such Union at a subsequent period:

Belgium, Brazil, United States, France, the United Kingdom of Great Britain and Ireland, Australia, Canada, New Zealand, South Africa, Greece, Italy, Japan, Poland, Portugal, Roumania, Serbia.

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[1] "That it is desirable that the nations at war with the Central Powers withdraw from the existing conventions relating to International Scientific Associations in accordance with the Statutes or Regulations of such Conventions, respectively, as soon as circumstances permit; and

"That new associations, deemed to be useful to the progress of science and its applications, be established without delay by the nations at war with the Central Powers with the eventual coöperation of neutral nations."

After a Union[1] has been formed, nations not included in the above list, but fulfilling the conditions of Article 1 of the resolutions of the Conference of London, may be admitted, either at their own request, or on the proposal of one of the countries already belonging to the Union.

Such requests or proposals shall be submitted to a vote of the Union concerned. A favorable majority of not less than three-quarters of the countries already forming part of the Union shall be required for admission.

4. A country may join the International Research Council, or any Union connected with it, either through its principal Academy, its National Research Council, some other national institution or association of institutions, or through its Government.

5. The Statutes of the Unions formed by the International Research Council require the approval of this Council.

IV—ADMINISTRATION OF THE COUNCIL

6. The work of the Council is directed by the General Assembly, consisting of delegates appointed by the adhering countries.

7. There shall be an Executive Committee, consisting of five members elected by the General Assembly, which shall direct the affairs of the Council in the interval between the meetings of the General Assembly in accordance with the resolutions adopted at the previous meeting of the Assembly.[2]

8. The Bureau of the Executive Committee shall consist of a President, two Vice-Presidents, and a General Secretary, who shall be elected by the General Assembly, and hold office until the end of the second General Assembly following that of their election, except that the President and one of the two Vice-Presidents shall retire at the end of the first General Assembly following that of their election. Retiring members are re-eligible.

9. The General Secretary shall be responsible for the conduct of the correspondence, the administration of funds, and the preparation and distribution of the publications authorized by the General Assembly.

10. The Executive Committee may fill up by co-optation any vacancy occurring in its body in the interval between two General Assemblies. Any person so appointed shall hold office until the next General Assembly, which shall then proceed to an election. The member thus elected shall complete the term of office of the person whose place he takes.

V—POWERS AND FUNCTIONS OF THE EXECUTIVE COMMITTEE

11. The Executive Committee may in the interval between two meetings of the General Assembly submit proposals to the organizations adhering to the Council for their approval. It shall do so when requested by one-third of the bodies constituting the Council.

12. The Executive Committee may nominate special Committees for the discussion of any question falling within the purview of the International Research Council. The members of such Committees need not necessarily have been delegates at one of the General Assemblies. These special Committees may themselves co-opt other members by a majority of two-thirds.

13. The Executive Committee shall present an Annual Report to the organization in each country which has joined the Council. Such reports shall also be sent to each delegate who was present at the previous meeting of the General Assembly.

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[1] The term "Union" in this and subsequent paragraphs is intended to include "Association."

[2] Articles 7 and 8 are subject to revision. The Executive Committee elected in 1919 will vacate office at the next General Assembly.

VI—MEETINGS OF THE GENERAL ASSEMBLY

14. An ordinary meeting of the General Assembly shall, as a rule, take place once every three years. The date of the meeting, unless determined by the General Assembly at its previous meeting, shall be fixed by the Executive Committee, and communicated to the organizations adhering to the International Research Council at least four months beforehand, together with the agenda of business to be transacted.

15. For special reasons the President may, with the consent of the Executive Committee, summon an extraordinary meeting of the General Assembly. He is obliged to do so at the request of one-third of the adhering countries.

16. The delegates of the different countries taking part in the meetings of the General Assembly shall be appointed by the organizations adhering to the International Research Council.

17. The President of the Executive Committee may invite to a meeting of the General Assembly scientific men who are not delegates, provided that they are subjects of one of the adhering countries. Such invited guests may take part in the discussions, but shall have no power of voting. Members of any of the Special Committees mentioned in Article 12 shall have the right to attend those meetings of the General Assembly which deal with the subjects referred to the Committee.

18. The agenda of business to be transacted at a meeting shall be determined by the Executive Committee, and communicated to the adhering organizations at least four months before the first day of the meeting. No question which has not been placed on the agenda shall be discussed, unless a proposal to that effect be approved by not less than one-half of the total votes of the countries represented at the General Assembly.

VII—FINANCE AND VOTING POWER

19. The Executive Committee shall prepare an estimate of the budget for each of the years intervening between two General Assemblies. A Finance Committee, nominated for the purpose by the General Assembly, shall examine this estimate, together with the accounts for the preceding years. This Committee shall submit separate reports on the two questions to the General Assembly which, having considered these reports, shall fix the unit of contribution for the succeeding period. The contributions due from countries and their corresponding voting powers are determined by the population of the countries according to the following scale:

POPULATION	NO. OF VOTES	NO. OF UNITS OF CONTRIBUTION
Less than 5 millions	1	1
Between 5 and 10 millions	2	2
Between 10 and 15 millions	3	3
Between 15 and 20 millions	4	5
Over 20 millions	5	8

Each state is at liberty to include in its population, according to the indication of its Government, the inhabitants of its colonies or protectorates. Self-governing dominions (South Africa, Australia, Canada, New Zealand) have separate voting power according to the above scale.

The contribution to be levied during the first period of the Convention shall not exceed 250 francs[1] per annum per unit.

In each country the organization adhering to the Council shall be responsible for the payment of the contribution.

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[1] The value of the franc shall be that of the French currency.

20. The income of the Council derived from the contributions of the different countries shall be devoted to the expenses of the Bureau.

Funds derived from donations shall be used by the Council in accordance with the wishes expressed by the donors.

If any country withdraws from the Council, it resigns at the same time its rights to a share in the assets of the Council.

21. At the General Assembly votes upon scientific questions shall be decided by the majority of votes cast by the delegates present. In questions of an administrative character, and all questions not purely scientific, the vote shall be taken by countries, each country having the number of votes indicated in Article 19. In case of doubt as to the category to which a question belongs, the Chairman of the meeting shall decide. Votes taken in Committees shall be counted individually and not by countries. When there is an equal division of votes, the Chairman shall in all cases have a second or casting vote.

22. In questions of an administrative character, any country which is not represented by a delegate may forward its vote to the President by post, and such votes shall be counted if received before the vote is taken.

VIII—DURATION OF THE CONVENTION AND MODIFICATIONS

23. The present Convention shall come into force on the 1st of January, 1920, provided that at least three of the countries mentioned in Article 3 have signified their adhesion. It shall remain in force until the 31st of December, 1931, and shall then, with the assent of the adhering countries, be continued for a further period of 12 years.

24. No change shall be made in the terms of the Convention except with the approval of two-thirds of the votes of the adhering countries.

25. The French text of the Convention (of which the above is a translation) shall be considered to be the authoritative text.

At the final session of the Council the original Executive Committee was unanimously re-elected, to serve during the periods specified in the Constitution. The Committee consists of:

Professor Emile Picard, *President*
Professor Arthur Schuster, *General Secretary*
Professor George E. Hale, *Member*
Director Georges Lecointe, *Member*
Professor Vito Volterra, *Member*

To secure overlapping in the terms of service, and, therefore, continuity of administration, the first president and one member of the first Executive Committee are to hold office for three years only, but the General Secretary and the other two members are to serve during six years. Officers and members elected at the next triennial meeting, or at later meetings, are to serve during six years. All officers and members are re-eligible.

The duration of the present convention, as defined by the Constitution, is twelve years, to December 31, 1931. It may then be renewed for another period of twelve years, with the assent of the adhering countries.

Sixteen allied and associated countries are named in the Constitution as eligible to participate in the foundation of the Research Council. The five of these countries not represented by delegates may become members of the Council and of the affiliated unions by expressing their adhesion to the terms of the Constitutions and by pledging their corresponding shares of financial support.

At the concluding session of the Conference, the Executive Committee was authorized to invite all leading neutral countries to enter into full membership relations with the Research Council and the affiliated unions.

In accordance with the terms of the Constitution, each adhering country is under obligations to contribute to the financial support of the Council. The relative amount of its contribution and the number of votes to which it is entitled are as defined by the following scale:

POPULATION OF COUNTRY	NUMBER OF VOTES	NO. OF FINANCIAL UNITS
Less than 5 million inhabitants.........	1	1
Between 5 and 10 million inhabitants...	2	2
Between 10 and 15 million inhabitants...	3	3
Between 15 and 20 million inhabitants...	4	5
More than 20 million inhabitants.......	5	8

The value of the financial unit for the Research Council itself during the first period of the convention (three years) may not exceed 250 francs per annum. That is, the annual obligation of the United States, in the first period, may not exceed 2000 francs annually. Funds received in this manner from the adhering countries shall be devoted to meeting the expenses of the administrative Bureau of the Council.

In advance of each triennial meeting the Executive Committee shall prepare a proposed financial budget for each of the three years following the meeting. A Finance Committee, nominated by the General Assembly of the Council, shall study this budget, and likewise audit the financial accounts for the preceding period. This Committee shall submit to the General Assembly separate and distinctive reports upon these two subjects. The Council in General Assembly shall determine the value of the contributive financial unit.

It was decided that the city of Brussels shall be the legal seat of the International Council of Research, and that the proceedings shall be governed by the laws of Belgium. It was likewise determined that the administrative seat of the Council—the Secretariat—shall be in the city where resides the General Secretary, in this instance in London.

The Executive Committee of the Council will submit the Constitution to the national academies, the national research councils, or the governments whose representatives took part in the Brussels Conference, for their approval. The International Research Council shall be considered as definitively constituted when three of the greater allied countries shall have expressed their approval. It is hoped that these approvals may be obtained not later than January 1, 1920.

One of the fundamental objects of the International Research Council is to encourage the creation of international unions representing, respectively, the chief divisions of science, in so far as such unions may be considered desirable and necessary. Simultaneously with the completion of the organization of the Research Council in Brussels three affiliated unions were definitively established, as follows:

The International Astronomical Union.
The International Geodetic and Geophysical Union.
The International Union of Pure and Applied Chemistry.

A Union of Biological Sciences and a Union of Scientific Radio-Telegraphy were organized, though somewhat less definitively; constitutions were formulated at Brussels, but it was decided that the plans proposed should be submitted to the different academies for their consideration.

Proposals were made and carefully considered for the establishing of unions of the mathematical sciences, of the physical sciences, of the geological sciences, of the geographical sciences, of bibliography, etc., but organic decisions were not reached. The expectation is that these unions will take definite form at or before the time of the next triennial meeting, in 1922. The mathematicians have decided, for example, to meet in Strasbourg in the year 1920, and the subject of an international union will at that time receive their consideration.

Engineering science and patent laws, both from the international point of view, were considered in Brussels by delegates representing these subjects.

A detailed report on the organization of the International Astronomical Union will have been presented to the National Research Council by the Secretary of the American Section.

The Secretary of the American Section of the Geodetic and Geophysical Union will have presented to the National Research Council a detailed report on the organization of the Union.

The Secretary of the American Section of the International Union of Pure and Applied Chemistry will have presented to the National Research Council a detailed report concerning the Union.

The International Union of Biological Sciences made progress in the direction of definitive organization. It is proposed that this Union shall be composed of six sections, as follows: General Biology, Physiology, Zoölogy, Botany, The Medical Sciences, Applied Biology.

The International Union of Radio-Telegraphy, which made progress toward definitive organization under the able leadership of General Ferrié, Inspector General of the Service of Military Telegraphy, Paris, proposes to concern itself chiefly with the study of radiometric phenomena as a pure science.

The activities of each union require that financial resources be available, as in the case of the International Research Council. It is proposed that all the contributions be united for any one country in one appropriation, payable to the International Research Council, which shall apportion the funds in accordance with the relative needs. The wisdom of this policy can scarcely be questioned, in view of the intimate relationship of governments to the subject of appropriations for these purposes. A definite statement as to the total financial obligations of the United States cannot now be made. This will depend in part upon the list of countries which give their adherence to the proposals. The total number of contributory financial units remains unknown for the present. It is hoped that the number depending upon the allied and associated countries will be augmented by the adherence of many neutral countries. It should also be stated that the effective population of certain member countries, and, therefore, the amounts of their financial contributions, will depend upon whether they decide to include the inhabitants of their colonies and protectorates.

A resolution expressing the profound sympathy of the delegates for their colleagues in Russia was adopted unanimously at the closing session of the Conference.

Formal receptions were tendered to the delegates: by the city of Brussels, represented by the celebrated Burgomaster Adolf Max and the members of his Council, at the Hotel de Ville; by M. Hermignie, Minister of Sciences and Arts, and Madame Hermignie; and by M. Hymans, Minister of Foreign Affairs, and Madame Hymans. M. Lecointe, Director of the Belgian Royal Observatory at Uccle (a suburb of Brussels) and Madame Lecointe, received the astronomical, geodetic, and geophysical delegates in the director's residence. The members of the Observatory staff kindly provided opportunities for the delegates to see the more interesting and important features of the Observatory and its work. Much of the success of the Brussels Conference was due to the thoughtful and able services of Director Lecointe, Belgian member of the Executive Committee of the Research Council.

The Conference was memorable for the splendid spirit of coöperation and personal friendliness which prevailed universally. The discussions were conducted with sincere efforts to reach wise conclusions. The decisions were in general unanimous. These facts are happy indications that the evolution of the new organizations will follow along successful lines.

The chairman desires to express his appreciation of the uniformly helpful spirit in which every member of the American delegation joined in the solution of the many problems before the Conference.

Respectfully submitted,
W. W. CAMPBELL,
Chairman of the American Delegation to the International Research Council.

REPORT ON THE ORGANIZATION OF THE INTERNATIONAL ASTRONOMICAL UNION[1]

Presented for the American Section, International Astronomical Union

BY W. W. CAMPBELL, *Chairman,* AND JOEL STEBBINS, *Secretary*

The American delegation to the conference of interallied academies held in London in October, 1918, had proposed that the existing organizations relating internationally to one and the same subject, to Astronomy for example, should be succeeded by a single society so formulated as to serve the purposes of all the associations from which they should withdraw. This policy was approved by the conference. At meetings held later in Paris it was decided that representatives of Astronomy in the allied and associated nations should meet in Brussels in July, 1919, to create the International Astronomical Union.

The American delegation of astronomers was organized under the auspices of the National Research Council. The Council proposed that the delegates be chosen by a subsidiary organization known as the American Section of the (proposed) Astronomical Union; this Section for the present occasion to be constituted as follows:

National Academy of Sciences—5 members: W. W. CAMPBELL, GEORGE E. HALE, A. A. MICHELSON, F. R. MOULTON, FRANK SCHLESINGER.

American Astronomical Society—20 members: C. G. ABBOT, W. S. ADAMS, R. G. AITKEN, S. I. BAILEY, E. E. BARNARD, L. A. BAUER, BENJAMIN BOSS, E. W. BROWN, W. S. EICHELBERGER, E. B. FROST, J. F. HAYFORD, W. J. HUMPHREYS, W. J. HUSSEY, A. O. LEUSCHNER, S. A. MITCHELL, H. N. RUSSELL, C. E. ST. JOHN, F. H. SEARES, V. M. SLIPHER, JOEL STEBBINS.

American Mathematical Society—3 members: G. D. BIRKHOFF, W. D. MACMILLAN, R. S. WOODWARD.

American Physical Society—3 members: J. S. AMES, HENRY CREW, THEODORE LYMAN.

U. S. Coast and Geodetic Survey—1 member: WILLIAM BOWIE.

U. S. Naval Observatory—1 member: J. A. HOOGEWERFF.

The members selected later by these organizations are as listed above.

The American Section thus constituted held its first meeting in the office of the National Research Council, Washington, D. C., on March 8, 1919. Twenty of the thirty-three members were present. Mr. W. W. Campbell was appointed permanent Chairman of the Section, and Mr. Joel Stebbins, Secretary. An Executive Committee of the Section was appointed, consisting of the Chairman, the Secretary, and Messrs. C. G. Abbot, E. W. Brown, and Frank Schlesinger. The Section gave assent to the acts of the London Conference, and formally approved the proposal that those nations which had remained neutral throughout the war should be admitted into the International Astronomical Union promptly upon the conclusion of peace. The Section did not attempt to outline the form of an international organization which would promise the greatest usefulness, but concerned itself largely with the technical work of the proposed Union in which the American astronomers would be chiefly interested. The Executive Committee was instructed to appoint subcommittees of the American Section which should prepare reports upon nineteen prominent divisions of astronomical research, for presentation at a later meeting of the Section. The Brussels delegation was selected in part, and the Executive Committee was authorized to complete it.

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[1] Reprinted in Reprint and Circular Series of the National Research Council, Number 10.

The delegation was eventually composed as follows:

W. W. CAMPBELL, Lick Observatory, *Chairman;* W. S. ADAMS, Mount Wilson Observatory; BENJAMIN BOSS, Dudley Observatory, Albany; MAJOR PHILIP FOX, Dearborn Observatory, Evanston, at that time with the American Expeditionary Forces in France; S. A. MITCHELL, McCormick Observatory, Virginia; C. E. ST. JOHN, Mount Wilson Observatory; F. H. SEARES, Mount Wilson Observatory; FRANK SCHLESINGER, Allegheny Observatory, Pittsburgh; JOEL STEBBINS, University of Illinois, *Secretary.*

The special subcommittees were in due time constituted, as follows:

Committee on the Variation of Latitude.—F. B. LITTELL, *Chairman,* A. O. LEUSCHNER, FRANK SCHLESINGER.

It was voted to ask the American Section of the International Geophysical Union to appoint a similar committee to confer and make a joint recommendation on the most promising method of continuing the variation of latitude work.

Committee on Standards of Wave-Length.—HENRY CREW, *Chairman,* H. D. BABCOCK, KEIVIN BURNS, W. W. CAMPBELL, C. E. ST. JOHN.

Committee on Solar Rotation.—C. E. ST. JOHN, *Chairman,* W. S. ADAMS, FRANK SCHLESINGER.

Committee on Eclipses.—S. A. MITCHELL, *Chairman,* E. E. BARNARD, H. D. CURTIS.

Committee on Stellar Classification.—H. N. RUSSELL, *Chairman,* W. S. ADAMS, MISS ANNIE J. CANNON, R. H. CURTISS.

Committee on Asteroids and Comets.—A. O. LEUSCHNER, *Chairman,* E. W. BROWN, G. H. PETERS.

Committee on Almanacs.—W. S. EICHELBERGER, *Chairman,* E. W. BROWN, R. H. TUCKER.

Committee on Radial Velocities.—W. W. CAMPBELL, *Chairman,* W. S. ADAMS, J. S. PLASKETT.

Committee on Double Stars.—R. G. AITKEN, *Chairman,* ERIC DOOLITTLE, W. J. HUSSEY.

Committee on Notation, Units, and Economy of Publication.—W. J. HUMPHREYS, *Chairman,* E. B. FROST, A. O. LEUSCHNER.

Committee on Meridian Astronomy.—BENJAMIN BOSS, *Chairman,* F. B. LITTELL, FRANK SCHLESINGER.

Committee on Abstracts and Bibliographies.—F. E. FOWLE, *Chairman,* H. D. CURTIS, G. S. FULCHER.

Committee on Research Surveys.—G. E. HALE, *Chairman,* F. R. MOULTON, HARLOW SHAPLEY.

Committee on Stellar Photometry.—F. H. SEARES, *Chairman,* S. I. BAILEY, F. C. JORDAN, J. A. PARKHURST, JOEL STEBBINS.

Committee on Wireless Determination of Longitude.—J. A. HOOGEWERFF, *Chairman,* W. W. CAMPBELL, J. J. CARTY. This committee was requested to study the feasibility of determinations of longitude by wireless at widely distributed stations, and report on what seems to be the proper time and method for such undertakings.

Committee on Solar Radiation.—MR. C. G. ABBOT was asked to prepare a report on solar radiation.

Committee on the Spectroheliograph.—The Mount Wilson Observatory was asked to prepare a report on work with the spectroheliograph.

Committee on Reform of the Calendar.—R. T. CRAWFORD, *Chairman,* W. W. CAMPBELL, HAROLD JACOBY.

Another subcommittee was appointed at a later date, as follows:

Committee on Stellar Parallaxes.—FRANK SCHLESINGER, *Chairman,* W. S. ADAMS, S. A. MITCHELL.

The American Section met again in the office of the National Research Council on June 23 and 24, to receive and act upon the reports of the technical subcommittees, to consider questions of policy, and to instruct the delegation which would represent the Section at Brussels. Twenty members of the Section were present. The reports of the subcommittees, presenting the aspects of their subjects which were then prominent in the minds of astronomers, and suggesting promising procedure for the immediate future, were of a high order of excellence. They are appended to this report in the form in which they were adopted by the Section. The discussions, conducted with enthusiasm and frankness, were extremely valuable.

A joint meeting of the American Section with the proposed International Geodetic and Geophysical Union (Major William Bowie, Chairman) was held on June 24.

The delegation of astronomers sailed from New York on June 30 and reached London on July 7. The next ten days were devoted profitably and with unusual pleasure to renewing old scientific acquaintances and making many new ones, to learning of progress in astronomy and the related sciences made in Great Britain during the war period, to discussing with our British colleagues the many problems coming up for consideration at the Brussels Conference, etc.

The President and Council of the Royal Astronomical Society had most kindly arranged by cable that the members of the American delegation should attend and address a special meeting of the Society on the afternoon of July 11. At this meeting, under the presidency of Professor Fowler, each member of our astronomical delegation, and likewise Dr. L. A. Bauer, Secretary of the Geophysical Section, addressed the Society informally on the scientific subject which at that time especially interested him. This meeting had been preceded by a reception in the rooms of the Society, which gave opportunity to meet many British men and women whose names had long been familiar.

Opportunities to observe the equipment and work of the Royal Observatory at Greenwich, of the University observatories at Cambridge and Oxford, and of the laboratory of Professor Fowler in South Kensington were provided by members of their staffs.

In common with many British colleagues, the American delegation journeyed from London to Brussels on July 17. The Conference lasted from July 18 to July 28, inclusive. Thirty-three astronomical delegates were present: eight from Belgium, eight from Great Britain, seven from France, one from Italy, and nine from the United States.

The most difficult question confronting the International Astronomical Union related to its composition. Should it be organized as a great astronomical society, comprehending all divisions of astronomical science in a somewhat general or indefinite manner, or should it consist of working committees, each committee concerned with some definite line of astronomical research? Should there, for example, be a great division of the Union relating to the solar system, or, on the contrary, several comparatively small committees occupying themselves responsibly with such definite subjects as solar rotation, eclipses, solar radiation, etc.? Should there be another great division of the Union relating to the sidereal system, or, on the contrary, several comparatively small committees occupying themselves responsibly with such definite subjects as stellar parallaxes, radial velocities of the stars, double stars, variable stars, etc.? The latter plan was adopted. To begin with, thirty-two such special committees were established. These vary in size in accordance with the requirements and present states of the problems concerned. The Committee on the Carte du Ciel and the Committee on Meridian Astronomy are relatively large, with more than a dozen members each, whereas other committees, such as those on Lunar Nomenclature, on Solar Radiation, on Astronomical Ephemerides, etc., are relatively small, with not more than half a dozen members each. The separate committees are working bodies. They are intended to be essentially complete in themselves. The Union as a whole will meet in general every three years in different educational centers of the world, but it is expected that the members of a given committee will find many occasions between general meetings when conferences by letter or otherwise may profitably occur. Committee reports will doubtless aim to represent the views of the committees as to the current status of their subjects, and to present practical suggestions for further undertakings. In other words, it is anticipated that each committee shall make known through its report, presented in a general session of the Union, those aspects of its subject with which the workers in that subject may most profitably acquaint and busy themselves.

Each committee has power to add to its numbers by favorable vote of two-thirds of the current membership of the committee.

It is provided in the Constitution that the committees shall automatically go out of existence at the close of each triennial meeting of the Union. This offers the opportunity for reorganization upon the basis of those who work.

The Constitution of the Union, as distributed from the office of the General Secretary of the International Research Council, follows this report together with the Regulations of the International Time Commission and of the International Central Bureau for Astronomical Telegrams, which were adopted by the Union.

The number of votes and the relative financial obligations of each adhering country are fixed by a scale identical with that which governs these subjects in the International Research Council. The unit of financial contribution for the United States during the first three years of the convention may not surpass 1,500 francs annually. In this connection we should note the fact that, upon the recommendation of the American delegates representing Astronomy and Geophysics, the administration of the latitude variation problem is to be transferred to the International Astronomical Union. The financial obligation of the United States in support of this work is one of the larger items entering into the financial budget of the Astronomical Union.

The budget of the Astronomical Union for subsequent years shall be determined in a manner precisely parallel to that adopted for the Research Council.

The following officers of the Astronomical Union were elected in general session:

President, M. BENJAMIN BAILLAUD, Director of the Paris Observatory.

General Secretary, ALFRED FOWLER, F.R.S., Professor of Astrophysics, Imperial College of Science and Technology, South Kensington, London.

Vice-Presidents, W. W. CAMPBELL, Director of the Lick Observatory; SIR F. W. DYSON, the Astronomer Royal, Greenwich, London; M. GEORGES LECOINTE, Director of the Royal Observatory of Belgium, Brussels; M. ANNIBALE RICCÒ, Director of the Observatory of Mt. Etna, Sicily.

The fifth vice-presidency was left vacant for the present, pending the adhesion of countries which had remained neutral during the war. The officers here listed compose the Executive Committee of the Union.

The technical committees are as follows:

1. *Committee on Relativity.*—EDDINGTON, *Chairman*, ARMELLINI, BRILLOUIN, H. D. CURTIS, DE DONDER, ISHIWARA, JEANS, LEVI-CIVITA, E. PICARD.
2. *Committee on the Republication of Early Astronomical Works.*—DREYER, *Chairman*, BIGOURDAN, BIRKENMAYER, CREW, KNOBEL, MASCART, TERAO.
3. *Committee on Notation, Units, and Economy of Publication.*—STROOBANT, *Chairman*, BIGOURDAN, BOSS, DESLANDRES, DYSON, FROST, TURNER.
4. *Committee on Ephemerides.*—COWELL, *Chairman*, ANDOYER, BROWN, DYSON, EICHELBERGER.
5. *Committee on Abstracts and Bibliography.*—BAILLAUD, *Chairman*, BIGOURDAN, H. D. CURTIS, FOWLER, KNOBEL, MILLOSEVICH, STROOBANT.
6. *Committee on Astronomical Telegrams.*—LECOINTE, *Chairman*, BAILEY, BAILLAUD.
7. *Committee on Dynamical Astronomy and Astronomical Tables.*—ANDOYER, *Chairman*, BROWN, COWELL, GLAISHER, HAMY, LEUSCHNER, MOULTON, SAMPSON.
8. *Committee on Meridian Astronomy (including Refraction).*—HOUGH, *Chairman*, ANTONIAZZI, DE LA BAUME PLUVINEL, BIGOURDAN, BOSS, DYSON, EDDINGTON, GONNESSIAT, HAMY, DI LEGGE, LITTELL, PHILIPPOT, PORTER, TUCKER, (Australian).

9. *Committee on Theoretical and Applied Optics and the Study of Instruments.*—HAMY, *Chairman,* CARTY, CONRADY, COTTON, DELVOSAL, FABRY, LITTELL, LUNN, SAMPSON.

10. *Committee on Solar Radiation.*—ABBOT, *Chairman,* BEMPORAD, CALLENDAR, DESLANDRES, FOWLE, GORCZYNSKI.

11. *Committee on the Spectro-Helio-Enregistreur.*—DESLANDRES, *Chairman.*

12. *Committee on the Solar Atmosphere.*—HALE, *Chairman,* DESLANDRES, EVERSHED, FOWLER, HIRAYAMA, LEE, NEWALL, PÉROT, RICCÒ, ST. JOHN.

13. *Committee on Astronomical Expeditions, Eclipses, etc.*—DE LA BAUME PLUVINEL, *Chairman,* ANGOT, BARNARD, CAMPBELL, DESLANDRES, FOWLER, HINKS, MITCHELL, PALAZZO, RICCÒ.

14. *Committee on the Determination of Wave-Lengths and Tables of the Solar Spectrum.*—ST. JOHN, *Chairman,* BABCOCK, FABRY, FOWLER, HAMY, MEGGERS, MERTON, NAGAOKA, NEWALL, PÉROT, WRIGHT.

15. *Committee on Solar Rotation.*—NEWALL, *Chairman,* ADAMS, DESLANDRES, EVERSHED, DELURY, PÉROT, SAMPSON, ST. JOHN.

16. *Committee on Physical Observations of the Planets.*—CERULLI, *Chairman,* BARNARD, DESLANDRES, JARRY-DESLOGES, LAMPLAND, MASCART, PHILLIPS, STROOBANT, H. THOMSON.

17. *Committee on Lunar Nomenclature.*—TURNER, *Chairman,* BIGOURDAN, MISS BLAGG, PICKERING, PUISEUX.

18. *Committee on the Determination of Terrestrial Longitude by Wireless Telegraphy.*—GENERAL FERRIÉ, *Chairman,* BIGOURDAN, CARTY, DELPORTE, DYSON, HAMY, HOOGEWERFF, HOUGH, NAKANO, SAMPSON.

19. *Committee on the Variation of Latitude.*—KIMURA, *Chairman,* DE LA BAUME PLUVINEL, BIANCHI, BIGOURDAN, BRILLOUIN, CELORIA, HILLS, JEFFREYS, JONES, LARMOR, LITTELL, REINA, ROSS, SCHLESINGER, VOLTA.

20. *Committee on Asteroids.*—BROWN, *Chairman,* ANT. ABETTI, ANDOYER, BIGOURDAN, BOURGET, COWELL, CROMMELIN, FAYET, GONNESSIAT, KRASSOWSKI, LEUSCHNER, METCALF, MILLOSEVICH, STROOBANT.

21. *Committee on Comets.*—LEUSCHNER, *Chairman,* ANDOYER, DE LA BAUME PLUVINEL, BARNARD, CROMMELIN, DESLANDRES, FAYET, FOWLER,

22. *Committee on Meteors.*—DENNING, *Chairman,* DESLANDRES, LEBEUF, MITCHELL, OLIVIER, MRS. WILSON.

23. *Committee on the Carte du Ciel.*—TURNER, *Chairman,* BAILLAUD, BALBI, BALDWIN, BEMPORAD, BHASKARAN, COOK, COSSERAT, CURLEWIS, DELVOSAL, DYSON, GONNESSIAT, HOUGH, LECOINTE, L. PICART, RICCÒ, SAMPSON, SCHLESINGER.

24. *Committee on Stellar Parallaxes.*—SCHLESINGER, *Chairman,* G. ABETTI, ADAMS, BIGOURDAN, COSSERAT, DAVIDSON, DYSON, FOX, MILLER, MITCHELL, RAMBAUT, VAN BIESBROECK.

25. *Committee on Stellar Photometry.*—SEARES, *Chairman,* BAILLAUD, EDDINGTON, HALM, MISS LEAVITT, MELOTTE, PARKHURST, SAMPSON, TURNER.

26. *Committee on Double Stars.*—AITKEN, *Chairman,* G. ABETTI, BIGOURDAN, DOOLITTLE, ESPIN, FOX, HUSSEY, JACKSON, JONCKHEERE, VAN BIESBROECK.

27. *Committee on Variable Stars.*—BAILEY, *Chairman,* BEMPORAD, BROOK, COSSERAT, JORDAN, MARKWICK, MASCART, PACE, PHILLIPS, PLUMMER, RUSSELL, SHAPLEY, STEBBINS, TURNER.

28. *Committee on the Nebulae.*—BIGOURDAN, *Chairman,* DESLANDRES, DREYER, FABRY, KNOX-SHAW, NEWALL, REYNOLDS, SLIPHER, WRIGHT.

29. *Committee on the Spectral Classification of Stars.*—ADAMS, *Chairman,* MISS CANNON, R. H. CURTISS, FOWLER, DE GRAMONT, HAMY, NEWALL, PLASKETT, RUSSELL.

30. *Committee on Stellar Radial Velocities.*—CAMPBELL, *Chairman,* ADAMS, DESLANDRES, HAMY, LUNT, NEWALL, PLASKETT.

31. *Committee on Time.*—SAMPSON, *Chairman*, BAILLAUD, BIGOURDAN, CELORIA, DYSON, GENERAL FERRIÉ, HOOGEWERFF, HOUGH, LECOINTE, PHILIPPOT.

32. *Committee on the Reform of the Calendar.*—CARDINAL MERCIER, *Honorary Chairman*, BIGOURDAN, *Chairman*, CAMPBELL, CRAWFORD, DESLANDRES, DYSON, LECOINTE, MILLOSEVICH, RICCÒ, SAMPSON.

Certain of the committees, such as those on Eclipses, on Time, etc., held meetings in Brussels promptly following their formation. It was deeply regretted that the work of organization was so time-consuming as to prevent nearly all of the committees from holding first meetings.

The invitation of the Italian delegates that the next meeting of the International Astronomical Union should be held in Rome in the year 1922 was accepted.

The earnestness and enthusiasm with which the work of organization was conducted, and the unanimity with which decisions were finally made, are the satisfactory indications that the Union will meet efficiently its purpose of promoting astronomical research through the coördinated efforts of the astronomers in many countries.

Respectfully submitted,
W. W. CAMPBELL, *Chairman*
JOEL STEBBINS, *Secretary*
American Section, International Astronomical Union.

INTERNATIONAL RESEARCH COUNCIL

INTERNATIONAL ASTRONOMICAL UNION

Statutes of Convention

I—OBJECTS OF THE UNION AND CONDITIONS OF ADMISSION

1. The purpose of the Union is:

(*a*) To facilitate the relations between astronomers of different countries where international coöperation is necessary or useful.

(*b*) To promote the study of astronomy in all its departments.

2. The admission of countries to the Union shall be subject to the Regulations of the International Research Council.

II—NATIONAL COMMITTEES

3. A National Committee shall be formed in each of the countries belonging to the Union. It shall be formed under the responsibility of the principal Academy of the country concerned, or of its National Research Council, or of some other national institution or association of institutions, or of its Government.

4. The functions of the National Committees shall be to promote and coördinate in their respective countries the study of the various branches of Astronomy, more especially in relation to their international requirements.

They shall be empowered to propose singly or jointly with other National Committees questions falling within the purview of the Union for discussion by the Union.

The National Committees shall nominate delegates to represent them at the meetings of the Union.

III—ADMINISTRATION OF THE UNION

5. The work of the Union shall be directed by the General Assembly of the delegates.

6. The Bureau of the Union shall consist of a President, not more than five Vice-Presidents, and a General Secretary, who shall be elected by the General Assembly,

and hold office until the end of the second General Assembly following that of their election; provided that the first President and three of the first Vice-Presidents (as determined by the drawing of lots) shall retire at the end of the first General Assembly following that of their election. The retiring members are re-eligible.

This Bureau shall be the Executive Committee of the Union.

The five Vice-Presidents shall be chosen so as to represent the different branches of Astronomy.

In the case of any vacancy occurring among its members, the Executive Committee shall have power to fill up such vacancy. Any person so appointed shall hold office until the next General Assembly, which shall then proceed to an election. The member thus elected shall complete the term of office of the person whose place he takes.

There shall be an Administrative Office, which, under the direction of the General Secretary, shall conduct the correspondence, preserve the archives, prepare and issue the publications sanctioned by the Union, and administer its funds.

IV—Standing Committees

7. The General Assembly shall appoint Standing Committees, for the study of special branches of Astronomy, the encouragement of collective investigations, and the discussion of questions relating to international agreements or to standardization.

The Standing Committees shall present reports of their work to the Union.

8. The President and the members of each Committee are elected by the General Assembly on the nomination of the Executive Committee of the Union. They hold office until the end of the next ordinary General Assembly, and are re-eligible.

When a Standing Committee is composed of members appointed partly by the Astronomical Union and partly by some other Union connected with the International Research Council, it shall have the power to elect its own President.

Standing Committees shall draw up their own by-laws. They may co-opt other members by a majority of two-thirds. Such co-opted members need not necessarily be delegates, but shall be subjects of one of the countries adhering to the Union.

9. With the sanction of the Executive Committee, a Standing Committee may issue its publications independently. A Standing Committee may entrust any part of its work to any national institution or to individuals.

V—The General Assembly

10. The General Assembly shall, as a rule, hold an ordinary meeting once every three years. The date and place of the meeting, unless determined by the General Assembly at its previous meeting, shall be fixed by the Executive Committee, and communicated at least four months beforehand to the adhering organizations.

11. For special reasons, the President of the Union, with the consent of the Executive Committee, may summon an extraordinary meeting of the General Assembly. He must do so at the request of one-third of the votes of the countries adhering to the Union.

12. All members of the National Committees may attend the meetings and take part in the discussions of the General Assembly without power of voting.

The President of the Union may invite scientific men, who are not delegates, to attend a meeting of the General Assembly, provided they are subjects of one of the adhering countries. Such invited guests may take part in the discussions, but shall have no power of voting.

Members of any committees who are not delegates (Article 8) shall have the right to attend those meetings of the General Assembly which deal with the subjects referred to the Committee.

13. The agenda of business to be transacted at a meeting shall be determined by the Executive Committee and communicated to the adhering organizations at least four

months before the first day of the meeting. No question which has not been placed on the agenda shall be discussed, unless a proposal to that effect be approved by one-half of the countries represented at the General Assembly.

VI—FINANCE AND VOTING POWER

14. The Executive Committee shall prepare an estimate of the budget for each of the years intervening between two General Assemblies. A Finance Committee, nominated for the purpose by the General Assembly, shall examine this estimate together with the accounts for the preceding years. This Committee shall submit separate reports on the two questions to the General Assembly, which, having considered these reports, shall fix the unit of contribution for the succeeding period. The contributions due from the adhering countries, and their corresponding voting powers, are determined by the population of the country according to the following scale:

POPULATION	NO. OF VOTES	NO. OF UNITS OF CONTRIBUTION
Less than 5 millions	1	1
Between 5 and 10 millions	2	2
Between 10 and 15 millions	3	3
Between 15 and 20 millions	4	5
Over 20 millions	5	8

Each country is at liberty to include in its population the inhabitants of its colonies or protectorates, their number being that indicated by its Government. Self-governing Dominions (South Africa, Australia, Canada, New Zealand) have separate voting power according to the above scale.

The contribution to be levied during the first period of the convention shall not exceed 1,500 francs per annum per unit.[1]

In each country the organization adhering to the Council shall be responsible for the payment of the contribution.

15. The income of the Union is to be dovoted to:

(*a*) Cost of publication and expenses of administration.

(*b*) Cost of reduction and discussion of observations, including the payment of assistants.

Funds derived from donations shall be used by the Union, in accordance with the wishes expressed by the donors.

If any country withdraws from the Union it resigns at the same time its rights to a share in the assets of the Union.

16. At the General Assemblies votes upon scientific questions shall be decided by a majority of votes cast by the delegates present and voting. In questions of administrative character, and all questions not purely scientific, the vote shall be taken by countries, each country having the number of votes designated in Article 14. In case of doubt as to the category to which a question belongs, the Chairman of the meeting shall decide. Votes taken in Committee shall be counted individually and not by countries. When there is an equal division of votes, the Chairman shall in all cases have a second or casting vote.

17. In questions of an administrative character which appear on the agenda a country not represented at a meeting may forward its vote to the President by post, and such vote shall be counted if received before the vote is taken.

[1] The value of the franc shall be that of the French currency.

VII—REGULATIONS

18. The General Assembly may draw up regulations for the conduct of its business, the general duties to be assigned to the officers of the Union, and all matters not provided for in the Convention.

Each standing Committee may draw up its own regulations, which shall be approved by the General Assembly before coming into force.

No regulation shall be made, either by the General Assembly or by the Standing Committees, which violates the term of the present Convention.

VIII—DURATION OF CONVENTION AND MODIFICATIONS

19. The present Convention shall remain in force until December 31, 1931. It may then, with the assent of adhering countries, be continued for a further period of twelve years.

20. No change shall be made in the terms of the Convention except with the approval of two-thirds of the votes of the adhering countries.

21. The French text (of which the above is a translation) shall be considered to be the authoritative text.

INTERNATIONAL RESEARCH COUNCIL

INTERNATIONAL ASTRONOMICAL UNION

International Time Commission

REGULATIONS

1. An International Time Commission shall be formed for the purpose of unifying time by means of radio-telegraphic or other signals, whether these are scientific signals of great precision, or ordinary signals adapted to the needs of navigation, meteorology, seismology, railways, postal and telegraphic services, public administration, clockmakers, individuals, etc.

2. The domicile of the Commission shall be located at Paris.

3. The Bureau shall consist of a President, a Vice-President, and a General Secretary; the latter two shall be elected by the Commission itself. The General Secretary, whose tenure of office shall terminate on December 31, 1931, shall fulfil the functions of Director of the International Time Bureau.

4. The purpose of the International Time Bureau, located at the National Observatory of Paris, is:

(*a*) As regards ordinary signals, to harmonize the determinations of universal time, expressed in Greenwich Time, which shall be transmitted to them by the National Time centers. These shall be responsible for calculating, with the highest obtainable precision, the mean time, as deduced from determinations made by the Observatories of their respective countries. These results shall be sent as quickly as possible to the emitting stations and to the National Centers.

(*b*) As regards scientific signals of precision, to harmonize the time determinations made by the associated observatories, and deduce from them the most exact time.

The International Time Bureau shall publish the results of its comparisons. Any result whose publication is delayed shall be communicated in detail to all scientific associations and official institutions which may send in a request to that effect.

5. In addition to the Director, the International Time Bureau shall consist of:

(*a*) Scientific collaborators, nominated and dismissed by the Commission, on the proposal of the Director of the International Bureau. They shall be charged, with or without indemnification, with the conduct of special studies. Their term of office shall not exceed two years, but they shall be re-eligible.

(*b*) Scientific assistants, nominated and dismissed by the Director of the International Bureau. They shall be responsible for the carrying out of the programme drawn up by the Commission. Their term of office shall not exceed four years, but they shall be re-eligible.

The budget shall determine the sums allocated to each of these classes.

6. The Director of the International Bureau shall publish the results of the work undertaken in accordance with the decisions of the Commission. He shall present to the President an annual report covering the whole field of activity of the Bureau, together with the programme of work for the following year. This report and programme shall be published.

In his capacity of General Secretary, he shall, in consultation with the President, conduct the correspondence and administer the funds. He shall be responsible for the custody of documents.

7. The Director of the International Bureau shall prepare the estimate for the budget, which, after having been approved by the President and the Commission, shall be submitted to the International Astronomical Union.

8. The income of the Commission shall be devoted to:

(*a*) Covering the expenses of administration and publication.

(*b*) Honorarium to the General Secretary as Director of the International Time Bureau.

(*c*) Grants or remunerations for calculations, observations, or experiments undertaken at the request of the Commission.

(*d*) The purchase and upkeep of material for the International Time Bureau.

The Director of the Bureau, under the control of the International Astronomical Union, shall be responsible for the expenditure allocated to the above objects.

9. The above regulations shall remain in force until December 31, 1931.

INTERNATIONAL RESEARCH COUNCIL

INTERNATIONAL ASTRONOMICAL UNION

International Central Bureau for Astronomical Telegrams

REGULATIONS

1. The purpose of the Central Bureau is to receive, centralize, and dispatch information concerning astronomical discoveries, observations, and calculations, either by telegram or by post to the various institutions or private persons subscribing to it.

2. The domicile of the Bureau shall be located at the Royal Observatory of Belgium, at Uccle; the Chief of this Institution shall act as its Director.

3. The Bureau shall be placed under the control of the Standing Committee on Astronomical Telegrams.

4. Observatories and similar institutions may subscribe at the Central Bureau for Astronomical Telegrams. The general expenses of the Central Bureau shall be divided equally between the subscribers. At the beginning of each year the Standing Committee on the proposal of the Director, shall fix the annual subscription, which shall not exceed 25 francs (French currency), payable in advance. These funds shall be used exclusively for the expenses of printing the circulars and the correspondence. In addition to his subscription each subscriber shall cover the expenses of all telegrams addressed to him.

5. A statement of all expenses shall be made at the end of each year.

6. Subscribers shall give priority to the Central Bureau in respect of information on their discoveries, observations, or important calculations. Expenses incurred in the dispatch of telegrams for this purpose shall be refunded to the senders.

7. The Central Bureau shall take the necessary measures to reach its subscribers in all parts of the world. If necessary, special agreements shall be made for this purpose with observatories or secondary centers situated in other continents which may serve as relays.

8. The Central Bureau shall make every effort to reduce the expenses of transmission to a minimum; it shall continue to employ, for this purpose, the numeric code that has been in use for several years.

NOTES

A. Telegrams sent to the Central Bureau are to be addressed:

ASTRA BRUXELLES

B. Subscribers are requested to inform the Director of the Royal Observatory of Belgium, Uccle, of the SHORTEST TELEGRAPHIC ADDRESS to which messages conveying notices of discoveries should be sent in order to reach them.

REPORTS OF COMMITTEES OF THE AMERICAN SECTION OF THE INTERNATIONAL ASTRONOMICAL UNION

REPORT OF THE COMMITTEE ON NOTATION, UNITS, AND ECONOMY OF PUBLICATION

The Committee on Notation, Units, and Economy of Publication has given careful consideration to these subjects, and, although it has no recommendations it wishes to urge upon the Union, it begs to submit the following remarks in support of the appointment of a similar international committee.

I. UNITS

The multiplicity of units, applying to the same kind of measurement, that are now used in astronomical literature, and the indefiniteness that attaches to some of them, suggest the need of a careful study of this subject and international agreement as to what units should, as a rule, be adopted.

Astronomical distances, for instance, are already expressed in terms of the kilometer, mile, "astronomical unit" (mean solar distance), light-year, "parsec" (distance corresponding to a parallax of 1″), siriometer (corresponding to a parallax of 0.″2), "the unit" (corresponding to a parallax of 0.″1), megaparsec = 10^6 parsecs, and, perhaps, a few other units.

Although no other astronomical quantity is measured in so many different units as the stellar distance there are some whose numerical values are more confusing because the name of the unit employed has at least a double meaning. Thus the "absolute magnitude" of a star may refer to its brightness in terms of some supposed constant; or to that brightness which the star under discussion would show at a distance of 10 parsecs. Similarly, the unit of heat commonly used in certain astronomical discussions, the calorie, often leaves one in doubt as to which calorie is meant, the small or the large.

Obviously, then, there is need for general agreement in respect to the units to be used.

The following suggestions are offered for discussion:

1. *Distance.*—So far as practicable let the unit distance be, *a*, the kilometer (in reference to dimensions of sun, planets, etc.). It can even be used in expressing stellar distances without employing larger numbers than the chemist and the physicist are accustomed to in speaking of the number of molecules in a cubic centimeter of gas), *b*, the "astronomical unit" (in reference to the solar system chiefly), *c*, the light-year (especially in popular articles), and *d*, the parsec (wherever appropriate), or preferably a unit 10 times as great, to be given a separate name.

2. *Absolute magnitude.*—The meaning given to the expression "absolute magnitude" should be the magnitude at the distance of 10 parsecs, at which the star's parallax would be 0.″10.

3. *Unit of heat.*—Use the joule for the unit of heat instead of calorie, as the latter is not definite unless explained.

4. *Unit of wave-length.*—Use the international angstrom.

5. *Unit of pressure.*—One bar = 10^6 dynes per square centimeter. This is in keeping with very general use in meteorology.

II. NOTATION

It is only right that considerable latitude should be allowed the author in the choice of notation, nevertheless some of the existing confusion could and should be avoided. There is no uniformity, for instance, in the notation for the elements of a visual binary system. Also different methods are used in designating the brighter stars—by constellations and Greek letters, and by right ascension and declination. Again, the notation used in celestial mechanics, though difficult to standardize, appears to be more confused than necessary.

Suggestions

1. *Binary systems.*—Use a common notation—Aitken's, perhaps.

2. *System of units.*—If metric units are used do not add the English equivalents; if English units are used add, generally, the metric equivalents.

3. *Wave-length system.*—Use the abbreviations *I S* and *R S*, and not *I A* and *R A*. Do not use the diacritical mark over either the abbreviation *A* or the word angstrom.

4. *Unit of acceleration.*—A short name is needed for the C. G. S. unit of acceleration.

Additional suggestions in reference to notation will be made in connection with "economy of publication."

III. ECONOMY OF PUBLICATION

That there is need for economy in amount of publication, in cost of publication, and in manner of publication so as to save the time and effort of the reader, are all too evident to admit of question. The problem is how can these results be best attained.

Suggestions

1. *Observatory bulletins.*—There should be as few bulletins and as little published in them as circumstances will permit. It is better to publish in standard journals and purchase reprints.

2. *References.*—A nearer approach to standardization of references to journals, etc., than now exists should be adopted—librarian's method perhaps.

3. *Standard kinds of work.*—So far as practicable all results obtained in any one standard line of work should be presented in the same form.

4. *Summaries.*—Most articles should end with concise and accurate summaries.

5. *Catalogues.*—Catalogues of scientific literature should give concise abstracts.

6. *Economy in typesetting.*—Manuscript should be kept as free as practicable from all letters, symbols, and combinations that worry the printer or annoy the reader. Greek letters, for instance, are expensive luxuries—a dime per letter being the estimated added cost of publication. Similarly, subscripts, superscripts, inferior bars, superior bars, vertical fractions, etc., should be avoided as far as practicable.

The following recent suggestions by the London Mathematical Society are excellent:

Instead of	Always print
$\sqrt{2}, \frac{1}{\sqrt{2}}, \sqrt{13}$	$\surd 2$ or $2^{1/2}$, $1/\surd 2$ or $2^{-1/2}$, $\surd 13$ or $13^{1/2}$
$\sqrt{ax^2 + 2bx + c}$	$\surd(ax^2 + 2bx + c)$ or $(ax^2 + 2bx + c)^{1/2}$
$\sqrt{\frac{a}{b}}$	$\surd(a/b)$ or $(a/b)^{1/2}$
$\sqrt{-1}$	ι or i
$n.\overline{n + 1}.\overline{n + 2}$	$n(n + 1)(n + 2)$
$(\underline{\lvert n})^2, \underline{\lvert n + 1}, \underline{\lvert 2n}, 2^n\underline{\lvert n}$	$(n!)^2, (n + 1)!, (2n)!, 2^n.n!$
$\dot{x}, \dot{y}, \dot{r}, \ddot{\theta}$	x', y', r', θ'' (by preference)
$\frac{a}{2}, \frac{a + b}{3}, \frac{a + b + c}{4}$	$\frac{1}{2}a, \frac{1}{3}(a + b), \frac{1}{4}(a + b + c)$
$\frac{a + b}{c}, \frac{a}{b + c}, b + c$	$(a + b)/c, a/(b + c), a/b + c$

$\frac{p}{q}, \frac{p}{q} + \frac{r}{s}$	$p/q, p/q + r/s$ (in current text)
$\frac{p}{q + r/s}$ or $p/\left(q + \frac{r}{s}\right)$	$p/(q + r/s)$
$\frac{1}{x}, \frac{1}{x^n}$	$1/x$ or x^{-1}, $1/x^n$ or x^{-n}
$\frac{p + \frac{q}{2}}{\frac{r}{3} + \frac{s}{4}}, \frac{x}{y + \frac{z}{t}}$	$\frac{p + \frac{1}{2}q}{\frac{1}{3}r + \frac{1}{4}s}, \frac{x}{y + z/t}$
$e^{-\frac{n\pi x}{l}}, e^{-\frac{x^2}{4kt}}$	$e^{-n\pi x/l}, e^{-x^2/4kt}$
$\frac{1\pi}{a}, \int_0^{\frac{\pi}{2}}, \int_0^{\frac{\pi}{n}}$	$1\pi/a, \int_0^{\frac{1}{2}\pi}, \int_0^{\pi/n}$
$(2/l)\int_0^l \sin(r\pi x/l)\sin(s\pi x/l)dx$	$\frac{2}{l}\int_0^l \sin\frac{r\pi x}{l}\sin\frac{s\pi x}{l}\,dx$*

A few of the above expressions might, perhaps, be written in still better forms. Furthermore, it is quite possible to write an integral with limits in a more condensed and more convenient form than it usually has. Thus

Instead of	Write
$\int_b^a x^2dx$	$\int [a,b]x^2dx$

The first, or usual, form occupies, vertically, the space of two lines. The proposed form occupies but one line.

W. J. HUMPHREYS, *Chairman*, EDWIN B. FROST, A. O. LEUSCHNER.

REPORT OF THE COMMITTEE ON ALMANACS

The Committee on Almanacs recommends to the American Section that its delegates to the Brussels meeting of July 18, 1919, be instructed to endeavor to secure the adoption by the International Astronomical Union, of the Report of the Committee on Beginning the Day at Midnight adopted by the American Astronomical Society in August 1918.

W. S. EICHELBERGER, *Chairman*, E. W. BROWN, R. H. TUCKER.

REPORT OF THE COMMITTEE ON MERIDIAN ASTRONOMY

The experience gained through the application of photography to the determining of stellar positions demonstrates that it is advantageous to secure star positions, as far as possible, by photographic methods. This leads to the suggestion of the following meridian program, which is designed to provide the necessary foundation for photographic astrometry.

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* It may be noted that an integral with limits always involves a double line; so that the ordinary notation for fractions involves no additional use of "spaces" beyond those required for the integral in any case.

The suggested plan, in brief, is to adopt 3000 fundamental stars as the initial basis of the system, to build about 40,000 reference stars upon this and then by photographic means to continue a list of reference stars to fainter magnitudes. The details of the plan follow.

I. STANDARD STARS

It is suggested that the list of fundamental stars compiled in 1910 by the Committee on Fundamental Stars of the Astrographic Chart, and published in the supplement to the Connaissance des Temps for 1914, be adopted.

II. PHOTOGRAPHIC REFERENCE STARS

As a secondary step toward providing a suitable structure upon which to base photographic plate determinations of stellar positions it is suggested that a list of stars be constructed, approximately one in each square degree, and included as far as possible between visual magnitudes 8.0 and 9.0. The list should favor the stars of the intermediary and reference lists of the Astrographic Chart, but in any event stars which have been previously observed should be selected.

III. DIFFERENTIAL MERIDIAN OBSERVATIONS

It is recommended that differential observations be undertaken in zones 20° wide, four observations being taken of each star, two in each position of the clamp. All the fundamental stars in each zone should be observed, together with a sufficient number of fundamental stars at varying declinations to link the zone with the fundamental system.

IV. REVISION OF STANDARD SYSTEMS

Because of the rapidity with which the weight of a star position diminishes, it is recommended that standard systems be revised every fifteen to twenty years, and that the results be incorporated in the ephemerides as they become available.

V. QUESTIONNAIRE

In order to disseminate information in compact form among the meridian observers, it is recommended that 6 months before each meeting of the International Astronomical Union the following questionnaire be sent to each observatory and that the reports be collated and published as part of the proceedings of the meeting.

1. Brief description of meridian work in hand, and estimated time of conclusion.
2. Instruments, character, size, etc.
3. Equipment:

(*a*) Collimators
(*b*) Mires
(*c*) Nadirs
(*d*) Impersonal right-ascension micrometers
(*e*) Personal equation apparatus
(*f*) Reversing prism on ocular—on microscopes
(*g*) Screens or other devices for magnitude
(*h*) Flexure—how determined
(*i*) Clock—how safeguarded from temperature and barometric changes
(*j*) Thermometer installation—use of whirling thermometers—Assman thermometers —or other devices
(*k*) Printing chronograph
(*l*) Devices for stabilizing mercury reflecting surfaces
(*m*) Methods of determining corrections for irregularities in the forms of the pivots

4. Remarks on any novel devices in use, with comments on the value of the same.
5. Reflected observations of stars. Are they made—how used?
6. Application of photography to meridian work.
7. Method of determining graduation errors.

VI. PUBLICATIONS

1. The present practice varies from publication of the minute detail of observations to that of giving only the final results. It is suggested that this matter be considered and that a recommendation be made in order on the one hand to save the great waste of material and effort involved in unnecessarily extended publication and on the other hand to secure the publication of as much detail as the consensus of opinion shall decide is actually desirable.

2. Uniformity should be observed in star names, in number of decimals for right-ascension and declination, and in use of declinations instead of north polar distances.

3. Standard equinoxes should be adopted.

4. No proper motions should be applied in observational catalogues to reduce from the epoch of observation to the date of equinox of the catalogue.

VII. SOUTHERN OBSERVATORIES

Attention is called to the need of more southern observatories for meridian work in view of the relative weakness of the positions of the southern stars.

BENJAMIN BOSS, *Chairman*, FRANK SCHLESINGER, FRANK B. LITTELL

REPORT OF THE COMMITTEE ON COÖPERATIVE WORK WITH THE SPECTROHELIOGRAPH

In several reports presented to the International Union for Coöperation in Solar Research, the advantages of photographing the sun frequently with the spectroheliograph at several stations widely separated in longitude have been pointed out. Without enumerating these, it is evident that a more nearly continuous record of the rapidly changing phenomena of the sun's atmosphere than we now possess would provide material for the study of many questions, such as the possible connection between solar eruptions and terrestrial magnetic storms.

Most of the telescopes hitherto used for this work give a very small solar image, while the spectroheliographs employed with them are in very few cases of sufficient dispersion to permit satisfactory photographs to be taken with the *Ha* line. As the hydrogen flocculi, for most purposes, are of much greater interest and importance than the calcium flocculi, the object of this report is to indicate the general nature of the instrumental equipment needed to record them in such a way as to render the resulting photographs fairly comparable.

The chief instrumental requirements are:

1. A telescope giving a large solar image, not less than 10 cm. and preferably from 15 to 20 cm. in diameter. This large scale is needed in order that the minute structure defining the fields of force of the hydrogen flocculi may be clearly recorded.

2. A spectroheliograph of sufficient dispersion to permit the hydrogen flocculi to be photographed with light confined to the central part of the *Ha* line. A linear dispersion of 1 mm. = 3.5 *A* will serve very well.

It is desirable, but not essential, that this spectroheliograph should be of sufficient aperture to permit the entire solar image to be photographed at once. If this is not feasible, a second objective, giving a smaller solar image (at least 5 cm. in diameter), should be so attached to the telescope that it can be readily substituted for the objective of greater focal length.

3. It is important that spectroheliograms intended for comparative studies of eruptive phenomena and changes in the structure of the hydrogen flocculi should not differ greatly, either in the size of the solar image or in the dispersion of the spectroheliographs with which they are taken. For this reason it is desirable that the optical constants of the instruments used at coöperating observatories should be as nearly alike as possible.

4. It is desirable that the spectroheliographs should be adaptable for the photography of the calcium flocculi with K_2 and with K_3. It would also be advantageous to design them for use as spectroregistreurs des vitesses (Deslandres) and for the study of the magnetic fields in sun-spots, in case spectrographs especially designed for these purposes are not available.

A final word should be said regarding the necessity for good atmospheric conditions, to permit the sun to be photographed on the largest possible number of days and to afford the perfection of definition required to reveal the minute details of the flocculi.

GEORGE E. HALE.

REPORT OF THE COMMITTEE ON ECLIPSES

(Abstract)

The Committee believes that it might be of assistance to astronomy along the following lines:

1. To advise as to what eclipse problems are most worthy of attack, and what equipment is needed for successful work.

2. For the eclipse of 1923, to collect well in advance all available information regarding possible eclipse sites, weather probabilities, transportation facilities, living conditions, etc. (The Mexican Meteorological Service has already been communicated with in regard to the eclipse of 1923.)

3. To publish such information and eclipse computations well in advance.

4. To advise intending observers as to location so as to secure a wide distribution of stations.

5. To gain information regarding the apparatus that is available at different observatories, and to arrange as a central clearing-house for the loan of apparatus to intending observers who have not adequate facilities.

A discussion is given of the various eclipse problems under the headings:

Photography of the corona.
  What is the corona?
  What causes the corona?
  Motion of the corona.
Polarized light in the corona.
Brightness of the corona.
Spectrum of the corona.
Wave-lengths of coronal lines and rotation of the corona.
Flash spectrum.
Einstein effect.
Times of contact.
Other eclipse problems.
Movie camera.

S. A. MITCHELL, *Chairman*, E. E. BARNARD, H. D. CURTIS.

REPORT OF THE COMMITTEE ON STANDARDS OF WAVE-LENGTH

1. It is recommended that the International Astronomical Union appoint at the earliest practicable date, a committee whose functions shall be essentially the same as those of the Committee on Wave-Lengths of the International Union for Coöperation in Solar Research.

2. *The primary standard.*—The remarkable sharpness of the red Cadmium line, the extraordinary skill with which the meter was evaluated in terms of this wave-length, and the final definition of the angstrom, as an arbitrary unit, by the international Solar Union at Paris in 1907, would seem to place the primary standard almost beyond the range of question.

However, the recent perfection of a new method for making end-standards of length, the comparative ease with which the optical length of these end-standards can be determined, and the possible superiority of some other line (such as the bright yellow Neon line) over one from solid Cadmium as a source of light combine to urge a reconsideration even of the primary standard.

Since a committee, consisting of Messrs. Ames, Anderson, Michelson and Stratton, has been appointed by the National Research Council to consider the practicability of adopting a wave-length of light as the fundamental standard of length, it is recommended that the question of a primary standard of wave-length be held in abeyance—that is, not reopened—until the above-mentioned committee makes its report.

In case the International Bureau of Weights and Measures adopts a certain wave-length of light as their standard of length, it would appear to follow, as a matter of course, that this wave-length should be adopted also by the Astronomical Union, and that the numerical value attached to it should be that assigned by the International Bureau of Weights and Measures.

As to the possible use of a Neon line, or the sharp component of the green Mercury line, or of a sharp line in the absorption spectrum of Iodine, for the fundamental unit of length, instead of the red Cadmium line, your committee feels that this is a matter belonging entirely to the Committee of the International Bureau of Weights and Measures.

3. *Secondary standards.*—A series of secondary standards is now being determined at the Bureau of Standards and at Mt. Wilson. The Committee recommends that the Astronomical Union encourage a third series of independent measures on these lines.

It is recommended, therefore, that the International Astronomical Union arrange, if possible, with European spectroscopists for a determination of these secondary standards in Iron, Neon, and other substances which can be worked under definite and easily reproducible conditions.

The dearth of Secondary Standards in the yellow-green region and in the infra-red appears to your committee an unsolved problem. Its solution will be the discovery of a workable and easily duplicated source of light which will furnish properly distributed lines of sufficient sharpness and intensity.

4. *Tertiary standards.*—Here the greatest need appears to be for lines to fill gaps in the ultra-violet, the yellow-green, and infra-red of Iron. With reference to this need your committee has no recommendation to make except to seek further information as to how the need may be met.

For the measurement of tertiary standards independent determinations by the interferometer and by the grating are urgently recommended, on the ground that the errors of these two instruments are largely complementary. The importance of this work is emphasized by the fact that it will be scarcely practicable to print a new "Table of Stronger Lines" for the identification of impurities and similar purposes until the tertiary standards are more complete than at present.

5. *Sources.*—In order to obtain lines of constant wave-length and constant intensity-distribution, it is recommended that the modification of the Pfund arc employed at Mt. Wilson be adopted, namely, the Pfund arc operated between 110 and 250 volts as convenient, with 5 amperes or less, at a length of 12 millimeters, and used over a central zone, at right angles to the axis of the arc, not to exceed $1^1/_4$ millimeters in width.

6. *Classification.*—It is recommended that tables of wave-lengths be accompanied by at least two descriptive columns, one indicating the inherent nature of the line (intensity, sharpness, asymmetry, etc.), the other indicating the character of the measures on the line. There is urgent need of some quantitative description of relative intensity as a function of wave-length.

For indicating the intrinsic character of the line, the modification of Gale and Adams' groups, shortly to be defined at Mt. Wilson, is recommended.

For indicating the degree of accuracy, the notation of Burns (*Lick Obs. Bulletin* No. 247, p. 29 and *Bulletin Bureau of Standards*, No. 274, p. 251) is recommended.

7. *Tables of wave-lengths.*—The time for an accurate and definitive determination of the wave-lengths of the different chemical elements does not yet seem to have arrived.

It is recommended, however, that a *Table of Stronger Lines* in which wave-lengths of all elements shall be arranged in order of magnitude be prepared at the earliest practicable date. It is believed that present needs call for a table containing approximately twice as many lines as Kayser's *Tabelle der Hauptlinien,* which includes ten thousand wave-lengths and occupies one hundred octavo pages.

8. *Survey.*—It is recommended that the conference at Brussels consider the possibility and advisability of making a survey of facilities, equipment, and opportunities for spectroscopic work in the various laboratories of the world, looking toward the avoidance of unnecessary duplication and the encouragement of coöperation in research.

HENRY CREW, *Chairman,* HAROLD D. BABCOCK, KEIVIN BURNS, W. W. CAMPBELL, CHARLES E. ST. JOHN.

REPORT OF THE COMMITTEE ON THE DETERMINATION OF SOLAR ROTATION BY THE DISPLACEMENT OF LINES

Since the Bonn meeting of the International Union for Coöperation in Solar Research in 1913, results of investigations of the sun's rotation have been published by a number of observers, namely, J. S. Plaskett, Hubrecht, Schlesinger, DeLury, Evershed and Royds, H. H. Plaskett, and St. John and Ware.

As the data relative to the linear velocity at the solar equator now cover a period of some twenty years, a review of the results at this time is of interest:

LINEAR VELOCITY OF SOLAR ROTATION AT THE EQUATOR

OBSERVER	VELOCITY	NO. OF LINES	REGION	DATE
Duner	2.08	2	6301–6302	1900–5
Halm	2.04	2	6301–6302	1904
Adams	2.06	20	4196–4294	1907
Adams	2.05	22	4196–4291	1908.5
Storey and Wilson	2.08	10	6280–6318	1909
Plaskett, J. S.	2.01	19	5506–5688	1911
Plaskett, J. S.	2.02	15	4196–4291	1911
DeLury	1.97	19	5506–5688	1911
Hubrecht	1.86	40	4299–4400	1911
Plaskett, J. S.	2.01	27	4250–5600	1911–12–13
Schlesinger	2.00	20	4058–4276	1912
Evershed and Royds	1.95		3906–5624	1913
Plaskett, H. H.	1.98	12	5574–5628	1913
St. John and Ware	1.94	35	4123–4338	1914
Plaskett, H. H.	1.95	5	5900	1915
St. John and Ware	1.94	26	5018–5316	1914–18
St. John and Ware	1.95	7	6265–6337	1916–17

In view of the accuracy attainable in spectro-photographic measures the differences between these results are surprising and disappointing and are the outstanding feature in the problem of solar rotation. They are larger than the probable errors of measurement, and either depend upon systematic errors of the measurers or they are of solar or terrestrial origin. The tracing of them to their source is the line along which investigation may be directed with great advantage.

Investigations now in progress at Mount Wilson, in which simultaneous observations are made upon the two limbs and the center of the sun, indicate that temporary and local conditions in the sun's reversing layer are frequently such as to produce differences of ten per cent in the rotation values obtained by comparing east and west limbs directly.

The observations show further that high values at one limb are not correlated with high values at the other as they would be if they were due to real changes in the rotation of the reversing layer or to conditions in the terrestrial atmosphere. In the light of these results it is clear that a short series of observations may give misleading results and it even becomes a question whether the solar rotation can be determined as definitely as has been thought. The means of extended series of observations taken under constant conditions should, however, give results of great value. Such a series at Mount Wilson Observatory is now in its sixth year. The observations show no definite evidence of periodicity in the sun's rotation from 1914 to 1919, but furnish strong indications of frequent local disturbances of a definite type in the sun's reversing layer, a subject still under investigation.

The formulae of Faye represent satisfactorily the relation of velocity to latitude as given by the observations of Duner, Adams, Plaskett and DeLury, and Schlesinger, but observations in the higher latitudes should be greatly extended both in number and in range of latitude. The failure of the Faye formula to represent Hubrecht's observations is probably due to the very limited time covered by them.

The only published results relating to a difference in the rotation law in the two hemispheres are those of Hubrecht. His earlier paper (1912) referred to in the report at the Bonn meeting showed an excess of the velocities in the southern over the northern hemispheres, but in the later publication (1915) covering the same epoch he comes to the opposite result basing his conclusions upon Tunstall's measurement of his plates to the exclusion of his own. The question of a difference between the two hemispheres is one of great interest and need not wait for solution until all discrepancies between observers are settled as it is a relative matter for any one observer and instrument. It, therefore, offers an inviting field of investigation at the present time, and is on the working program at Mount Wilson.

The different rates of rotation for different levels in the solar atmosphere shown by the Mount Wilson observations of Adams appear again in preliminary reductions of the present Mount Wilson series which show relatively high velocities for the strong magnesium triplet in the green, for 4227 and the *H* and *K* lines of calcium, but low values for the lines of lanthanum and of the nitrogen (cyanogen) band at 3883. The observations of Schlesinger published since the Bonn meeting agree with those of Plaskett and DeLury, Storey and Wilson, and Hubrecht in showing no positive evidence of differences in rotational velocity. It is to be noted that in the Mount Wilson observations lines differing widely in intensity, representing great differences in level, have been employed. As differences in rotational velocity must be directly related to differences in level, such lines, notwithstanding the difficulties of measurement, offer a valuable means of detecting it. The great majority of lines of medium intensity of the common elements originate in not greatly different levels, and for such lines differences in rotation and velocity would be small and correspondingly difficult to detect.

Conditions in the earth's atmosphere have been suggested by DeLury as possible causes of divergent results in observations for solar rotation. He finds evidence of the effect of an over-lapping sky spectrum in some of his observations at Ottawa, and attributed to it the large variations in rotational velocity at different times and all differences found for different elements and lines.

That a large proportion of superposed skylight might appreciably reduce line displacements at the sun's limb has been taken into consideration by all solar observers. Tests on Mount Wilson show that, under ordinary observing conditions, the integrated light just outside the solar image is not of sufficient intensity to affect the photographic plate during the exposure time employed for solar rotation plates; that it requires considerable cloudiness over and around the sun, conditions never approximated in regular work, to cause a measureable decrease in the line displacements at the limb; and that this large proportion of superposed sky-light produces no differential effects between strong and weak lines.

Recommendations for Program of Work

1. In order to trace to their source the systematic differences that are found in the value of the solar rotation by different observers, it is desirable that further study be given to possible sources of systematic errors and to determining the source of the differences found by the same observer upon plates taken at frequent intervals, differences of the same order of magnitude as those between different observers.

2. In the meantime it is desirable to carry on, when possible, continuous series of observations under constant instrumental conditions and by the same observer.

3. As it seems increasingly probable that local conditions in the reversing layer are frequent causes of divergent results, it is advisable to extend observations over a considerable period of time in investigation of such questions as variation with latitude or differences between the hemispheres.

4. Simultaneous observation on the center and the limb is recommended as a valuable means of checking results and of investigating the hemispheres separately.

We recommend the appointment of an International Committee on Solar Rotation determined by spectrographic methods, the four points above to be referred as suggestions to this committee.

C. E. ST. JOHN, *Chairman*, W. S. ADAMS, FRANK SCHLESINGER.

REPORT OF THE COMMITTEE ON WIRELESS DETERMINATION OF LONGITUDE

The Committee recommends:

1. That the American delegation to the Brussels meeting of the International Astronomical Union urge the desirability of a Division of the Union which shall promote in the most practicable manner a world-wide program of accurate radio determinations of differences of longitudes, on the basis of one general program to be supported by the inter-allied nations concerned.

2. That the American members of this Division or Committee of the Union should be selected by the American Delegation with reference

to their fitness for planning, directing, and carrying on the American Section of the inter-allied program.

3. That the selected primary stations be so distributed throughout both the southern and northern hemispheres as to cover, as uniformly as possible, the entire world, and to give suitable checks on the work.

4. That observatories and other related institutions, both government and private, be encouraged to participate by receiving the fundamental radio signals and making simultaneous time observations, so as to link up as many positions as possible with the primary stations.

5. That the governments controlling radio stations likely to be of use be asked to give their consent to and promises of assistance in the work.

6. That Greenwich, England, be so included in the plans that the differences of longitude between it and the primary stations will be accurately determined.

7. That the determination of the difference of longitude between any pair or pairs of primary stations be made by parties from one country which would select their instruments and work out and be responsible for the result; but that nothing in this recommendation would prevent a duplication of this work by observers from another country, and, where other checks were not provided, that such duplication be encouraged.

8. That in view of the many arrangements and preparations required to secure accurate and complete results, the earliest date practicable for an international determination of differences of longitudes is the winter of 1921–22.

J. A. HOOGEWERFF, *Chairman*, W. W. CAMPBELL.

Because of his absence, Mr. J. J. Carty did not see the report.

REPORT OF THE COMMITTEE ON VARIATION OF LATITUDE

The Committee on Variation of Latitude of the American Section of the proposed International Astronomical Union submits the following report, based upon the action taken at a joint meeting of this committee with a similar committee representing the American Section of the proposed International Geophysical Union held in Washington on April 29, 1919. There were present at the meeting Messrs. Bowie and Marvin representing the Geophysical Union and Messrs. Schlesinger, Leuschner, and Littell representing the Astronomical Union. Mr. Bowie acted as chairman and Mr. Littell as secretary of the meeting.

RECOMMENDATIONS

The following recommendations are made by the committee:

1. That the observations for variation of latitude, their reduction, and their publication be entrusted to the International Astronomical Union.

2. That the continuity of the observations by the present methods at the four international variation of latitude stations, Ukiah, California; Carloforte, Italy; Mizusawa, Japan; and Charjui, Turkestan be maintained, as far as practicable, and that the matter of utilizing the observations made at other stations be considered.

3. That the question of renewing variation of latitude observations in the southern hemisphere be considered.

4. That, inasmuch as no definite plan can be advanced for defraying the expense of the variation of latitude work until some international arrangement is formulated, the several nations maintain the stations within their domains, and that some provisional arrangement be made for caring for the records, reductions, and publications, pending a permanent organization of the work.

FRANK B. LITTELL, *Chairman*, A. O. LEUSCHNER, FRANK SCHLESINGER

REPORT OF THE COMMITTEE ON ASTEROIDS AND COMETS

Among the most important objects to be served by the discovery, observations, and by researches in general on comets and asteroids, are the following:

1. Testing of gravitation by comparison of theory and observation leading to deviation from the law (e.g., those shown by the moon).

2. Testing of gravitational theories as sufficient to represent their motions for some time in the past and future.

3. Distribution with reference to theories of formation and evolution.

4. Distribution of orbits with reference to theories of the stability of their motions.

5. Positions for photometric research.

6. Photometric research for testing of theories of causes of variation of light.

7. Theories of distribution tested by search for asteroids in regions indicated by theory.

8. Positions of asteroids recorded for future researches, the direction of which is at present unknown.

These ultimate aims involve:

Observations of position, visual and photographic;

Photometric observations, visual and photographic;

Methods of observation and reduction;

Researches in orbit methods and orbit determinations, preliminary and final, and ephemerides;

Methods of special and general perturbations, of varying degree of accuracy according to special requirements;

Researches in pure celestial mechanics (e.g., periodic solutions), etc., etc.

Complete bibliographies should be prepared for each line of work.

The Committee is of the opinion that much unnecessary effort and duplication could be avoided and a thorough foundation for future researches along the lines indicated above could be furnished by first of all bringing together all published material in a bibliography which would give in abstract form the results of past observations and investigations. Observations of positions, photometric observations, etc., would be listed merely by dates of observation and references. At the present time there is completely lacking a reference work for asteroids such as Galle's Comet List. The latter also requires to be brought up to date; several fragmentary continuations of Galle's list are in existence. Bauschinger's Statistik der Kleinen Planeten gives merely statistical information based on adopted asteroid orbits, without giving a complete history of the work done on the individual asteroids, including references to the dates of observations.

The research requirements along the various lines indicated above cannot be fully stated until a complete history and bibliography for comets and asteroids are available. Galle's list has been of incomparable value in this direction, particularly in the identification of comets. After the history and bibliography have been completed the research requirements along the various lines indicated above should be published.

Arrangements should be made by international coöperation for the publication of opposition ephemerides.

After the plans have been outlined in the rough and published, a census should be made of observatories and astronomers, for the purpose of securing volunteers for specific work of observation or research. The results of this census should be published, so that every astronomer will know to whom to turn for information of any kind regarding asteroids, and so as to avoid the present useless duplications, particularly of observation.

The general direction of these plans should be in charge of an international committee composed of the chairmen of the Committees on Comets and Asteroids of the various national sections.

As a first step toward the realization of the foregoing plans it is necessary to adopt a distinct classification for comets and asteroids. An example for a suitable asteroid classification is attached as Appendix A. This classification is based on the present state of observation and computation and on the scientific importance of the object. It at once enables the astronomer interested in a particular kind of research to select the object and the material needed.

The present designation of asteroids might be supplemented by using the letters *a*, *b*, *c*, etc., for easy recognition of the condition of the available observational material and of its orbit theory, an asterisk being added to designate those of scientific importance. These asteroids might be given a description to indicate the nature of their scientific importance, thus: P_1 = Trojan Group, p_2 = Hecuba Group, etc.; *v* for variable, *l* for libration, *e* for eccentricity, *i* for inclination, etc. The magnitude at unit distance might also be given. An investigator could thus pick out at a glance the asteroids of particular interest in his line of research.

After each asteroid has been properly classified and designated, cards should be completed in order of importance. This work should all be done under the auspices of the International Committee, provided such a committee adopts some plan as outlined here.

Ephemerides.—An international convention will have to be held for providing observations and preliminary orbit determinations for newly discovered planets and for carrying forward the computations. It would seem wise at this time for the international committee to canvas the different countries as to the share of burden each can undertake. At the outset probably different countries in the Union would care for the asteroids discovered in their own country, without attempting a complete duplication of the work now in progress at Berlin. In this connection, the names of the observatories and astronomers who have undertaken to secure the necessary and sufficient observations in the case of each planet and to carry forward the computation should be published.

It is very essential that an understanding be reached to secure the necessary and sufficient observations of a newly discovered object to secure a satisfactory orbit sufficient to locate the body of the next opposition and to avoid all unnecessary observations and computations.

After the International Committee has adopted definite plans in regard to the foregoing points national observatories and almanac offices will no doubt be in a position to care for such ephemerides as involve routine computation only. The whole work, however, should be organized under the auspices of the International Committee.

As there may be differences of opinion no definite plans are proposed until the different countries are able to state what assistance they can render.

It is desired that monographs be published for the benefit of various groups of research men by persons invited to undertake this task by the committee, or by volunteers.

Recommendations.—In particular, the Committee makes the following recommendations:

1. That the Executive Board of the National Research Council be requested to secure funds for the preparation of a history and bibliography of comets and asteroids, with the understanding that this work will be undertaken by the United States.

2. That the International Astronomical Union appoint an International Committee on Comets and Asteroids.

3. That the American Delegation to the International Astronomical Union submit to the International Committee the proposals of the American Committee on Comets and Asteroids for their approval.

4. That in particular a preliminary distribution of the necessary observational and computational activities among the countries represented in the Union be agreed upon, on the basis of such offers as may be received from the different countries.

5. That the American Delegation pledge that it will arrange for the care of the observational and computational work incident to comets and asteroids discovered in the United States.

A. O. LEUSCHNER, *Chairman*, E. W. BROWN, G. H. PETERS.

APPENDIX A

MEMORANDUM ON CLASSIFICATION OF PLANETS

Class a.—Asteroids for which accurate definitive orbits and general perturbations are available.

1. *Ordinary asteroids* which have no immediate scientific interest. Observations of these should be suspended, but approximate ephemerides should be provided for identification purposes, if accidentally observed.
2. *Important asteroids* of further scientific interest should be observed in accordance with prearranged coöperative plans with the object of securing sufficient observations for the scientific purposes in hand, and avoiding unnecessary multiplication of observations. Ephemerides should be published, the accuracy of the ephemerides in each case to depend on the research requirements for that case.

Class b.—Asteroids for which accurate definitive elements are available, but no general perturbations.

1. *Ordinary asteroids.* Same as *a* (1). Approximate general perturbations should be computed as a later part of an asteroid program, as opportunity presents itself, preferably only the principal terms in groups.
2. *Important asteroids.* Observations to be dispensed with until general perturbations have been computed, degree of accuracy to depend on particular scientific requirements in each case, then same as *a* (2).

Class c.—Asteroids for which satisfactory elements are not available, but which have been sufficiently observed for the determination of satisfactory osculating elements.

1. *Ordinary asteroids.* Same as *a* (1). Elements to be improved, then the same as *b* (1).

· *Asteroids for which osculating elements should be derived,* with the use of special perturbations, accuracy in each case to depend on the special research requirements; then same as *b* (2), then *a* (2).

Class d.—Asteroids for which sufficient observations are not available for satisfactory orbit determinations. These should be observed according to coöperative plan to secure necessary and sufficient observations to bring them through the preceding classes into *a* (1) or *a* (2).

Class e.—Lost asteroids. These should be made the object of special orbit investigations to ascertain range of solution, and most probable orbit.

Class f.—New discoveries. A fairly definite observational programme for necessary and sufficient observations during discovery opposition should be agreed upon, the discoverer to be responsible for the execution of the observational programme in coöperation with one or two other observatories, the orbit determinations to proceed hand in hand with the observations, so that a fairly accurate orbit may be available at end of discovery opposition to serve for incorporation of asteroid in one of the preceding classes.

Accidental discoveries to be reported to central agency or agencies for assignment of observational programme and orbit computations.

Particularly interesting cases, such as 1911 MT, or object Wolf 1918, etc., to be generally distributed by telegraph to stimulate observation and investigation.

Class g.—Asteroids observed insufficiently even for preliminary orbit determinations.

SUPPLEMENTARY RECOMMENDATIONS OF COMMITTEE ON COMETS AND ASTEROIDS

Recommended for Reference to International Committee on Comets and Asteroids

1. That mean places of asteroids and comets visually observed with an equatorial be published instead of apparent places. At present the orbit computer makes no essential use of the apparent place reduction of the comparison star. It is practically universal to employ the mean place, corrected or uncorrected for annual aberration according to method, obtained by forming apparent place of object minus apparent place reduction of comparison star. This mean place may also be obtained by applying $\Delta\alpha$ and $\Delta\delta$ directly to mean place of comparison star. In case the orbit computer wishes to take account of the differential apparent place reduction, he would, if apparent place were discontinued, be limited to the use of the differential formulae, which, however, are not longer than a single apparent place reduction. Thus it is seen that the proposed change would give the orbit computer directly what he uses; would in no way increase his labor; would save the observer an apparent place reduction for every observation; and visual observations would be presented in the same way that photographic now are.

2. That each observatory adopt its own distinctive provisional designation for asteroids not identified up to the moment of publication with a previously discovered one. Washington has chosen *W* and Johannesburg

has chosen *T*, followed by a number, but it might be anything distinctive. It is advisable that this always be preceded with the year of discovery, thus, 1917 *W*15, 1912 *T*16, etc. This will enable an asteroid to be referred to by a *distinctive* designation in current discussion and in indexes, thus facilitating such discussion and the looking up of references.

3. That this provisional designation of asteroids should be retained until undoubted identification with an old asteroid or until identified at a second opposition, when it may be given the permanent number designation. This would prevent holes in the consecutive numbering such as (132) Aethra, etc.

4. That the term "astrographic," as first used in Greenwich Observations 1910, be more generally adopted to denote the position of an object obtained from a photographic plate, by using in the reduction the mean places of comparison stars at the beginning of the year. Neglecting the differential apparent place reduction, it is identical with the position obtained by applying $\Delta\alpha$ and $\Delta\delta$ directly to the mean place of the comparison star, as mentioned in (1).

5. That observers be urged to publish observation times in decimal of day, G. M. T. (Greenwich Civil Time after 1925, if the present agitation results in discontinuance of astronomical mean time). For this G. M. T. decimal of day is the form which most computers use, since it is most convenient, because the national ephemerides use G. M. T.

6. That the ephemerides give the rectangular solar coördinates at the beginning of the year, since they are most made use of by orbit computers who almost universally use mean place at beginning of year, and also to give these coördinates in the usual manner so that they may be readily differenced. The Berliner Jahrbuch has done this since 1868. The reduction to some other epoch such as 1900 or 1925 would also be useful. In addition to the coördinates the velocities of the coördinates per one-tenth of a day should also be given to facilitate interpolation and orbit computation by Laplacean methods.

7. That 1900.0 be adopted for the publication of positions of asteroids and for orbit computation. Many times more reference stars for both photographic and visual purposes will be referred to 1900.0 when the astrographic program is completed than to any other equinox. Perhaps more photographic positions of asteroids are now published than visual and probably in a few years they will greatly outnumber the visual. Since it is easier for the photographic observer to derive positions in the equinox of the catalog, it would seem that in the long run, more computation will be saved by this coöperation of observer and computer than by the adoption of any other equinox or equinoxes.

A. O. Leuschner, *Chairman*, E. W. Brown, G. H. Peters.

REPORT OF THE COMMITTEE ON STELLAR PARALLAXES

At the present time determinations of stellar parallax by the trigonometric method form part of the regular work at the following observatories:

	APERTURE	CHARACTER	FOCAL LENGTH
Allegheny	30 inches	Photographic Refractor	46 ft.
Dearborn	18.5	Visual Refractor	28
Greenwich	26	Photographic Refractor	23
McCormick	26	Visual Refractor	32
Mount Wilson	60	Reflector	80
Swarthmore	24	Visual Refractor	36
Yerkes	40	Visual Refractor	62

At these seven institutions material sufficient for the determination of three hundred parallaxes is obtained each year. The average probable error of one determination is under .01″.

Determination of stellar distances by the spectroscopic method are being carried out at Mount Wilson with the 60-inch reflector. When the 100-inch telescope is in regular operation, a larger part of the time of the 60-inch telescope will probably be devoted to obtaining spectrograms for this purpose, so that we may expect a rapid increase in our knowledge from this source. This committee recommends that other institutions be urged to undertake this work, their programs to include a large number of objects already observed by the spectroscopic method. Attention is called to the fact that existing spectrograms (obtained, for example, in radial velocity investigations) could profitably be examined from the point of view of absolute magnitudes and stellar distances, at least for the spectral types Fo to M.

The paucity of reliable parallaxes determined by the trigonometric method has hitherto been a serious obstacle to the effective application of the spectroscopic method, since the latter depends principally upon the trigonometric results for the determination of relationships between line intensities and absolute magnitudes for the various spectral types. The war has delayed the publication of much material that will overcome this obstacle to a considerable extent, and it is likely that in a few years ample material of this kind will be at hand. One of the most important services that observers employing the trigonometric method can render at the present time is to arrange their programmes with the needs of the spectroscopic method in mind.

This committee makes the following recommendations in addition to those already mentioned:

1. That observers employing either method should record the reason for each star being placed upon their observing lists, in order that their results may be properly used in statistical discussions.

2. That they should aim to observe a large number of stars in duplicate in order that data may be forthcoming for the determination of systematic differences and systematic errors.

3. That a catalogue of stellar distances in brief form be published in the near future.

4. Most important of all, that steps be taken at once to determine stellar distances by both trigonometric and spectroscopic methods in the southern hemisphere. In no respect is our knowledge of southern stars as meagre as in this.

Finally, it is recommended that the International Astronomical Union appoint a committee to promote coöperation along the lines indicated.

Frank Schlesinger, *Chairman*, W. S. Adams, S. A. Mitchell.

REPORT OF THE COMMITTEE ON STELLAR PHOTOMETRY

The following observations and suggestions are submitted for the consideration of the American delegates to the meeting of the International Astronomical Union, with the hope that they may be helpful in the organization of an International Committee on Stellar Photometry. It is for this purpose that they are presented, rather than as a program for the operations of such a committee when once formed.

The organization of an International Committee on Stellar Photometry, which we herewith recommend, requires an immediate specification of the functions to be exercised. These we believe may be summarized as follows:

(1) To advise in the matter of notation, nomenclature, definitions, conventions, etc., whose universal adoption will simplify and unify the publication and use of photometric results.

(2) To plan and execute investigations requiring the coöperation of several observers or institutions. The necessity for coöperation in such investigations may arise from geographical considerations, from the magnitude of the undertaking or from the demands for precision which can be satisfied only by combining the results obtained with different instruments and under widely different conditions.

Thus the systematic observation of variable stars requires operations in both northern and southern hemispheres and in different longitudes; again, such an undertaking as the reduction of the provisional magnitudes of the *Carte du Ciel* to the normal scale, because of the labor involved if for no other reason, could not be carried out by any single institution; and, finally, the peculiar difficulties and numerous sources of error involved in the establishment of reliable standards of brightness raises an imperative demand for confirmation which can be satisfied only by concerted and coöperative effort.

The nature of the activities included under (1) is sufficiently obvious from the above formulation. There can be no question as to the desirability of a central committee charged with decisions relating to notation, nomenclature, etc. For example, in the matter of naming newly discovered variable stars, endless confusion can be avoided only through the action of a centralized body of recognized authority and responsibility. Nevertheless some elaboration of statement is necessary in order to emphasize what we believe to be a sound basis of procedure.

A sharp distinction should be drawn between the adoption, on the one hand, of definitions and conventions, and, on the other, of the numerical results which flow from the introduction and usage of such definitions and conventions. The adoption of a definition or convention naturally should never be undertaken hastily or without detailed consideration of all the attendant circumstances; but once an appropriate formulation has been attained, there need be no hesitation or objection to its universal acceptance. Thus we find everywhere in use the definition of the scale of magnitudes represented by the formula

$$m - m_0 = -0.4 (\log I - \log I_0).$$

Similarly, the convention adopted by the committee on magnitudes of the *Carte du Ciel* relative to the zero point of the photographic scale of magnitudes finds ready acceptance on the part of all observers. Or again, the definition of color-index as the difference: photographic magnitude *minus* visual or photovisual magnitude, affords so convenient and useful a measure of the color of a star that it meets with the approval of everyone engaged with the problems of stellar photometry. Thus we find introduced into astronomical practice a uniformity and standardization which is only useful and helpful.

The practical application of these definitions and conventions requires, however, the usage of numerical standards and constants. And here caution must be exercised if progress is really to be aided. A definition or convention in the nature of the case possesses elements of permanence and invariability; but a numerical constant is a thing of the day, to be discarded the moment a more precise determination becomes available. The quality of inconstancy, the ever-increasing precision which characterizes the metrical results of science, should always be borne in mind; otherwise there is danger that the seal of authority may be placed upon results in which authority should have no place. The thing to be emphasized is evidence, not authority, and the evidence should be frequently reviewed.

Thus in the matter of magnitudes: it will be necessary in undertaking any coöperative investigation to specify the standards which are to be used. The selection naturally will be made by the International Committee after a critical examination of the evidence underlying the results then available; but it should be clearly understood that the standards thus chosen are for that particular investigation and are not necessarily to be used for any other which subsequently may be undertaken. In the present state of the question it seems undesirable that any particular set of standard magnitudes be designated by the adjective "international." Such an action on the part of an International Committee would necessarily convey the impression of an authoritativeness and finality of decision which it would be difficult to justify in view of the present outstanding differences in the results of different observers.

This does not mean that there should be no international standards. On the contrary, standards to which this designation may properly be applied should be established. But this can be attained only through a coöperative effort of the type specified under (2). The question will be reverted to later. An additional remark should be made here, however, as a supplement to the above considerations, namely, that the provisional character even of any act of "international" standards should be emphasized, say, through some such designation as I. C. P. 1920 (International Committee on Photometry, Standards of 1920).

We believe, too, that much conservatism should be displayed in the matter of standardization of methods and equipment. As an illustration, it is well known that photometric results obtained by visual methods depend upon the color perception of the eye of the observer. Thus for the Harvard durchmusterung (*Harvard Annals 45*) and the durchmusterung of Müller and Kempf (Potsdam Pub. 9), both catalogues of visual magnitudes, Schwarzschild finds (Göttingen Aktinometrie B 16)

$$H - P = -0.27 - 0.04(P - 6.5) + 0.23\,(G - P)$$

in which $G - P$ is the color-index obtained by comparing Göttingen photographic with Potsdam visual magnitudes. The last term of the equation represents the differences which are to be attributed to peculiarities of color-perception. In visual photometry naturally there can be no standardization in the sense in which the term is used here. Each observer must work with the eye with which he has been provided.

Of late years, however, photovisual magnitudes have been much used as a substitute for those derived visually. An isochromatic plate exposed behind a suitably chosen yellow filter affords results which are at least approximately the same as those obtained visually. But photovisual magnitudes are obviously dependent upon the color-sensibility of the particular kind of plate and filter employed, and in this connection it is frequently suggested that a standard plate and filter be universally adopted and used for all photovisual observations.

Unfortunately the difficulties are not so easily to be avoided. The suggestion overlooks a third contributing factor whose significance is not generally appreciated, namely, the telescope. The selective absorption by the glass of the objective or by the silvered surface of the reflector plays a very important part in determining the color-sensibility of the equipment. Moreover, it is not merely a question of refractors as against reflectors, for different objectives seldom behave exactly the same and not infrequently very large differences are to be met with. This is best illustrated by the following comparisons of different series of photographic results. Here the complicating influence of a filter does not enter, because no filter has been used, and the effect of the kind of plate is largely eliminated, because with some exceptions the same brand of plate has been used throughout. The comparisons show only the term depending on the color-index C.

MW—Greenwich	= +0.27C	Mt. Wilson Cont. No. 97, p. 28
MW—Dziewulski	= −0.06C	
MW—16-in. Metcalf	= +0.33C	Mt. Wilson Cont. No. 98, p. 5
MW—8-in. Draper	= +0.06C	
8-in. Draper—0.5 in. Ross-Zeiss	= −0.13C	
8-in. Draper—4-in. Cooke	= +0.21C	

Other examples might have been added, but those given illustrate the magnitude of the effect and show the impracticability of restricting the photographic measurement of stellar brightness to any closely specified region of the spectrum, for the observer must use the telescope which is available.

Emphasis should, therefore, be directed, not toward standardization, but toward the publication of results in such a form that they may be reduced to *any* system for purposes of comparison and combination with other results. Apparently this is to be accomplished only by giving the color of a star in addition to its brightness. Spectral type cannot be regarded as a satisfactory substitute for color, for it is now known that the color-indices of stars of the same type-color, especially the *G*'s and *K*'s—may differ by half a magnitude or more.

Admitting the impracticability of basing the observations upon any prescribed system of color (in the sense that the measurements are to be made within certain specified limits of wave-length), one might still ask if it is not desirable that the results be reduced to a standard system in advance of publication, in order that all catalogue values may be directly comparable. The answer here is that in the present state of photometry the reduction factors for color will necessarily be uncertain, and hence subject to correction. It thus appears important that the results referred to the color system of the equipment used should always be included in the published results, even when reduced values are also given.

These details are presented as an indication that reservation and caution must characterize the decisions of a central committee in case its activities are to be helpful; at the same time they reveal the disconcerting fact that measures of stellar brightness will attain their full value only when results for the colors of the individual stars become available. This is strikingly shown by an attempt to compare the results of the Durchmusterung of the Selected Areas recently issued as *Harvard Annals* 101 with photographic magnitudes for these same regions determined at Mount Wilson. The color-sensibility of the two instruments—the 16-inch Metcalf and the 60-inch reflector—is so different that the comparison is meaningless except for the few stars whose colors are known. The differences in this instance are extreme, because of the exceptional behavior of the 16-inch Metcalf instrument. A similar situation must be met in comparing the magnitudes of the Polar Sequence determined at Greenwich with those of other investigators. The influence of the instrument is here also unusually large; nevertheless such differences always appear, to some extent at least, and, unless taken into account, vitiate the comparison of results with different instruments. This fact was recognized by Miss Leavitt in her investigation of the Polar Sequence and allowed for wherever possible. The resulting color system in *Harvard Circular* 170 is not very different from that of the Mount Wilson photographic magnitudes; but the true relation of the results of these two investigations can be determined only by allowing for the difference in the two systems. Owing to the systematic difference in the color of bright stars and faint stars, there is a direct influence upon the scales, whose accumulated effect between the 6th and 10th magnitudes amounts to nearly a tenth of a magnitude.

Before leaving these general considerations, and more or less as a summary of them, it may finally be remarked that in our opinion the most effective coöperation is to be attained by granting to the individual investigator the greatest freedom of initiative and operation consistent with the ends to be accomplished. What is desirable as an end can profitably be determined by consultation and mutual agreement; but a healthful development of research requires that the individual be responsible for the means by which its objects are to be attained.

The organization of an International Committee will require a classification of the activities which it is to undertake and the appointment of various subcommittees. These naturally will find their origin in the questions which the committee must consider, hence it seems desirable that a few of the most important of these problems should be passed in review.

1. *Standards of magnitude.*—With proper allowance for differences in color systems and zero points, the photographic scales of Harvard, Greenwich (Chapman and Melotte), Potsdam (Dziewulski), and Mount Wilson are in satisfactory agreement between the 10th and 16th magnitudes. The interval including the brighter stars is fully covered only by the results of Harvard and Mount Wilson. Here there is a scale divergence of 6%. For the reconciliation of this difference further investigations will be required, which, in part at least, should be in the hands of other observers. The question is, therefore, very definitely one for the consideration of an international committee. The observations should preferably be made on the stars of the Polar Sequence. In any event groups such as the Pleiades which contain no red stars that are bright should be avoided, owing

to the fact that the gradual increase in color with increasing magnitude makes it difficult to distinguish the influence of the color equation of the instrument from that of an error in the scale.

The visual and photovisual standards now available also require further attention. These will most conveniently be considered in connection with the photographic standards.

When once it is clear that all serious systematic errors have been eliminated, the committee may with propriety consider the combination of the results of the different investigations for the formation of series of international standards, which doubtless would then require revision only at infrequent intervals.

It would be a convenience to observers if the adopted values were referred to alternative systems of color: in the case of photographic magnitudes, for example, to the color system of an average objective, say, that of the 8-inch Draper telescope which is essentially that of the Harvard Polar Sequence, and also to the color system of the reflector, which very likely is constant or nearly so for all instruments of this type. The visual standards should be referred to the color system of the Harvard Photometry; but photovisual standards based on the reflector should also be available. The standardization of a plate and filter for use with the reflector may not prove feasible; but having chosen an isochromatic plate which is easily obtainable it doubtless would not prove too difficult to construct a filter such that the combination reproduces the adopted standards. Because of its bearing upon various physical questions such as stellar temperatures, the spectral intensity-curves corresponding to the color-system used should in all cases be published.

2. *Reduction of the magnitudes of the Carte du Ciel to the normal scale.*—This undertaking has been in the hands of the Committee on Magnitudes of the *Carte du Ciel.* In case the work of this committee is merged with that of the organization under discussion, the investigation will immediately become one of the most important and pressing questions before the newly organized committee.

Two methods of procedure are possible: (*a*) Await the results of (1) above, and then use the resulting standards; (*b*) establish the scale for the Astrographic Zones with the aid of the valuable counts recently published by Turner. This would require a knowledge of the star-density as a function of galactic latitude and longitude for each interval of magnitude. Each of these methods has its advantages and disadvantages which will require the attention of the committee.

3. *Nomenclature of newly discovered variable stars.*—Hitherto in the hands of the Astronomische Gesellschaft. An important question of policy must be faced in this connection. Probably the Argelander notation should be continued, because of its convenience. For catalogue purposes, however, the Harvard notation has important advantages, be-

cause of its elasticity. The number once assigned is permanent and always places the star in its proper serial order in the catalogue.

4. *Ephemerides of variable stars.*—Hitherto published annually in the *Vierteljahrsschrift.* Again a question of policy must be considered. In case this very desirable aid to the observer is to be prepared under the direction of the International Committee, the form of publication should be most carefully considered.

5. *Catalogue of variable stars.*—Intimately related to (3) and (4). A complete catalogue, of carefully considered form, should be published; at intervals which may be determined by experience it should be completely revised. In the interim annual supplements containing revisions and additions should be available.

6. *Systematic observation of variable stars.*—To insure a proper distribution of effort and to avoid a needless duplication of results, there should be mutual agreement between observers as to the objects to be observed or at least an announcement as to objects under observation. A central committee, as a clearing house for the dissemination of information and the arrangement of details, would prove invaluable. Directive action on the part of the committee should be reduced to a minimum. The affiliation of associations of amateurs should be invited.

Other questions might easily be added, but these we believe sufficient, to indicate the character of the organization required.

Very respectfully submitted,

F. H. SEARES, *Chairman*, S. I. BAILEY, F. C. JORDAN, J. A. PARKHURST, JOEL STEBBINS.

REPORT OF THE COMMITTEE ON DOUBLE STARS

I. CLASSIFICATION

The term "double star" is still loosely used to describe any pair of stars which have been connected by micrometric measures. It seems desirable to agree, if possible, upon some more exact definition and to adopt some system of classification and of printing conventions which will enable us to distinguish between pairs of different character. To be of service, such a system must be adopted by common consent of astronomers and such consent can best be obtained by discussion and agreement at a meeting of the character of the proposed International Astronomical Union. As a basis for discussion, the Committee on Double Stars suggests:

1. That in measuring and cataloging *new pairs* definite limits as to magnitude and angular separation be observed, such as those, for example, proposed in *The Working Definition of a Double Star*, published in *A. N.* **188,** 1911, (281).

2. That double stars already cataloged be divided into four classes, as follows:[1]

---

[1] The section requested the committee to devise a different mode of printing.

Class A.—Systems for which the measures show a definite curvature of the relative path, or a well-determined variation in velocity along the relative path. Class A systems are to be indicated in catalogs by the use of small capitals; e.g., 61 CYGNI, Σ 483, δ EQUULEI.

Class B.—Systems which do not come in Class A, but whose physical connection is probable, as shown by the fact that the components have the same proper motion. Since limits must be adopted to exclude stars belonging simply to a great cluster (e.g., Ursa Major) it is suggested that pairs be entered in Class B only if the angular separation is less than 100 times the annual proper motion. Pairs under 1″.0 separation which have been under observation, say, for a decade or more, and have shown no relative motion may be included in this class on the assumption that they are moving together at a rate less than ″.01 per annum. Systems of Class B are to be indicated in catalogs by the use of bold faced type; e.g., **Σ 110, OΣ 175, OΣ 424.**

Class C.—Systems whose components have from the measures apparently different proper motions. This class will include the purely optical pairs and also pairs which may later require transfer to Class A. In catalogs they are to be indicated by the use of italics; e.g., *δ Herculis, Σ 2760, Σ 742.*

Class D.—Systems about whose motions nothing is known. This class might be made to include the wide, faint pairs apparently simply of historic interest as well as new pairs of unknown proper or relative motion. It is evident that pairs entered in this class will ultimately be transferred to one of the three preceding classes or dropped entirely from our catalogs. They will be indicated in catalogs by the use of lower case Roman type; e.g., h 528, A 3000.

II. COÖPERATION

It is highly desirable that double star observers work in closer coöperation than heretofore to insure (1) the sufficient observation of all double stars in need of measurement at any given epoch; and (2) the avoidance of unnecessary duplication of measures. Of course no action should be taken that would in any way discourage personal initiative in the choice of problems or methods; but it is suggested that certain astronomers who have been specially interested in collecting all measures of double stars (e.g., Messrs. Eric Doolittle, Robert Jonckheere, R. T. A. Innes) might be asked to act as a "clearing committee," which would supply any observer wishing it with a list of double stars in need of measurement and, in principle, suited to the power of the telescope to be employed.

III. GENERAL CATALOGS

Experience in using and handling Burnham's General Catalog has proved the inconvenience of having the data for any one system recorded partly in one volume and partly in another. It is, therefore, recommended that in future catalogs all data for a particular system be printed in one place. For catalogs printed in the near future the epoch 1950.0 is recommended for the R. A. and Decl.

The material for each pair should include, in addition to measures indicating the character of the motion, the magnitude of the star (combined brightness of the components) on some consistent scale (which might well be that adopted in the photometric magnitudes of the Henry Draper Catalogue), the spectrum (when known), and the proper motion (in *polar coördinates,* not in R. A. and Decl.). For systems having known orbits all the elements, and not simply the period, should be recorded. It is not at all essential that all measures should be recorded, but references should be given to all published measures, orbits and notes. This, essentially, is Professor Doolittle's plan for the extension of Burnham's General Catalog.

The ideal catalog would cover the entire sky. If this ideal cannot be attained, it is recommended that those who plan the publication of partial catalogs come to an agreement as to limiting declination so that unnecessary overlapping may be avoided.

IV. WORK IN THE SOUTHERN HEMISPHERE

The most important piece of observational double star work that can be undertaken at the present time is the extension to the South Pole of a survey of stars to about the 9th magnitude along lines similar to those of the survey of the northern sky completed at the Lick Observatory, and the systematic remeasurement of all the closer pairs in the southern sky which have already been cataloged. A careful examination of all published data indicates that at least 1800 double stars with angular separation under 5″ remain to be discovered among the stars as bright as 9.0 magnitude in the southern half of the sky—two thirds of them in the area south of $-30°$ declination. This assumes the southern sky to be as rich in such systems as the northern. Further, there is only one system (α Centauri) south of $-40°$ for which we have satisfactory orbit elements; there are at least 18 such north of $+40°$.

At present the only workers in this field are Mr. Innes, at Johannesburg, using a 9-inch refracting telescope, and Mr. Dawson, at La Plata, using a 17-inch refractor. A larger telescope in a favorable position is urgently needed. Using a telescope of 20 to 25 inches aperture in a good location, a competent observer could extend to the South Pole such a survey as has been mentioned in about six years and could at the same time secure measures of all close known pairs south of $-30°$ declination. Any action that can be taken by the International Astronomical Union to encourage and assist such an undertaking either through one of the observatories already existing in the southern hemisphere or through a special expedition from the north is heartily to be recommended.

V. COMMITTEE ON DOUBLE STARS

Finally, your Committee recommends the appointment of an international committee on double stars to act under the authority of the International Astronomical Union.

ROBERT G. AITKEN, *Chairman,* ERIC DOOLITTLE, W. J. HUSSEY.

NOTE.—As the committee's work was carried on by correspondence there was not time for a complete discussion in advance of the meeting of the Section. The following comments were added by Mr. Hussey:

"1. Personally I do not attach very much importance to the formal definition of a double star. I am inclined to accept our heritage from the past and work according to our best judgment for the future. The attempts made to define a double star in terms of magnitude and distance have generally broken down by not being sufficiently inclusive, owing to the movements of the stars themselves.

"2. It does not seem to me wise to complicate the printing of results by the use of different kinds of type for the different classes of double stars, such as capitals, small capitals, italics, lower case, and bold face, etc. Such attempts lead to unsightly printed pages, and to the necessity of keeping constantly in mind the nomenclature used. In my opinion it is better to follow the prevailing practice of using brief descriptive terms, which carry their own meaning, such as binary, optical, pair, fixed, etc.

' 3. The visual double stars are evidently binaries or optical pairs. To which class a given star belongs can only be ascertained from a sufficiently extended series of micrometrical measures, and such a series may require many years. Until such measures are obtained the status of a pair remains undetermined. It is not justifiable, as is proposed in the section "Class B" of the Report, to regard all pairs whose distances are under 1″.00 as having common proper motions when no change is shown by measurements extending over so brief a period as ten years. Longer series of measurements may show some such stars to be optical pairs, others binaries, and still others common proper motion pairs.

"4. Pairs whose components have different proper motions are optical systems. No star of this class can be a binary, and, therefore, appropriate to be put in Class A, as suggested in the paragraph headed Class C of the report.

"5. Northern Work. At the present time the most pressing need in the north is the remeasurement of the pairs discovered during the past twenty years, and especially the close pairs discovered at the Lick Observatory.

"6. Southern Work. In the south there is needed the measurement of the known pairs, not recently measured, and the examination under good conditions of the southern stars to the ninth magnitude for the discovery of new pairs.

"In this connection it may be stated that the Observatory of the University of Michigan began some years ago the construction of a 24-inch refracting telescope for double-star work in the Southern Hemisphere. The mounting for this instrument was nearly completed five and a half years ago, when work upon it had gone as far as was practicable until the objective could be obtained. As soon as the glass for the objective can be obtained this instrument will be completed, and then taken to the Southern Hemisphere for double-star work and for other observations. No site has been definitely selected for this instrument. It will be taken to that place which promises the best results.

W. J. HUSSEY."

REPORT OF THE COMMITTEE ON STELLAR CLASSIFICATION

Your Committee recommends the following proposal for adoption by the American Section, and presentation by its delegation at the international meeting at Brussels.

CLASSIFICATION OF SPECTRA

(*a*) That the methods of determining the spectral class, according to the Draper Classification, which have been developed by Adams and Kohlschütter, be adopted as an extension of the international standard of classification.

(*b*) That the notation of the Henry Draper Catalogue (*Harvard Annals*, vol. 91) be adopted in the case of stars of Classes R and N.

(*c*) That the notation of the Henry Draper Catalogue be adopted for the spectra of gaseous nebulae, and that P50 be employed to denote spectra intermediate between gaseous nebulae and fifth-type stars.

(*d*) That a series of standard stars be chosen, representing typical spectra, separated by not more than five divisions of the Draper scale (such as B0, B5, A0, etc.) and that reproductions of negatives of these spectra be published for the benefit of astronomers generally.

(*e*) That the presence of the more noteworthy spectral peculiarities (such as bright lines, unusually sharp or diffuse lines, and the peculiarities in intensity associated with bright or faint absolute magnitude, and with the Cepheid type of variation) be denoted by distinctives ymbols appended to the ordinary designation of the spectral class.

(*f*) That a Committee on Stellar Classification be appointed by the International Astronomical Union, which shall undertake the preparation of the list and reproductions of standard spectra (recommendation (*d*)) and the formulation of the notation for spectral peculiarities (recommendation (*e*)) after consultation with the most experienced investigators in this field.

In explanation of (*e*) it may be noted that the use of new distinctive symbols for spectral peculiarities is proposed because the small Roman letters at present employed to denote certain characteristics are not used in the same sense in different spectral classes.

The remaining recommendations explain themselves, and all the recommendations have received the unanimous approval of the members of the Committee, either during preliminary discussion, or in their final form.

H N. RUSSELL, *Chairman*, W. S. ADAMS, A. J. CANNON, R. H. CURTISS.

REPORT OF THE COMMITTEE ON RADIAL VELOCITIES

We beg to recommend to the American Section of the International Astronomical Union:

1. The formation of a Committee on Radial Velocities, in the International Astronomical Union, whose duty it shall be to encourage and promote research in this field.

2. There should be coöperation among radial velocity observers to the ends that duplication may be limited to necessary and desirable degrees, that the resources of the various observatories may be applied chiefly to those problems for which they are the more uniquely adapted, and that the accumulation of accurate and homogeneous results may be expedited. It is not proposed that the individuality of investigators, as to selection of problems and methods, be interfered with, even by suggestion, but only that the evident advantages of coöperation be available to all qualified observers who seek them.

3. A Committee of the American Astronomical Society, embracing the leading radial velocity observers, or their representatives, in 1910–11 gave careful consideration to the proposal that radial velocity determinations should be extended to several thousand stars in the Harvard Revised Photometry fainter than those previously observed for this purpose, on the basis of coöperative division of effort. All of the members but one were of the opinion that, however strongly they might desire to engage in the suggested coöperative plan, their telescopic resources were too weak to give promise of coping successfully with many additional stars. In the meantime a 60-inch and a 73-inch reflecting telescope have engaged heavily in this work, and other great reflectors are under construction or nearing completion. We are of the opinion that coöperation is now practicable and desirable. We recommend that those institutions engaged extensively in measuring the radial velocities of stars should endeavor so to coördinate their programs that the work of determining the radial velocities of the stars in Boss's Preliminary General Catalogue shall be divided amongst them on the basis of their latitudes, the light gathering powers of their telescopes, etc.

4. The present situation in the Southern Hemisphere closely resembles that in the Northern Hemisphere in the year 1910. The intensive use of very large reflecting telescopes in the Northern Hemisphere will soon leave radial velocity determinations in the southern sky far behind, and

thus create a lack of symmetry and balance in the results for the two hemispheres which will impose regrettable limitations upon their statistical power. This will be unfortunate. There is immediate and serious need for increased observing resources in the Southern Hemisphere, based upon the use of reflecting telescopes 60 inches and greater in diameter. Organization for this service should be encouraged. It is mutually understood that the Victoria, Lick, and Mt. Wilson observatories are prepared to form their radial velocity programs in this manner.

5. Observers equipped with medium-sized telescopes should not hold back from radial velocity work because telescopes of much greater power are engaged extensively in this field. They should not be misled into assuming that the relative exposure times vary inversely as the *areas* of the objectives of the telescopes; under average atmospheric conditions the exposures are more nearly as the inverse *diameters* of the objectives. The instruments of medium power may find much useful work to do in observing the brighter stars of Classes O, B and A, whose spectra contain few and broadened lines, with reduced dispersion; and many such stars are waiting observation.

6. The investigation of spectroscopic binaries is a field in which more workers may find fruitful employment. This is especially true of the southern sky. There is need for the application of reflecting telescopes to further study of spectroscopic binary stars in whose spectra the K line of calcium varies but slightly in wave-length. There are, of course, many other minor problems to which radial velocity methods could be applied with promise or certainty of valuable results.

7. Consideration should be given to questions of wave-lengths and methods, to insure that results obtained at all observatories be homogeneous, comparable and reducible to one system. This principle should apply not only to bright stars of a given spectral class, but to bright and faint stars of the same class as observed with spectrographs of widely different dispersions, and as far as possible to stars of all the different spectral classes.

8. There exist at various observatories a great number of spectrograms obtained primarily for radial velocity determinations. We recommend that these spectrograms be utilized for estimates of the absolute magnitudes of the corresponding stars, and therefrom the spectroscopic parallaxes of these stars.

W. W. CAMPBELL, *Chairman*, WALTER S. ADAMS, J. S. PLASKETT.

REPORT OF THE COMMITTEE ON REFORM OF THE CALENDAR

I

At this very unsettled time in the world's history all sorts of reformers, wise and otherwise, are springing up advocating changes in all sorts of things. It is advisable that scientific organizations of standing be conservative in their advocacy of far-reaching reforms touching the every-day life of the people, *at the present moment*, even though such reforms seem in themselves desirable.

II

The calendar can undoubtedly be revised or reformed to advantage. The following suggestions for such revision or reform, favored by your Committee, are presented for your consideration *without recommendation* as to whether action to accomplish them should be advocated for the present year:

1. The year of twelve months to be divided into four quarters of three months each, and the arrangements for the first quarter to repeat themselves precisely in each of the other quarters. The first month of each quarter to have thirty days; the second month thirty days; and the third month thirty-one days. This accounts for ninety-one days in each quarter, or 364 days in all.

The remaining day in ordinary years to be "New Year's Day." It is to be given no other descriptive title. It is not to belong to any week nor to any month. It begins the year. January 1 to be the day following New Year's Day.

The 366th day of a leap year to be likewise an extra day, bearing an appropriate name (perhaps "Leap Day"), but no week-day name, nor should it be a part of any month. It may be (*a*) the day following December 31, hence the day preceding New Year's Day, or (*b*) the day following June 31, or (*c*) the day following Christmas Day (at the present time the day following Christmas Day is a holiday in some countries). It is assumed to be a holiday—it comes only one year in four.

January 1 to fall on Monday. From this it results that the first month in each quarter (January, April, July, October) begins on Monday in every year; that the second month in each quarter (February, May, August, November) begins always on Wednesday; and that the third month in each quarter (March, June, September, December) begins always on Friday. Further results of this are that (*a*) the first day of the month never falls on Sunday, (*b*) the fifteenth day of the month never falls on Sunday, (*c*) the thirtieth day of the month never falls on Sunday, (*d*) the last day of each quarter (the 31st of March, June, September, December) always falls on Sunday, (*e*) the thirty-day months always have four Sundays each, (*f*) the thirty-one-day months always have five Sundays each, (*g*) the number of week-days in the month is twenty-six for every month, (*h*) holidays *always* fall on the same day of the week. This proposed calendar is outlined in the following table, which will serve for *all years alike.* Leap day is assumed to follow December 31.

New Year's Day															
First Quarter	January					February					March				
Second Quarter	April					May					June				
Third Quarter	July					August					September				
Fourth Quarter	October					November					December				
Leap Day															
Monday	1	8	15	22	29		6	13	20	27		4	11	18	25
Tuesday	2	9	16	23	30		7	14	21	28		5	12	19	26
Wednesday	3	10	17	24		1	8	15	22	29		6	13	20	27
Thursday	4	11	18	25		2	9	16	23	30		7	14	21	28
Friday	5	12	19	26		3	10	17	24		1	8	15	22	29
Saturday	6	13	20	27		4	11	18	25		2	9	16	23	30
Sunday	7	14	21	28		5	12	19	26		3	10	17	24	31

This would be a perpetual calendar, and need be printed but once.

If this calendar should be adopted it should go into effect at the beginning of a year which, according to the present calendar, would begin with Monday in order to avoid such confusion as might arise from the adding or dropping certain days of the beginning week. According to the present calendar the next year that begins on Monday is 1923, and the next following year to begin on Monday is 1934.

It should be pointed out that according to the proposed calendar the Sun will come to the Vernal Equinox about March 19 or 20 instead of March 21 or 22 as under the present calendar.

2. The day to be divided into twenty-four hours numbered consecutively from one to twenty-four, the noon hour to be twelve.

3. The astronomical day to begin at midnight.

R. T. Crawford, *Chairman.*

Signed after expressing very strong preference for "Leap Day" following December 31, in order to break the continuity of the weeks only *once* per annum. W. W. Campbell.

Signed, *excepting* II 2, relating to the 24-hour day, because it would necessitate an impossible change of dials and mechanisms in all existing timepieces. Harold Jacoby.

REPORT OF THE COMMITTEE ON RESEARCH SURVEYS

One of the objects of the National Research Council, as expressed in the Executive Order issued by President Wilson on May 11, 1918, is "to survey the larger possibilities of science, to formulate comprehensive projects of research, and to develop effective means of utilizing the scientific and technical resources of the country for dealing with these projects."

The Research Council has already undertaken the preparation of research surveys, pointing out important possibilities in both scientific and industrial research. The chief objects of these surveys are:

1. To aid the specialist in recognizing the wider bearing of his in-

vestigations and the importance of organizing them in such a way as to render his efforts most productive.

2. To indicate the possibility of applying in other branches of science the special instruments, methods, or results developed in particular fields.

3. To formulate large and important research projects, calling for special financial support or for the coöperation of investigators in the same field or of those dealing with other branches of science.

4. To encourage more active investigation in the borderlands between the various branches of science.

The first of the research surveys relating to astronomy was written in 1917 by Dr. Henry Norris Russell, but withheld from publication because of the war. This has recently been revised, and will shortly appear among the Bulletins of the National Research Council under the title "Some Problems of Sidereal Astronomy."[1] The recent report of Dr. C. G. Abbot, entitled "The Larger Opportunities for Research on the Relations of Solar and Terrestrial Radiation,"[2] is also well adapted for publication as a Research Council Bulletin, and your committee recommends that it be brought out in this series in the near future.

We also recommend that Professor E. W. Brown be requested to prepare for publication a survey of the gravitational problems of the solar system. Later Bulletins should deal with cosmogony, the atom and electron in sidereal astronomy, the future possibilities of research in stellar spectroscopy, stellar distances, and other subjects. A synopsis of the adaptation of physical apparatus to astronomical problems would also be a valuable contribution, especially if it indicated some of the astronomical possibilities of recently developed physical methods.

Because of the importance of obtaining the personal views of leading investigators on the opportunities for research in their respective fields, it is not proposed that any single branch of astronomy be exclusively dealt with by a single writer. The suggestive value of these reports is likely to be greater if they are prepared by individuals rather than by committees, as in the latter case the average view, involving the consensus of opinion, is substituted for the special view of men who have approached the subject from different directions.

Your committee requests that it be continued and authorized to arrange for the preparation and publication of research surveys in astronomy, in conformity with any general plan that may be adopted by the National and International Research Councils.

GEORGE E. HALE, *Chairman*, F. R. MOULTON, HARLOW SHAPLEY.

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[1] Reprint and Circular Series of the National Research Council, Number 5.

[2] Reprint and Circular Series of the National Research Council, Number 7.

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CONTENTS

Page

Statistics.—On the Rate of Growth of the Population of the United States since 1790 and Its Mathematical Representation . . . . *By Raymond Pearl and Lowell J. Reed* 275

Physics.—On the Springfield Rifle and the Leduc Formula . . . . *By Arthur Gordon Webster* 289

Chemistry.—Alkyl Amides of Isothiocyanacetic Acid . . . . *By Treat B. Johnson, Arthur J. Hill and E. B. Kelsey* 290

Astronomy.—Studies of Magnitudes, in Star Clusters. XI. Frequency of Curves the Absolute Magnitude and Color Index for 1152 Giant Stars . . . . *By Harlow Shapley* . . 293

Mathematics.—On the Distortion in Conformal Mapping When the Second Coefficient in the Mapping Function Has an Assigned Value . . *By T. H. Gronwall* 300

Physics.—On the Connection of the Specific Heats with the Equation of State of a Gas . . . . *By Arthur Gordon Webster* 302

Chemistry.—Anomalous Osmose . . . . *By F. E. Bartell* 306

Physics.—A Photographic Method of Finding the Instantaneous Velocity of Sound Waves at Points Near the Source . . *By Arthur L. Foley* 310

Mathematics.—Conformal Mapping of a Family of Real Conics on Another . . . . *By T. H. Gronwall* 312

Physics.—Experiments on the Vibration of Air in Conical Horns . . . *By A. G. Webster* 316

Genetics.—The Relative Importance of Heredity and Environment in Determining the Piebald Pattern of Guinea-Pigs . . . . *By Sewall Wright* 320

Geology.—Fossil Plants from the Late Cretaceous of Tennessee . . . *By Edward W. Berry* 333

National Research Council.—Notes from the Minutes of the Meetings in January and February, 1920, of the Executive Board and Interim Committee of the National Research Council 335

——— Report of the Meetings of the International Research Council and of the Affiliated Unions Held at Brussels, July 18–28, 1919 . . 340

——— Report on the Organization of the International Astronomical Union 349

International Research Council 340

International Astronomical Union 349

VOLUME 6 JULY, 1920 NUMBER 7

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PROCEEDINGS
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THE DYNAMICS OF A FLUCTUATING GROWTH RATE[1]

By H. S. Reed

Graduate School of Tropical Agriculture and Citrus Experiment Station, University of California

Communicated by R. Pearl, April 17, 1920

Quantitative studies of growth phenomena should lead to a consideration of the energy relationships involved. We cannot but believe, in the absence of contrary evidence, that the life processes of living organisms are manifestations of the same principles of energy relationships which exist in other parts of the universe. The study of these energy relationships ought not, therefore, to be disregarded. Our aspirations for such knowledge, which have received so much encouragement from the discovery of the principle of the conservation of energy, can never be entirely suppressed.

This quantitative study of the rate of growth of shoots on a perennial plant, has been attempted in order to gain an insight into some kinetic aspects of the problem of growth. Growth, in one form or another, is the end product of most of the energy transformations of a tree. Not all forms of growth are readily measured. Increase in length is the most readily measured and its determination is fairly free from error. Increase in weight might be better, but where one wishes to study identical shoots through the season, weighing is impossible because it requires removal of the shoots from the tree.

The present study is based upon measurements of 70 selected shoots of Royal apricot trees which grew on the grounds of the Citrus Experiment Station at Riverside, California. The observations were made during the growing season of 1918. At the time the observations began, the trees had been growing two years in the orchard. The trees on which shoots were selected for measurement were scattered over the entire orchard in such a way that a fairly random distribution was obtained.

Shortly after the new shoots appeared, two were selected on each tree which had been pruned according to the "Winter-vase" type (a tree which is pruned every winter to produce a vase type). The type of pruning was thus uniform, and since the trees were periodically irrigated throughout the season, it is believed that conditions necessary for fairly uniform growth were afforded. The trees, being so young, produced no fruit and consequently growth was not influenced by that activity. The selected shoots were marked with a line of India ink ten centimeters from the growing point and all measurements were made from this as a base line.

Most of the shoots produced several lateral branches during the season. The length of these laterals was disregarded as far as these studies are concerned. The propriety of this procedure may be questioned, yet it is difficult to see any valid objection thereto, because in measuring the increase in length of a growing shoot we are measuring the catalytic activity of a group of meristematic cells in the apical bud of that shoot. Unless the lateral branches of a shoot have some modifying effect either upon the growth catalyst, or upon the supply of material upon which the catalyst acts, it is difficult to see how the presence of the lateral shoots can modify the growth rate of the main branch. Since we know that removal of the lateral branches of a shoot retards rather than accelerates the length-growth of the main shoot, we are not warranted in assuming that their presence has a retarding influence upon the growth of the main shoot.

The Course of Growth of the Apricot Shoots.[2]—If the growth of a branch is similar in its rate to some form of autocatalytic reaction, we should expect the length of the branches to be determined by some sort of a unimolecular reaction formula which shall involve the time and a growth constant.

The most generally useful equation to express this sort of a rate is that of autocatalysis,

$$dx/dt = kx(a-x) \qquad (1)$$

in which a represents the final size of the plant, x is the height (or size) at any time, t, and k is the velocity constant of the reaction. A paper recently published (Reed and Holland, 1919) has shown that the growth rate of Helianthus is well expressed by this equation, but it does not necessarily follow that the growth rate of individual shoots on a perennial tree will follow the same type of growth as the axis of an annual plant like the sunflower.

Inspection of the observed values shows that there were three quite distinct cycles of growth in the growing season. The existence of these cycles may be seen by inspection of the graph in figure 1, but better by the observed increments in table 4. The large weekly increments in the first few weeks of the season diminish to a minimum value at the 9th week; then for a few weeks are larger, diminishing again to the 18th week; they increase to the 21st week and then decline to the end of the season. The season's growth, therefore, consisted of three cycles of growth each of about 9 weeks duration. These cycles were quite apparent during the growing season. At the beginning of each cycle, growth was rapid, the leaves arising upon that part of the shoot were of maximum size and outspread. As the end of a growth cycle was approached, the slower-growing portion of the shoot produced smaller and smaller leaves which had more of a tendency to curl. Simultaneously, the apical bud of the shoot was enlarged and became almost completely dormant. Following the partial dormancy of the apical bud, lateral shoots were often formed from subapical buds. However, within a week or two, the next cycle of growth would start and the shoot would repeat the processes described for the preceding cycle.

If the shoots pass through distinct intra-seasonal cycles of growth, we should expect that the growth in each cycle should show evidence of being homogeneous and that it could be expressed definitely. We may, therefore, examine each of the three cycles of growth of the apricot shoots with this end in view. From inspection of the data it seems proper to designate the first cycle as the time from 0 to 8 weeks, the second from 9 to 17 weeks, and the third from 18 to 28 weeks. If each of these follows the course of a more or less independent autocatalytic reaction, we may compute their growth by the formula

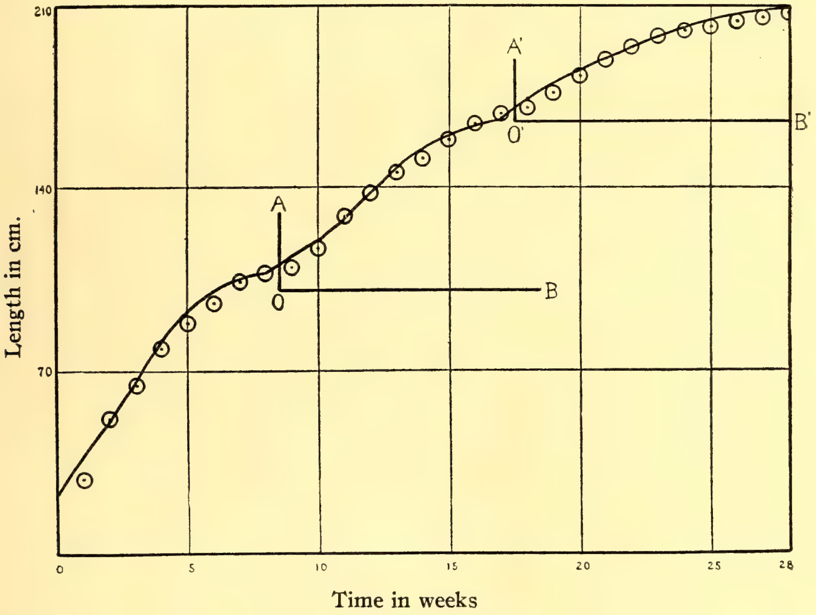

FIG. 1

Growth of apricot shoots represented as three cycles.————————, curve calculated from

$$\log \frac{x}{a - x} = k\,(t - t),$$

⊙ ⊙ ⊙ ⊙ , observed values.

$$\log \frac{x}{a - x} = K\,(t - t_1) \qquad (2)$$

which is the integral form of the equation (1), in which a is the length of the shoot at the end of the cycle, x is the length at any given time, t, $K = ak$, and t_1 is the time at which the cycle is half completed, i.e., when $x = a/2$.

The computations may be followed by the aid of table 1. Since the new shoots were marked at the commencement of observations at a uniform distance of 10 cm. from the tip, we may take the value of x as 10 when $t = 0$. The successive length of shoots in each cycle was computed from the equation (2). For convenience the time was computed in days instead of weeks. Data for the three cycles are given in table 1.

For the computation of the second and third cycles the axes were moved to AOB and A′O′B′ (fig. 1). For the second cycle the observed values of x are diminished by 100 cm. and for the third cycle by 165 cm. Having obtained the mean value of K for each cycle, the calculated values of x were obtained from the equation stated above.

The observed and calculated values of this kind of a reaction are apt to show more divergence in the initial stages than after the reaction gets under way. The growth rate is apparently no exception to this rule. However, the divergence is not excessive when one considers the nature of the material and the observational errors involved in making measurements. The graph in figure 1 shows the nature of the curves and their agreement with observed values. It is thus apparent, not only that the growth process followed the course of a definite reaction, but that the individual cycles which made up the growth period also follow a definite course which conforms to certain quantitative relationships.

It will be noted that there is very good agreement between the observed and computed values of these three cycles and that each in itself appears to follow the course of an autocatalytic reaction. The root-mean-square deviations of the values of the three cycles are 5.15 cm., 2.15 cm., and 2.52 cm.

It will be further noted that the mean value of K in the three cycles is successively less in each case, being, 0.0380, 0.0355, and 0.0277 respectively. Reference to the graph in figure 1 shows that the mean slope of the three curves is also successively less.

This analysis of the growth rate of the apricot shoots is taken to indicate that their seasonal growth was made up of three distinct cycles of approximately 9 weeks each. In the first cycle growth was more rapid than in any other. Growth goes on at a more rapid rate during approximately the first third of each cycle and then "fades away" toward the end of that cycle.

While this analysis affords some interesting insight into the growth

TABLE 1

GROWTH RATE OF APRICOT SHOOTS, CALCULATED UPON THE BASIS OF THREE CYCLES IN THE GROWING SEASON

1ST CYCLE					2ND CYCLE					3RD CYCLE				
t DAYS	x (OBSERVED) (CM.)	K	x (CALC.) (CM.)	θ (CM.)	t DAYS	x (OBSERVED) (CM.)	K	x (CALC.) (CM.)	θ	t DAYS	x (OBSERVED) (CM.)	K	x (CALC.)	θ (CM.)
0	10.0	0.0625	21.9	11.9	64	9.6	0.0419	12.2	2.6	126	5.3	0.0464	10.5	5.2
7	28.5*	0.0510	34.4	5.9	70	16.7	0.0418	19.0	2.3	133	11.8	0.0383	14.5	2.7
14	51.5	0.0295	50.2	-1.3	77	28.5	0.0320	27.8	-0.7	140	18.4*	0.0347	19.2	0.8
21	64.5	0.0294	67.1	2.6	84	37.4	0.0320	37.7	0.3	147	24.4	0.0285	24.4	0.0
28	78.6	0.0328	81.8	3.2	91	45.3*	0.0300	47.2	1.9	154	29.2	0.0273	29.3	0.1
35	88.0	0.0313	92.8	4.8	98	50.8	0.0269	54.9	4.1	161	33.0	0.0258	33.7	0.7
42	95.6	0.0310	100.1	4.5	105	58.4	0.0310	60.5	2.1	168	35.1	0.0224	37.2	2.1
49	103.9	0.0365	104.5	0.6	112	64.8	0.0374	64.2	-0.6	175	36.9	0.0206	39.9	3.0
56	107.2	0.0381	107.2	0.0	119	68.4	0.0463	66.5	-1.9	182	38.8	0.0201	41.9	3.1
										189	40.6	0.0203	43.2	2.6
										196	42.0	0.0205	44.1	2.1

1st cycle:
Assumed value of a = 110.4
Assumed value of t_1 = 16.0
Mean value of K = 0.0380

Root-mean-square deviation of observed from calculated values of x = 5.15 cm.

2nd cycle:
Assumed value of a = 69.7
Assumed value of t_1 = 82.0
Mean value of K = 0.0355

Root-mean-square deviation of observed from calculated values of x = 2.15 cm.

3rd cycle:
Assumed value of a = 45.8
Assumed value of t_1 = 145.0
Mean value of K = 0.0277

Root-mean-square deviation of observed from calculated values of x = 2.52 cm.

* Interpolated.

rate of the apricot shoots it is not entirely satisfactory to dissect thus a process which is undoubtedly homogeneous in its nature. Our efforts must continually bend toward the problem of explaining the organism and its activities as a whole. We shall, therefore, attempt to study the dynamics of the entire season's growth and to learn something about the nature of the process.

Wilhelmy's equation, which has been found to apply to many processes in physical chemistry, was used;

$$\frac{dx}{dt} = k\,(a-x) \qquad (3)$$

TABLE 2

CALCULATED AND OBSERVED LENGTH OF APRICOT BRANCHES

TIME WEEKS	OBSERVED CM.	CALCULATED FROM $x=210\ (1-e^{-0.095t})$ CM.	θ
1	15.8	19.1	− 3.3
2	41.5	36.3	− 5.2
3	54.5	52.1	+ 2.4
4	68.6	66.4	+ 2.2
5	78.0	79.4	− 1.4
6	85.6	91.4	− 5.8
7	93.9	102.0	− 8.1
8	97.2	111.8	−14.6
9	99.6	120.8	−21.2
10	106.7	128.8	−22.1
11	118.5	136.0	−17.5
12	127.4	142.8	−15.4
13	135.3	148.8	−13.5
14	140.8	154.7	−13.9
15	148.4	159.5	−11.1
16	154.8	164.0	− 9.2
17	158.4	168.1	− 9.7
18	161.3	172.0	−10.7
19	166.8	175.6	− 8.8
20	173.4	178.8	− 5.4
21	179.4	181.2	− 1.8
22	184.2	184.0	0.2
23	188.0	186.5	1.5
24	190.1	188.6	1.5
25	191.9	190.4	1.5
26	193.8	192.2	1.6
27	195.6	193.8	1.8
28	197.0	195.2	1.8

where x represents the length of the shoot at time t, a represents the final length, and k is a constant of the reaction.

By integrating the above equation, we get

$$x = a\,(1-e^{-kt}) \tag{4}$$

in which e is the base of the natural logarithms.

It is of interest to know whether the measurements of the apricot shoots will approximate values which will be given by the above equation. Reference to table 2, shows that the final mean length of the shoots was 197 cm. Let us assume that the limiting value of x is 210. Let this be

TABLE 3

LENGTH OF APRICOT SHOOTS AT WEEKLY INTERVALS DURING THE GROWING SEASON. VALUES OBSERVED AND CALCULATED FROM EQUATION (1)

t	x OBSERVED	$\{210[1-e^{-.095(t-1)}]\}_1$	$\{19.1\,e^{-.055t}\cos\frac{\pi}{14}\}_2$	x CALCULATED $\{\{\}_1+\{\}_2\}$	θ
0	0	−19.1	19.1	0	0
1	15.8	0	17.6	17.6	−1.8
2	41.5	19.1	15.4	34.5	7.0
3	54.5	36.3	12.7	49.0	5.5
4	68.6	52.1	9.5	61.6	7.0
5	78.0	66.4	6.3	72.7	5.3
6	85.6	79.4	3.0	82.4	3.2
7	93.9	91.4	0	91.4	2.5
8	97.2	102.0	−2.7	99.3	−2.1
9	99.6	111.8	−5.0	106.8	−7.2
10	106.7	120.8	−6.8	114.0	−7.3
11	118.5	128.8	−8.1	120.7	−2.2
12	127.4	136.0	−8.9	127.1	0.3
13	135.3	142.8	−9.1	133.7	1.6
14	140.8	148.8	−8.8	140.8	0.8
15	148.4	154.7	−8.1	146.6	1.8
16	154.8	159.5	−7.1	152.4	2.4
17	158.4	164.0	−5.9	158.1	0.3
18	161.3	168.1	−4.4	163.7	−2.4
19	166.8	172.0	−2.9	169.1	−2.3
20	173.4	175.6	−1.4	174.2	−0.8
21	179.4	178.8	0	178.8	0.6
22	184.2	181.2	1.3	182.5	1.7
23	188.0	184.0	2.3	186.3	1.7
24	190.1	186.5	3.2	189.7	0.4
25	191.9	188.6	3.8	192.4	−0.4
26	193.8	190.4	4.1	194.5	−0.7
27	195.6	192.2	4.2	196.4	−0.8
28	197.0	193.8	4.1	197.9	−0.9

taken as the value of a. By a series of approximations it was found that the value of k is near 0.095. We may then write:

$$x = 210\ (1 - e^{-.095t}) \tag{5}$$

The values of x corresponding to values of t were determined and are given in table 2, together with the observed length of the shoots.

While there is a general harmony between the calculated and observed length of the branches at the weekly intervals, the values are not sufficiently close to be satisfactory. An examination of the figures shows that during the first seven weeks the computed length of the branches is fairly correct; from the 8th to the 20th week the computed values are too large; from the 20th week to the end of the season, the two sets of values agree fairly well.

The formula appears to fit the growth in the initial and latter part of the season, but growth lags behind the calculated values from the 8th to the 20th weeks.

This leads us to consider the possibility that the length-growth of these apricot branches follows the course of a reaction which consists of two consecutive unimolecular reactions, one of which at first accelerates and later retards the other. (The dynamics of such reactions have been discussed by Mellor, *Chemical Statics and Dynamics* (1914).) It is, therefore, necessary to increase the previously obtained values of x by some quantity which will be large when t is small and which will be comparatively small when t is large.

The nature of this quantity was ascertained by shifting all the values of t toward the ordinate, which was done by substituting $t-1$ for t in each case. The values by which the resulting determinations differed from the observed values were then computed and the differences were found to be periodically alternately positive and negative. (Compare columns 2 and 3 in table 3.) An appropriate form expressing these fluctuating differences may be a function of t involving the cosine. This will equal 1 when the value of the angle is 0, will decrease as the angle increases, then be negative as the angle increases from 90° to 270°, and become positive again in the last quadrant. By trial it was found that the difference between the observed values and those of the exponential function could be approximately expressed by

$$19.1\ [e^{-.055t} \cos \frac{\pi}{14} t]$$

The equation as used was then

$$x = 210\ [1 - e^{.095\ (t-1)}] + 19.1\ [e^{-.055t} \cos \frac{\pi}{14} t] \tag{6}$$

The values of x corresponding to the time intervals from 0 to 28 are given in the fifth column of table 3. The root-mean-square deviation

$(\Sigma d^2/n)^{1/2}$ of these results is 3.4 cm. The agreement, therefore, is satisfactory.

Comparing the calculated values of x with the observed, it will be seen that there are 16 deviations on the + side and 12 deviations on the − side. In figure 2, the upper curve shows the calculated course of the growth process alongside of the actual observed values.

The writer is fully aware that equation (6) is somewhat cumbersome. Its claim for consideration is, however, an important one, viz., it satisfactorily represents the observed facts. In the present state of our knowledge it seems advisable to use a formula like the above rather than a simpler type which might not so accurately represent the actual growth reaction. Future studies may show that a satisfactory equation of simpler form exists.

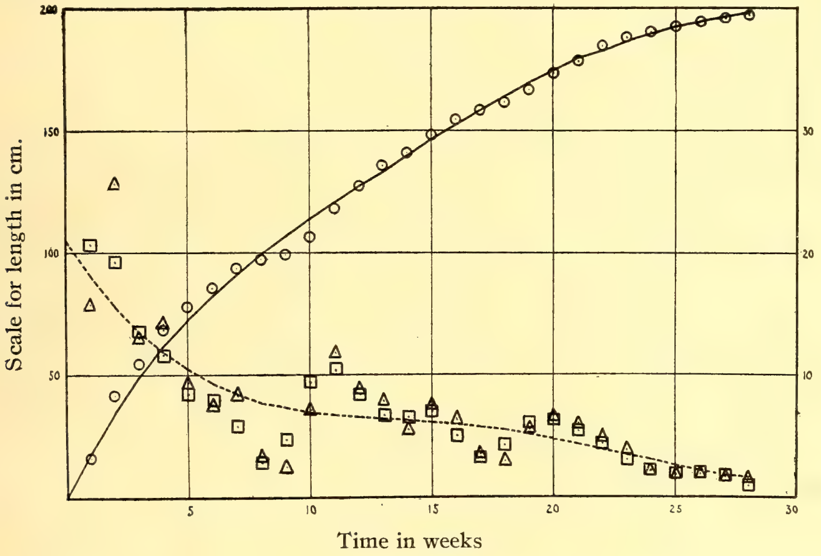

FIG. 2

Growth of apricot shoots. ———————— curve calculated from

$$x = 210[1 - e^{-.095\,(t-1)}] + 19.1\,[e^{-.055t}\cos\frac{\pi}{14}t]$$

– – – – – – – – – curve calculated from

$$\frac{dx}{dt} = 19.95\,e^{-.095\,(t-1)} - 19.1\,e^{-.055t}(0.055\cos\frac{\pi}{14}t + \frac{\pi}{14}\sin\frac{\pi}{14}t)$$

⊙ ⊙ ⊙, observed lengths of shoots.
△ △ △, observed increments of shoots.
⊡ ⊡ ⊡, "adjusted values" (S).

A consideration of equation (6) shows that we are here dealing with a homogeneous reaction, consisting of two unimolecular reactions, the first of which is always positive, and the second alternately positive and negative.

Putting this into terms of the tree, we may say that the growth of the apricot branches was, in the first 6 weeks, accelerated by amounts which diminished from 19.1 cm. at the outset to 3.0 cm. in the 6th week. From the 7th to the 21st week the growth was retarded by regularly varying amounts, and from then on, there was an acceleration, amounting at the most, to 4.2 cm.

The Dynamical Basis for Growth.—Our attention next passes to the question of the rate of growth. Since equation (6) represents the length of the apricot branches at successive intervals, we should obtain an equation by differentiating it, which will represent the rate at which they grow. Clearly, this relationship is of considerable physiological importance. By differentiating equation (6) we obtain the following

$$\frac{dx}{dt} = 19.95\ [e^{-.095\ (t-1)}] - 19.1\ [e^{-.055t}\ (.055\ \cos \frac{\pi}{14} t + \frac{\pi}{14} \sin \frac{\pi}{14} t)] \quad (7)$$

This expression will be more easily grasped if we obtain the values of the equation which correspond to different values of t and compare them with the rate per week (increments) actually observed as shown in table 4. The dotted line in figure 2 shows the graph obtained by plotting these values along with the observed values of the increments for the same intervals. The agreement between the two is good, with few exceptions. Better agreement might have been obtained had there been more frequent observations during the first two weeks; as it is, the number of observa-

TABLE 4

VALUES OF THE WEEKLY INCREMENTS IN LENGTH OF APRICOT BRANCHES, COMPARED WITH THE RATE OF GROWTH CALCULATED FROM THE FORMULA

$$\frac{dx}{dt} = 19.95[e^{-.095\ (t-1)}] - 19.1[e^{-.055t}(.055 \cos \frac{\pi}{14} t + \frac{\pi}{14} \sin \frac{\pi}{14} t)]$$

t	OBSERVED CM.	CALCULATED CM.	θ	t	OBSERVED CM.	CALCULATED CM.	θ
0		20.9		15	7.6	6.1	1.5
1	15.8	18.1	−2.3	16	6.4	6.0	0.4
2	25.7	15.6	10.1	17	3.6	5.7	−2.1
3	13.0	13.5	−0.5	18	2.9	5.4	−2.5
4	14.1	11.8	2.3	19	5.5	5.1	0.4
5	9.4	10.4	−1.0	20	6.6	4.7	1.9
6	7.6	9.3	−1.7	21	6.0	4.3	1.7
7	8.3	8.4	−0.1	22	4.8	3.9	0.9
8	3.3	7.7	−4.4	23	3.8	3.4	0.4
9	2.4	7.3	−4.9	24	2.1	3.0	−0.9
10	7.1	6.9	0.2	25	1.8	2.5	−0.7
11	11.8	6.7	5.1	26	1.9	2.1	−0.2
12	8.9	6.6	2.3	27	1.8	1.7	0.1
13	7.9	6.4	1.5	28	1.4	1.5	−0.1
14	5.5	6.3	−0.8				

tions in the early part of the growth period are too scanty to give sufficient data for more accurate determinations. It will be noted that half of the observed values lie above the graph and half below it. This gives confidence in the reliability of the calculated values. The root-mean-square deviation of the 28 values is 2.78 cm.

Table 5 gives the values of S in comparison with the computed values of dx/dt. S is computed from

$^1/_2$ [observed length at time $(t+1)-$observed length at time $(t-1)$].

TABLE 5

VALUES OF S COMPUTED FROM $^1/_2$ [(OBS. LENGTH AT TIME $t+1$) $-$ (OBS. LENGTH AT TIME $t-1$)] AND VALUES OF $\frac{dx}{dt}$.

t	S	$\frac{dx}{dt}$	θ	t	S	$\frac{dx}{dt}$	θ
1	20.7	18.1	−2.6	15	7.0	6.1	−0.9
2	19.3	15.6	−3.7	16	5.0	6.0	1.0
3	13.5	13.5	0.0	17	3.2	5.7	2.5
4	11.7	11.8	0.1	18	4.2	5.4	1.2
5	8.5	10.4	1.9	19	6.0	5.1	−0.9
6	8.0	9.3	1.3	20	6.3	4.7	−1.6
7	5.8	8.4	2.6	21	5.4	4.3	−1.1
8	2.8	7.7	4.9	22	4.3	3.9	−0.4
9	4.7	7.3	2.6	23	3.0	3.4	0.4
10	9.4	6.9	−2.5	24	2.0	3.0	1.0
11	10.3	6.7	−3.6	25	1.8	2.5	0.7
12	8.4	6.6	−1.8	26	1.8	2.1	0.3
13	6.7	6.4	−0.3	27	1.6	1.7	0.1
14	6.5	6.3	−0.2	28	0.7	1.5	0.8

If we use the values (S) which represent the slope of the chord whose mid-point corresponds to time, t, we shall obviate the minor fluctuations and get a more comparable expression of the observed rate of growth. McEwen and Michael (1919) have pointed out the usefulness of this method (p. 111) which depends upon the fact that the slope of the chord of a simple curve is approximately equal to that of the tangent at the point midway between the extremities of the chord. The root-mean-square deviations of these values is only 1.91 cm., which indicates a satisfactory agreement.

When these computations are translated into terms of the growing tree it appears that the growth rate in the early part of the season is high, but that it declines as the season advances. The rate of growth of the apricot branches indicates that the growth-impelling agency is at a maximum at the beginning of the season and that it diminishes as growth proceeds. In the first seven or eight weeks the growth rate declines more rapidly than in any other period. From the eighth to the nineteenth week the rate is fairly constant, only diminishing about 2.5 cm., but from that time on, the decline is considerably more rapid. At the end of the growing season the rate is less than one-tenth of the initial rate.

From a biological point of view, our interest in any equation representing the growth of organisms is not confined to evolving equations which shall represent their size at any given time. If we know the rate at which an organism grows and can express it quantitatively, we are in a position to analyze the processes by which it increases in size.

To facilitate matters, we may write equation (6) in a general form

$$x = a\,[1-e^{-k_1(t\,-\,1)}] + be^{-k_2 t}\cos \alpha t.$$

When differentiated, this becomes

$$\frac{dx}{dt} = ak_1e^{-k_1\,(t\,-\,1)} - be^{-k_2 t}\{k_2 \cos \alpha t + \alpha \sin \alpha t\}$$

which is the general form corresponding to equation (7). This can also be written in the form

$$\frac{dx}{dt} = k_1 \left[(a-x) + \frac{-be^{-k_2 t}\,\{(k_2-k_1)\cos \alpha t + \alpha \sin \alpha t\}}{k_1} \right]$$

For the sake of brevity let

$$Y = -be^{-k_2 t}\,[(k_2-k_1)\cos \alpha t + \alpha \sin \alpha t]$$

We may then write

$$\frac{dx}{dt} = k_1\,[(a-x) + Y/k_1]\ldots\ldots\ldots\ldots\ldots\ldots \qquad (8)$$

In the case of the apricot data

$$Y = 19.1\,e^{-.055t}\{-.04\,\cos \frac{\pi}{14}t + \frac{\pi}{14}\sin \frac{\pi}{14}t\}.$$

The advantage of this form of expression lies in the fact that Y may be expressed in terms of x, since equation (7) may be written

$$Y = \frac{dx}{dt} - k_1\,(a-x).$$

It seems, therefore, that the growth of these apricot shoots in one season conforms to that of a unimolecular consecutive reaction, in which the main reaction is influenced by a secondary reaction product. The rate of the main reaction would follow the path of curve A in figure 3, if it were alone operative. The secondary reaction (represented by curve B) has an additive effect on the main reaction and gives the resultant course shown by curve C. In the first part of the season, B has a negative value, its effect, therefore, is to diminish A. In the latter part of the season, B has positive values and accordingly augments A.

The writer is inclined to adopt the suggestion of Robertson (1913) concerning the progressive increase or decrease in the amount of growth-catalyst during a cycle. If we assume that in this sort of a reaction the amount of catalyst is in some way proportional to values of dx/dt, there would be in the present instance a progressive decrease during the growing season. If the initial excess of catalyst is unfavorable to growth then the effect of Y (which is negative during the first part of the season) would accelerate the reaction because it operates to cause a more rapid decrease in the amount of the catalyst. The amount of this acceleration is actually indicated by the values of $19.1\ [e^{-.055t} \cos \pi/14]$ in equation (6).

If the assumptions made in this study have any basis, it would seem that some light has been thrown upon the process of growth. It appears that growth proceeds at a definite rate, and is capable of mathematical expression. The growth rate appears to be proportional to the amount of a hypothetical catalyst and the intra-seasonal cycles of growth may be indicative of fluctuations in the amount of active catalyst in the organism.

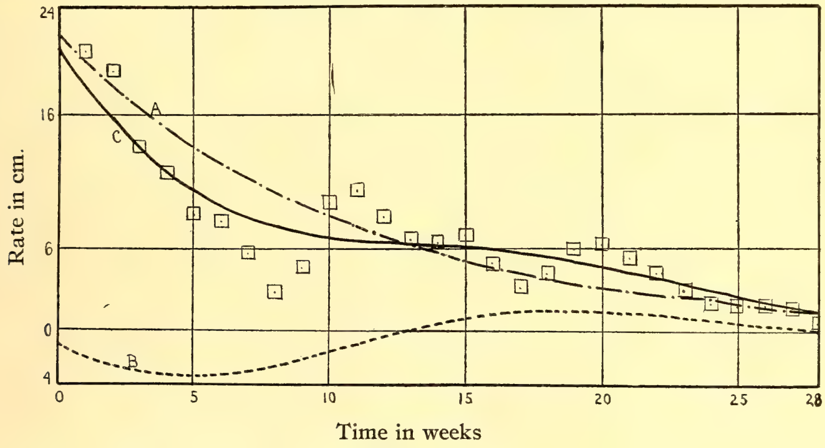

FIG. 3

Growth rate of apricot shoots.

—.—.—, (A), curve calculated from $ake^{-k_1(t-1)}$

———, (B), curve calculated from $-be^{-k_2t}[k_2 \cos at + a \sin at]$

————, (C), curve calculated from sum of values of (A) and (B).

⊡ ⊡ ⊡ observed increments.

Summary.—1. The growth of shoots on a selected sample of young apricot trees, followed a definite, though fluctuating rate. The maximum growth rate was exhibited soon after the growth of the season began, and declined with some regularity to the end of the season, though three distinct intra-seasonal cycles of growth were apparent.

2. The growth in each cycle closely resembled the rate of an autocatalytic reaction. The growth rate for the entire season conforms to that of a reaction consisting of two unimolecular reactions, one of which at first accelerates and subsequently retards the other.

3. If growth be assumed to be proportional to the amount of active catalyst present, a method is available for studying the dynamics of the growth process.

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Robertson, T. B. On the nature of the autocatalyst of growth. *Arch. Entwicklmech.*, **37**, 1913 (497–508).

[1] Paper No. 64, University of California, Graduate School of Tropical Agriculture and Citrus Experiment Station, Riverside, California.

[2] The writer wishes to acknowledge his great indebtedness to Dr. G. F. McEwen of the Scripps Institution for Biological Research of the University of California for valuable assistance in the mathematical work here reported.

ANALYTICAL NOTE ON CERTAIN RHYTHMIC RELATIONS IN ORGANIC SYSTEMS

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Periodic phenomena play an important rôle in nature, both organic and inorganic.

In chemical reactions rhythmic effects have been observed experimentally, and have also been shown, by the writer[1] and others,[2] to follow, under certain conditions, from the laws of chemical dynamics.

However, in the cases hitherto considered on the basis of chemical dynamics, the oscillations were found to be of the damped kind, and therefore, only transitory (unlike certain experimentally observed periodic reactions). Furthermore, in a much more general investigation by the writer, covering the kinetics not only of chemical but also of biological systems, it appeared, from the nature of the solution obtained, improbable[3] that undamped, permanent oscillations would arise in the absence of geometrical, structural causes, in the very comprehensive class of systems considered. For it seemed that the occurrence of such permanent oscillations, the occurrence of purely imaginary exponents in the exponential series solution presented, would demand peculiar and very specific relations between the characteristic constants of the systems undergoing transformation; whereas in nature these constants would, presumably, stand in random relation.

It was, therefore, with considerable surprise that the writer, on applying his method to certain special cases, found these to lead to undamped, and hence indefinitely continued, oscillations.

As the matter presents several features of interest, and illustrates certain methods and principles, it appears worth while to set forth the argument and conclusions here.

Starting out first from a broad basis, we may consider a system in the process of evolution, such a system comprising a variety of species of matter $S_1, S_2 \ldots S_n$ of mass $X_1, X_2 \ldots X_n$. The species of matter S may be defined in any suitable way. Some of them may, for example, be biological species of organism, others may be components of the "inorganic environment." Or, the species of matter S may be several components of an inorganic system in the course of chemical transformation.

We may think of the state of the system at an instant of time as being defined by statement of the values of $X_1, X_2 \ldots X_n$; of certain parameters Q defining the character of each species (in general, variable with time); and of certain other parameters P. The parameters P will, in general, define the geometrical constraints of the system, both at the boundaries (volume, area, extension in space), and also in its interior (structure, topography, geography); they will further define such factors as temperature and climatic conditions.

For a very broad class of cases, including those commonly treated in chemical dynamics, but extending far beyond the scope of that branch of science, the course of events in such a system will be represented by a system of differential equations of the form

$$\left.\begin{array}{c}\dfrac{dX_i}{dt} = F_i(X_1, X_2 \ldots X_n; P, Q)\\ (i = 1, 2 \ldots n)\end{array}\right\} \qquad (1)$$

If we restrict ourselves to the consideration of evolution at constant P's and Q's we may write briefly

$$\frac{dX_i}{dt} = F_i(X_1, X_2 \ldots X_n). \qquad (2)$$

The writer has elsewhere[4] given a somewhat detailed discussion of the general case, in which no special assumption is made regarding the form of the functions F, that is to say, regarding the mode of physical interdependence of the several species and their environment.

We now proceed to consider a simple special case, as follows:

The system comprises

1. A species of organism S_1, a plant species, say, deriving its nourishment from a source presented in such large excess that the mass of the source may be considered constant during the period of time with which we are concerned.

2. A species S_2, for example a herbivorous animal species, feeding on S_1.

In this case we have the following obvious relations

$$\left.\begin{array}{l}\text{Rate of in-}\\ \text{crease of } X_1\\ \text{per unit of}\\ \text{time}\end{array}\right\} = \left\{\begin{array}{l}\text{Mass of newly}\\ \text{formed } S_1 \text{ per}\\ \text{unit of time}\end{array}\right\} - \left\{\begin{array}{l}\text{Mass of } S_1\\ \text{destroyed by}\\ S_2 \text{ per unit of}\\ \text{time}\end{array}\right\} - \left\{\begin{array}{l}\text{Other dead}\\ \text{or excretory}\\ \text{matter elimi-}\\ \text{nated from } S_1\\ \text{per unit of}\\ \text{time}\end{array}\right\} \quad (3)$$

$$\left.\begin{array}{l}\text{Rate of in-}\\ \text{crease of } X_2\\ \text{per unit of}\\ \text{time}\end{array}\right\} = \left\{\begin{array}{l}\text{Mass of newly}\\ \text{formed } S_2 \text{ per}\\ \text{unit of time}\\ \text{(derived from}\\ S_1 \text{ as food in-}\\ \text{gested)}\end{array}\right\} - \left\{\begin{array}{l}\text{Mass of } S_2\\ \text{destroyed or}\\ \text{eliminated per}\\ \text{unit of time}\end{array}\right\} \tag{4}$$

Or, in analytical symbols,

$$\frac{dX_1}{dt} = A'_1X_1 - B_1X_1X_2 - A''_1X_1 \tag{5}$$

$$= (A'_1 - A''_1)X_1 - B_1X_1X_2 \tag{6}$$

$$= A_1X_1 - B_1X_1X_2 \tag{7}$$

$$= X_1(A_1 - B_1X_2) \tag{8}$$

$$\frac{dX_2}{dt} = A_2X_1X_2 - B_2X_2 \tag{9}$$

$$= X_2(A_2X_1 - B_2) \tag{10}$$

The coefficients A_1, A_2, B_1, B_2 are in general functions of X_1 and X_2.

The reasons for selecting the form (5), (9) for the analytical formulation of (3), (4) require perhaps a little explanation. For small changes the rate of formation of new material of a given species of organism under given conditions is proportional to the existing mass of that species. In other words, the growth of living matter is a typically autocatakinetic[5] process. This term has, therefore, been put in the form $A'\ X_1$ for the species S_1. Proportionality does not hold for large changes of X_1, X_2, and this is duly provided for in that A'_1 is a function of X_1, X_2.

Similarly the mass of matter rejected per unit of time from the species S_1 is proportional to X_1, and has been put in the form A''_1X_1, where A'' is in general a function of X_1, X_2.

Again, the mass of S_1 destroyed by S_2 feeding upon it will, for small changes, be proportional to X_2 and also to X_1. This term has, therefore, been set down in the form $B_1X_1X_2$. Here again the departures from proportionality are taken care of by the variations of B_1 with X_1 and X_2, of which variables B_1 is a function.

Similar remarks apply to the formulation (9) of (4). If there were no waste in the feeding process, and assuming that S_2 consumes no other substance than S_1, we would have $B_1 = A_2$; but in the more general case $B_1 \neq A_2$. Approaching now the analytical treatment of equations (5), (9), or their equivalents (8), (10), we note first of all that there are two ways of satisfying the condition for equilibrium, namely:

$$X_1 = X_2 = 0 \tag{11}$$

and

$$X_1 = \frac{B_2}{A_2};\ X_2 = \frac{A_1}{B_1} \tag{12}$$

We shall return later to the condition (11).

Condition (12) we will employ to define a new origin. Accordingly we introduce into (8), (10) the variables:

$$x_1 = X_1 - \frac{B_2}{A_2} \tag{13}$$

$$x_2 = X_2 - \frac{A_1}{B_1} \tag{14}$$

and obtain

$$\begin{aligned} \frac{dx_1}{dt} &= a_{12}x_2 + a_{112}x_1x_2 \\ \frac{dx_2}{dt} &= a_{21}x_1 + a_{212}x_1x_2 \end{aligned} \tag{15}$$

where

$$a_{12} = -\frac{B_1B_2}{A_2} \tag{16}$$

$$a_{112} = -B_1 \tag{17}$$

$$a_{21} = \frac{A_1A_2}{B_1} \tag{18}$$

$$a_{212} = A_2 \tag{19}$$

Note the significant fact that in (15) the linear terms in the dexter diagonal are lacking. It is this circumstance which imparts an oscillatory character to the process.

For, since a_{12} and a_{21} are in general functions of x_1, x_2, let us expand them by Taylor's theorem and put

$$a_{12} = p_0 + p_1x_1 + p_2x_2 + \ldots. \tag{20}$$

$$a_{21} = q_0 + q_1x_1 + q_2x_2 + \ldots. \tag{21}$$

A general solution of the system of differential equations (15) is then

$$x_1 = P_1e^{\lambda_1 t} + P_2e^{\lambda_2 t} + P_{11}e^{2\lambda_1 t} + P_{22}e^{2\lambda_2 t} + \ldots. \tag{23}$$

$$x_2 = Q_1e^{\lambda_1 t} + Q_2e^{\lambda_2 t} + Q_{11}e^{2\lambda_1 t} + Q_{22}e^{2\lambda_2 t} + \ldots. \tag{24}$$

where λ_1, λ_2, are the roots of the determinental equation for λ

$$\begin{vmatrix} -\lambda & p_0 \\ q_0 & -\lambda \end{vmatrix} = 0 \tag{25}$$

that is to say,

$$\lambda = \pm\sqrt{p_0q_0} \tag{26}$$

Now, according to (20), (21) p_0, q_0 are the equilibrium values of a_{12}, a_{21}. Hence, if we denote by $\bar{A}_1$, B_2 the equilibrium values of A_1, B_2, i.e., those values which correspond to $x_1 = x_2 = 0$, then we have, by (16), (18)

$$p_0q_0 = -\bar{A}_1\bar{B}_2 \tag{27}$$

and hence

$$\lambda = \pm\sqrt{-\bar{A}_1\bar{B}_2} \tag{28}$$

Now the coefficient B_2 is, in the nature of things, a positive number, as follows from its definition by (4), (9).

As regards the coefficient A_1, we have two possible alternatives.

If A_1 is negative for all values of X_1, X_2, then λ, as defined by (28), would be real; but this inference is nugatory. For B_1, like B_2 is, by definition (3), (5), an essentially positive quantity, and hence the equilibrium defined by (12) would in this case occur at a negative value of X_2. But this is physically impossible, since X_2 is a mass.

By referring to (5), (7) it will be seen that this case, in which A_1 is negative for all values of X_1, X_2, and in which an equilibrium of the type defined by (12) is physically impossible, corresponds to a species S_1 incapable of maintaining itself even in the absence of the tax placed upon it by the species S_2 feeding upon it. This is a case of minor interest.

If, on the contrary (12) can be satisfied by a positive value of A_1, so that an equilibrium of the type (12) is physically possible, then, evidently, by (28), λ is a pure imaginary. The solution (23), (24) then takes the form of Fourier's series; the process is an undamped oscillation continuing indefinitely.

In this connection, it is interesting to recall a passage in Spencer's "First Principles," chapter 22, paragraph 173:

"Every species of plant and animal is perpetually undergoing a rhythmical variation in number—now from abundance of food and absence of enemies rising above its average, and then by a consequent scarcity of food and abundance of enemies being depressed below its average...... ......amid these oscillations produced by their conflict, lies that average number of the species at which its expansive tendency is in equilibrium with surrounding repressive tendencies. Nor can it be questioned that this balancing of the preservative and destructive forces which we see going on in every race must necessarily go on. Since increase of numbers cannot but continue until increase of mortality stops it, and decrease of numbers cannot but continue until it is either arrested by fertility or extinguishes the race entirely."

A question now arises. Do the curves representing the solution (23), (24) dip below the zero axes of X_1, X_2? This would mean that one or the other, or both, of the species S_1, S_2 would become extinct through the violence of the oscillations.

To answer this question we consider the relation:

$$\frac{dX_2}{dX_1} = \frac{X_2(A_2X_1 - B_2)}{X_1(A_1 - B_1X_2)} \qquad (29)$$

which is obtained from (8) and (10) by division. From the periodicity of x_1, x_2 (and, therefore, X_1, X_2) it follows that the curve defined in rectangular coördinates X_1, X_2 by (29) is a closed curve. Furthermore, this curve can never cross the X_1 axis, for at all points of this axis the first, and all the higher derivatives of X_2 with regard to X_1 vanish, as can be seen from (29) directly and by successive differentiations.

Similarly it can be seen that the curve defined by (29) can never cross the X_2 axis.

Hence, if any point on any integral curve of (29) lies within the positive quadrant, the whole of that curve lies in that quadrant. Thus the oscillations can never exceed the limits of positive values X_1, X_2.

We conclude, therefore, that under the conditions of the problem as here set forth, neither the species S_1 nor the species S_2 can become extinct through severity of the oscillations alone. In practice the eventuality might arise, however, that in the course of these oscillations one or the other species might be so thinned out as to succumb to any extraneous influence that might arise such as has not been taken into account in our present considerations.

We return now briefly to the consideration of the equilibrium defined by the equation

$$X_1 = X_2 = 0 \qquad (11)$$

Applying here the criterion set forth by the author elsewhere,[6] it is seen that when A_1 is positive the determinental equation for λ has at this point two real roots of opposite sign, which is characteristic of unstable equilibrium. If, on the other hand, A_1 is negative in the neighborhood of the origin of X_1, X_2, then the equilibrium here is found to be stable, the two roots for λ being both negative.

In conclusion it may be remarked that a system of equations identical in form with (8), (10) is obtained in the discussion of certain consecutive autocatalytic chemical reactions. Here, however, the coefficients A, B are constants and the integration can be reduced to a quadrature. Aside from a certain number of periodic reactions which have been observed more or less as laboratory curiosities, a certain interest is also attached to this matter from the fact that rhythmical reactions (e.g., heartbeat, which may continue after excision), play an important rôle in physiology. We cannot, of course, say whether in such case geometrical (structural) features are the dominating factors.

[1] Lotka, A. J., *J. Phys. Chem.*, **14,** 1910 (271–274); *Zs. physik. Chem.*, **72,** 1910 (508–511); **80,** 1912 (159–164); *Phys. Rev.*, **24,** 1912 (235–238); *Proc. Amer. Acad. Arts Sci.*, **55,** 1920 (137–153).

[2] Hirniak, J., *Zs. physik. Chem.*, **75,** 1910 (675); compare also Lowry and John, *J. Chem. Soc.*, **97,** 1910 (2634–2645).

[3] Lotka, A. J., *Proc. Amer. Acad.*, loc. cit., p. 145, footnote 13.

[4] Lotka, A. J., *Science Progress*, **14,** 1920 (406–417); *Proc. Amer. Acad.*, loc. cit.

[5] Ostwald, Wo., *Über die zeitlichen Eigenschaften der Entwickelungsvorgänge*, Leipsic, 1908, p. 36.

[6] Lotka, A. J., *Proc. Amer. Acad.*, loc. cit., p. 144, et seq.

ON THE CLASS NUMBER OF THE FIELD $\Omega(e^{2i\pi/p^n})$ AND THE SECOND CASE OF FERMAT'S LAST THEOREM

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In the July 1919 number of the *Bulletin of the American Mathematical Society*, page 458, I gave an expression for the residue of the first factor of the class number of the field defined by $e^{2i\pi/p}$, p being prime, with respect to the modulus p^a, in terms of Bernoulli numbers. In the present paper an analogous expression for the residue of the first factor of the class number of $\Omega(e^{2i\pi/p^n})$ modulo p, will be obtained and the result used to show that certain results due to Bernstein[1] on Fermat's last theorem do not have the generality stated by him. In view of the criticisms on Kummer's 1857 memoir on the last theorem which I have given elsewhere[2] it is then pointed out that up to the present time no rigorous proof of the theorem for the exponents 59 and 67 has been given.

Westlund[3] reduced the first factor of the class number of $\Omega(e^{2i\pi/p^n})$, if $n > 1$, to the form

$$h_1 = k \times \frac{\prod\limits_s \varphi(\theta^s)}{{}^1\!/_2\, p^{n-2}(p-1)^2 \;\; {}^1\!/_2\, np^{n-2}(p-1)^2-1} = kk_1 \quad (1)$$

where k is the first factor of the class number of $\Omega(e^{2i\pi/m^1})$, $\theta = e^{2i\pi/\mu}$, $\mu = \varphi(m)' = p^{n-1}(p-1)$, $m = p^n$, $m^1 = p^{n-1}$, r_i is the least positive residue of r^i, modulo p^n, r being a primitive root of p^n. The integer s takes on all odd values $< \mu$ except multiples of p, and the function $\varphi(\theta)$ defined by Westlund may be put in the form

$$\varphi(\theta) = r_0 + r_1\theta + \ldots\ldots + r_{\mu-1}\theta^{\mu-1}.$$

We shall now reduce k_1 modulo p. To do this a modification of the method used by Kronecker in reducing the first factor of the class number of $\Omega(e^{2i\pi/p})$ will be employed.[4] We have

$$(r - \theta^{-1})\varphi(\theta) = p^n g(\theta) \quad (2)$$

where

$$g(\theta) = q_0 + q_1\theta + \ldots\ldots + q_{\mu-1}\theta^{\mu-1}$$

and

$$q_i = \frac{rr_i - r_{i+1}}{p^n}.$$

From an argument used by Westlund we also have

$$\prod_s (r - \theta^{-s}) = \frac{r^{\frac{\mu}{2}} + 1}{r^{\frac{\mu^1}{2}} + 1},$$

where $\mu^1 = \varphi(m^1)$. Since r is a primitive root of p^n, then $r^{\frac{\mu^1}{2}} = -1 +$

$p^{n-1}a$, where a is prime to p. Hence $r^{\frac{\mu}{2}} = (-1 + p^{n-1}a)^p = -1 + p^n a + p^{2n-1}a_1$, where a_1 is a multiple $a_2 a$ of a. Thus

$$\prod_s (r - \theta^{-s}) = \frac{p^n\, a + p^{2n-1}\, a_1}{p^{n-1}\, a} = p + p^n\, a_2,$$

whence

$$(1 + a_2\, p^{n-1}) \frac{\prod\limits_s \varphi(\theta^s)}{{}^1/_2\, np^{n-2}(p-1)^2 - 1} \equiv \prod_s g(\theta^s)$$

and since

$$2^{1/2 p^{n-2}(p-1)^2} \equiv 1 \pmod{p},$$

we have

$$k_1 \equiv \prod_s g(\theta^s) \pmod{p}. \tag{3}$$

Now $\prod\limits_s g(\theta^s) = a$, where a is a rational integer. Consider the expression $\prod\limits_s g(x^s) - a$, where x is an indeterminate. This is a polynomial in x which vanishes for $x = \theta$, and hence is divisible by $V(x) = \prod\limits_i (x - \theta^i)$, i ranging over the $\varphi(\mu)$ integers less than and prime to μ, since $V(x)$ is irreducible in the domain of rational integers. We write then

$$\prod_s g(x^s) = a + V(x)W(x), \tag{4}$$

where $W(x)$ is a rational integral function of x. Let

$$V_s = V_s(x) = \prod_i (x - \omega^i)$$

where i ranges over all the integers less than s and prime to it, and ω is a primitive s^{th} root of unity. Then

$$\frac{x^\mu - 1}{x^{\mu'} - 1} = V_\mu V_{c_1} V_{c_2} \ldots\ldots, \tag{5}$$

where $c_1, c_2 \ldots\ldots$ are all the numbers of the form $p^{n-1}k$, where k is a divisor of $p - 1$ which is $< p - 1$. Since r is a primitive root of p^n it is also a primitive root of p. Also, (5) gives

$$\frac{r^\mu - 1}{r^{\mu'} - 1} = V_\mu(r) V_{c_1}(r) V_{c_2}(r) \ldots\ldots \tag{6}$$

Now no V_c can be divisible by p since it would then follow that

$$r^c \equiv 1 \pmod{p}$$

which is impossible since c is not a multiple of $p - 1$. Since the left-hand member of (6) is divisible by p, we ,therefore, conclude that $V_\mu(r) \equiv 0 \pmod{p}$. Substituting $x = r$ in (4) and using (3), we have

$$k_1 \equiv \prod_s g(r^s) \pmod{p}. \tag{7}$$

By Fermat's theorem we have

$$\prod_s g(r^s) \equiv \prod_t (g(r^{2t-1}))^{p^{n-2}(p-1)} \pmod p, \qquad (7a)$$

where $t = 1, 2, \ldots\ldots, \frac{p-1}{2}$. To reduce $g(r^{2t-1})$ modulo p, we have

$$rr_i = r_{i+1} + rr_i - r_{i+1}$$

Raising to the power $2t$, and noting that $rr_i - r_{i+1} \equiv 0 \pmod{p^n}$, we get

$$r^{2t}r_i^{2t} \equiv r_{i+1}^{2t} + 2t(rr_i - r_{i+1})r_{i+1}^{2t-1} \pmod{p^{2n}}.$$

Transposing, we have

$$2tr_{i+1}^{2t-1}(rr_i - r_{i+1}) \equiv r^{2t}r_i^{2t} - r_{i+1}^{2t} \pmod{p^{2n}}.$$

But, by definition,

$$r_{i+1} \equiv r^{i+1} \pmod{p^n}$$

and therefore

$$(rr_i - r_{i+1})r_{i+1}^{2t+1} \equiv (rr_i - r_{i+1})r^{(i+1)(2t-1)} \pmod{p^{2n}},$$

whence, if $i = 0, 1, \ldots\ldots, \mu - 1$,

$$2tp_n r^{2t-1}\sum_i q_i r^{i(2t-1)} \equiv r^{2t}\sum_i r_i^{2t} - \sum_i r_{i+1}^{2t} \pmod{p^{2n}}.$$

Now

$$\Sigma r_i^{2t} = \Sigma r_{i+1}^{2t} = a_1^{2t} + a_2^{2t} + \ldots\ldots + a_\mu^{2t} = S_{2t},$$

where the a's are the integers less than p^n and prime to it. Hence

$$\prod_t g(r^{2t-1}) \equiv \prod_t \frac{(r^{2t}-1)S_{2t}}{2tp^n r^{2t-1}} \pmod p. \qquad (8)$$

To reduce the expression on the right-hand side we shall first consider the quantity S_{2t}. It will be shown that

$$S_{2t} \equiv R_{2t} \pmod{p^{n+1}} \qquad (9)$$

for $t < (p-1)/2$ and

$$S_{2t} \equiv R_{2t} \pmod{p^n} \qquad (9a)$$

for $t = (p-1)/2$, where

$$R_{2t} = 1^{2t} + 2^{2t} + \ldots\ldots + (p^n - 1)^{2t}.$$

We have

$$\begin{aligned} S_{2t} &= R_{2t} - (p^{2t} + (2p)^{2t} + (3p)^{2t} + \ldots\ldots + (p^n - p)^{2t}) \qquad (9b) \\ &= R_{2t} - p^{2t}(1^{2t} + 2^{2t} + \ldots\ldots + (p^{n-1} - 1)^{2t}). \end{aligned}$$

Now

$$T_{2t} = 1^{2t} + 2^{2t} + \ldots\ldots + (p^{n-1} - 1)^{2t} \equiv 0 \pmod{p^{n-1}}$$

for $t < (p-1)/2$ and

$$T_{p-1} \equiv 0 \pmod{p^{n-2}}.$$

To show this let k be a primitive root of p^{n-1}, then $k^{2t}T_{2t} \equiv T_{2t} \pmod{p^{n-1}}$ and

$$(k^{2t} - 1)T_{2t} \equiv 0 \pmod{p^{n-1}}.$$

In this relation $k^{2t} - 1$ is prime to p for $t < (p-1)/2$ and for $2t = p - 1$, $k^{2t} - 1$ is divisible by p but not by p^2. Hence

$$T_{2t} \equiv 0 \pmod{p^{n-1}}$$

for $2t < p-1$ and

$$T_{p-1} \equiv 0 \pmod{p^{n-2}}. \tag{9c}$$

Applying these to (9*b*), the relations (9) and (9*a*) follow. We also have by the Bernoulli summation formula

$$R_{2t} = \frac{p^{n(2t+1)}}{2t+1} + \frac{p^{2tn}}{2} + \binom{2t}{1}\frac{B_1}{2}p^{n(2t-1)}$$
$$-\binom{2t}{3}\frac{B_2}{4}p^{n(2t-3)} + \ldots\ldots + (-1)^{t+1}B_tp^n.$$

where $B_1 = {}^1/_6$, $B_2 = {}^1/_{30}$, etc.

By the von Staudt-Clausen theorem, the Bernoulli numbers B_1, B_2, $\ldots\ldots$, $B_{(p-3)/2}$ all have denominators prime to p. Hence if $2t < p-1$ every term in the above expansion is an integer, or else a fraction whose denominator is prime to p. Also the numerators except in the last term, are divisible by p^{2n}. If $2t = p-1$, then the denominator of B_{p-1} is divisible by p but not by p^2 and, therefore, we may write

$$R_{p-1} \equiv p^{n-1}a \pmod{p^{2n}} \text{ (a an integer prime to p).} \tag{10}$$

Hence we may write

$$R_{2t} \equiv (-1)^{t+1}B_tp^n \pmod{p^{n+1}}$$

and, therefore, for any $t < (p-1)/2$ we have

$$\frac{(r^{2t}-1)S_{2t}}{2tp^nr^{2t-1}} \equiv \frac{(r^{2t}-1)R_{2t}}{2tp^nr^{2t-1}} \equiv \frac{(-1)^{t+1}B_t(r^{2t}-1)}{2tr^{2t-1}} \pmod{p} \tag{11}$$

For $t = (p-1)/2$, the corresponding factor in the right-hand member of (8) takes the form

$$\frac{(r^{p-1}-1)S_{p-1}}{(p-1)p^nr^{p-2}}. \tag{11a}$$

We have from (9*a*)

$$S_{p-1} \equiv R_{p-1} \pmod{p^n}$$

and, therefore, the relation (10) shows that $(r^{p-1}-1)S_{p-1}$ is divisible by p^n but not by p^{n+1} since $r^{p-1}-1$ is divisible by p but not by p^2. Then modulo p, the expression (11*a*) may be reduced to an integer prime to p. Using this in connection with (11), (7*a*) and (8), we obtain

$$\prod_s g(r^s) \equiv \left(\prod_{t=1}^{(p-3)/2} B_t\right)^{p^{n-2}(p-1)} \pmod{p}. \tag{12}$$

Hence, $\prod\limits_s g(r^s)$ and, therefore, k, is divisible by p if and only if at least one of the numbers B_t ($t = 1, 2, \ldots\ldots (p-3)/2$) is divisible by p. (A Bernoulli number is said to be divisible by an integer i when its denominator is prime to i and its numerator is divisible by i.) Now Bernstein in his article cited above gives the result:

Under the assumption that the class number of $\Omega(e^{2i\pi/p^2})$ *is divisible by* p *but not by* p^2, *the relation*

$$x^p + y^p + z^p = 0 \qquad (13)$$

is impossible in integers prime to each other if $xyz \equiv 0 \pmod{p}$.

I shall now show that this criterion constitutes no extension over the one given by Kummer to the effect that (13) is impossible in integers if p is a regular prime. For if the former applies to other than regular primes as exponents, then p must be a prime such that

$$\prod_{i=1}^{(p-3)/2} B_i \equiv 0 \pmod{p}.$$

This being the case, it follows from (12) that $k_1 \equiv 0 \pmod{p}$, and by Kummer's theorem,[5] k is also divisible by p. Hence $h_1 \equiv 0 \pmod{p^2}$ contrary to Bernstein's assumption that the class number of $\Omega(e^{2i\pi/p^2})$ is not divisible by p^2.

In a paper cited above the writer pointed out errors in Kummer's memoir of 1857 on the relation (13). Considering this in connection with the failure of Bernstein's criterion it follows that Fermat's last theorem has not been rigorously proved for all non-regular primes less than 100. These non-regular primes are 37, 59 and 67. Mirimanoff[6] gave an adequate proof for the case $p = 37$, which leaves the cases $p = 59$ and 67 still to be disposed of.

In connection with the cyclotomic class number it may be noted that Furtwängler[7] proved that the class number of $\Omega(e^{2i\pi/p^n})$ is divisible by the prime p if, and only if, the first factor of the class number of the field $\Omega(e^{2i\pi/p})$ is divisible by p.

2. Bernstein[8] also gave a criterion in connection with (13) for the case where x, y and z are prime to p. He makes two assumptions the first being to the effect that the *second* factor of the class number of $\Omega(e^{2i\pi/p})$ is divisible by p, and states *l. c.*, p. 507, that his criterion includes that of Kummer's 1857 memoir as a special case. This is not correct, however, as there are no primes, p less than 100 such that the 2nd factor of the class number is divisible by p. (This follows from some computations by Kummer[9] in connection with certain investigations regarding the last theorem.) This being the case, Bernstein's criterion can at most supplement Kummer's which latter criterion stated that if (13) is satisfied in integers such that xyz is prime to p then $B_{(p-3)/3}$ and $B_{(p-5)/2}$ are divisible[10] by p.

This criterion would eliminate, for example, all p's less than 100, which Bernstein's condition does not do.

3. Kummer[11] rigorously established the theorem that (13) cannot be satisfied in integers prime to each other if p is a regular prime, and noted that all primes < 100 were regular except 37, 59 and 67. But Kummer[12] also showed that the first factor of the class numbers of $\Omega(e^{2i\pi/p})$ for all primes p, where $100 < p < 167$, excepting $p = 101$, 103, 131, 149 and 157, was in each case prime to p.[13] It follows that the primes p which are

> 100 and < 167 with the exceptions noted above are all regular and the last theorem is proved for these exponents p.

[1] *Göttingen Nachrichten*, 1910 (507–16).

[2] These PROCEEDINGS, **6,** 1920 (266).

[3] *Trans. Amer. Math. Soc.*, **4,** 1903 (201–12).

[4] Hilbert, *Die Theorie der Algebraischen Zahlkörpers*, p. 430

[5] Hilbert, *l. c.*, p. 429.

[6] *Crelle*, **111,** 1893 (26–30).

[7] *Ibid.*, **140,** 1911 (29).

[8] *L. c.*, pp. 482–88.

[9] *Abhandlungen Berlin Academy*, 1857, p. 73, verification of 2nd assumption.

[10] Kummer, *l. c.*, pp. 63–5.

[11] *Crelle*, **40** (93–139).

[12] *Berlin Monatsberichte*, 1874.

[13] Hilbert, *l. c.*, pp. 429, 435.

OBSERVATIONS ON THE STERILITY OF MUTANT HYBRIDS IN DROSOPHILA VIRILIS

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Communicated by C. B. Davenport, May 28, 1920

In an earlier paper[1] by Metz and Bridges, attention was called to two cases of apparent incompatibility between mutant races in Drosophila. One of these cases involved my data on the sex linked characters rugose and glazed in Drosophila virilis. Subsequent to the publication of this paper another character appeared in D. virilis that shows the same relations to rugose and glazed that they do to one another.[2] A study of these three characters has brought out some interesting relationships and has apparently revealed an error in the previous publication that should be corrected.

It may be recalled that hybrids between rugose and glazed were invariably sterile, while supposedly pure stock of each mutant was fertile and hybrids of either with other mutant stocks were fertile; hence it was concluded that rugose and glazed were incompatible. It appears now that a mistake was probably made in the statement that pure stocks of both rugose and glazed have normal fertility. Apparently this should have applied only to rugose, for later work has shown that females homozygous for glazed are usually sterile and that pure cultures cannot be maintained—at least without great difficulty. Possibly this condition has arisen by the secondary appearance of sterility factors in the stock since the previous paper was written, but more probably the earlier stock was impure, at least part of the time—the impurity having been overlooked through confusion with rugose in which the homozygous females are somatically normal. Unfortunately no accurate records of stock bottles were kept, so the point cannot be definitely settled; but in any event the present degree of sterility in glazed stock suggests an interpretation of the sterility of the hybrids which differs considerably from that advanced previously. This is emphasized even more by the behavior of the third character, wax, as will appear below.

The three (allelomorphic?)[2] mutants, rugose, glazed and wax, form a graded series in respect to their morphological characteristics and also in respect to fertility, but contrary to expectation the order of dominance of the somatic manifestations appears to be just the reverse of that of fertility. These features may be brought out more clearly by a brief summary of the salient points. I am indebted to Dr. Alexander Weinstein and Miss Eleanor D. Mason for obtaining many of the records involved here.

1. *Rugose;* characterized somatically by a slight paling and a roughening of the eye, evident only in the male, the female being entirely normal in appearance; in fertility both sexes seem to be fully equal to the wild type stock, as determined by a large number of matings and by the behavior of the pure stock cultures.

2. *Glazed;* more extreme in all respects; the eyes have a glazed appearance in both sexes, although the males are more noticeably affected; as noted above the females are usually sterile; among approximately 150 females tested[3] 3 were fertile, giving respectively 6, 7 and 8 offspring;[4] apparently the males are also affected somewhat, for they do not breed as well as do rugose males.

3. *Wax;* still more extreme; the eyes of both sexes are greatly affected, resembling masses of yellow wax; the females seem to be practically or entirely sterile; among approximately 100 tested no certain case of fertility was found. One bottle gave a single normal female, another gave a normal female and wax male. The first case is probably due to contamination, and the latter may be accounted for in the same way, although the presence of the wax male adds to the probability of actual, but very low, fertility here. The males, in turn, appear to breed more poorly than do the glazed males.[5]

4. *Hybrid females[6] from rugose × glazed;* somatic appearance normal as in rugose females; many tested, all sterile. (See Metz and Bridges, loc. cit.)

5. *Hybrid females from rugose × wax;* somatic appearance normal as in rugose females; approximately 100 tested, one fertile, giving 19 normal females, 8 rugose males and 5 wax males.[7]

6. Hybrids of rugose, glazed or wax with any other mutants thus far used are all fertile.

Reviewing these results it is seen that in each hybrid involving rugose and either glazed or wax, rugose is dominant as regards somatic manifestation. But the sterility of the hybrids seems to indicate that the fertility of rugose is recessive to the sterility of both glazed and wax. In the glazed-wax hybrid a similar relation holds for the somatic effect, glazed being dominant to wax. The high degree of sterility in both homozygous parent forms here, however, would naturally call for the observed sterility of the hybrid and eliminates the question of dominance with respect to this feature.

From the above standpoint sterility in the rugose-glazed and rugose wax hybrids is accounted for by assuming the dominance of sterility instead of assuming an incompatibility as was done in the previous paper when only the rugose-glazed hybrids were known. The present explanation calls for the occasional occurrence of fertile hybrids between rugose and glazed to correspond to the occasional fertility in pure glazed females. No such fertile hybrids have been obtained thus far, but this may be due merely to chance, since only a very few of them would be expected in the number of females tested. This conclusion is made still more probable by the occurrence of the one fertile hybrid between rugose and wax.

[1] These PROCEEDINGS, **3,** 1917 (673).

[2] These three characters are almost certainly allelomorphs, although the sterility of the hybrids and the dominance of the normal somatic constitution of rugose females makes it difficult to demonstrate the fact for the latter character. See Metz, C. W., *Genetics,* **3,** 1918 (116).

[3] The females were tested both singly and in mass cultures. The use of some mass cultures prevents recording the exact number of flies actually tested, for in these a few females may have died before they had a chance to breed.

[4] In each case these are such as to indicate fertility, not contamination or non-disjunction. In one case both parents were glazed, the offspring being 3 glazed females and 5 glazed males.

[5] It is difficult to obtain accurate determinations of the relative degrees of fertility of the males in these cases, for even the wax males breed fairly well, but in general the relations seem to be as described.

[6] Since the characters are sex linked and apparently allelomorphic no truly "hybrid" males can be obtained.

[7] In this and the preceding experiment offspring were obtained from two or three other bottles, but they were such as to indicate that they probably came from females heterozygous for one factor only, due to non-virginity or contamination.

STUDIES ON THE TRANSFORMATION OF THE INTESTINAL FLORA, WITH SPECIAL REFERENCE TO THE IMPLANTATION OF BACILLUS ACIDOPHILUS. I. FEEDING EXPERIMENTS WITH ALBINO RATS

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The type of bacteria developing in the alimentary tract may be influenced in a noteworthy manner, not only through special alterations in the chemical composition of the diet, but also through the ingestion of living cultures of *Bacillus acidophilus* (Moro) with or without accompanying carbohydrates.

When added in sufficient amounts to the basal diet of bread and meat, namely 2 grams daily, lactose and dextrin caused a very pronounced transformation of the intestinal flora of white rats within three to six days, and stimulated the development of *B. acidophilus* to such an extent that it entirely dominated the flora, thus suppressing or displacing almost completely all other bacterial types commonly found in the enteric tract. The simple flora persisted as long as the feeding of these carbohydrates was continued. *B. bifidus* (Tissier) occasionally increased in numbers under these dietary conditions, but as a rule it remained relatively obscure.

Maltose, saccharose and glucose exercised no transforming influence on the types of bacteria present. These results are in perfect agreement with those of Distaso and Schiller (1914) and Hull and Rettger (1917). Torrey (1919) claims, however, to have obtained a moderate increase in the numbers of *B. acidophilus* as a result of sucrose feeding in dogs.

The most plausible explanation of the favorable influence of lactose and dextrin feeding on the implantation of *B. acidophilus* is one which rests to a large extent upon the fact that in the rat at least they are not completely absorbed before they reach the large intestine. Here they establish an optimum environment by serving as a readily available source of energy for *B. acidophilus*. This theory was advanced by Hull and Rettger who found a direct relationship between the rapid development of *B. acidophilus* and the presence of a reducing substance in the contents of the colon and rectum after lactose feeding. These findings have been confirmed in the present investigation, not only in connection with the administration of lactose but also dextrin. No reducing substances could be detected, however, after the feeding of maltose, sucrose or glucose.

The stimulating influence of lactose and dextrin on the development of aciduric organisms in the intestine does not depend upon increased acidity of the intestinal contents, as hydrogen ion concentration determinations made on the fecal material of the caecum and colon of rats receiving these carbohydrates showed. The hydrogen ion concentration limits remained essentially the same during the preliminary and the transforming periods of the different experiments.

The feeding of 1 gram of lactose or dextrin daily to white rats brought about a partial transformation which never amounted to more than 50% of the entire flora. These results are in harmony with the findings of Hull and Rettger (1917) who likewise demonstrated that 2 grams of lactose are necessary to cause a complete transformation. They showed further that larger amounts than 2 grams of lactose stimulate *B. bifidus* to increased activity, and that when as much as 3 grams of lactose is administered daily the Tissier organism assumes the chief rôle, thus supplanting *B. acidophilus* even.

The addition of 1 cc. of *B. acidophilus* suspension (McFarland nephelometer turbidity scale of 5) to 1 gram of lactose or dextrin brought the *B. acidophilus* level to the same point as when 2 grams of either lactose or dextrin were fed. The simplification of flora was rapid and complete, and *B. acidophilus* persisted as the dominating type as long as the combined feeding of *B. acidophilus* and the carbohydrate continued. Furthermore, the administration of 2 cc. daily of the *B. acidophilus* culture or suspension without the carbohydrates results in the intestinal implantation of *B. acidophilus* and the complete suppression of the other bacterial types which are ordinarily present in the enteric tract of the white rat. The response of the different rats to the pure culture feeding was practically the same in all of the subjects. It was of special interest to note, further, that the various curves which represent the development of *B. acidophilus* in the feces of the rats were of practically the same height and character, whether 2 grams of lactose or dextrin, or 1 gram of either of these carbohydrates together with 1 cc. of the *B. acidophilus* culture, or 2 cc. of the culture alone were fed daily.

Post-mortem examination of the different sections of the alimentary canal of rats harboring the simplified flora in the lower intestine revealed a general distribution of *B. acidophilus* throughout the length of the intestine.

Since the bacteria within the digestive tract procure their pabulum directly or indirectly from the diet consumed by the host, it is but logical to assume that there must be a definite relationship between the chemical nature of the ingested food and the metabolic activities of the intestinal organisms. Many investigators have noted that marked changes in the intestinal flora follow sharp alterations in the diet, especially from a high protein to a high caloric diet and vice versa. However, the so-called "high calory diets" can at best accomplish a short-lived bacterial reformation only, that is a change in metabolic activities of organisms already present from the putrefactive to the fermentative type. This shifting of bacterial metabolism is not necessarily accompanied by an elimination of undesirable bacteria from the alimentary tract. Lactose and dextrin, among the carbohydrates studied, alone have the property of stimulating the development of aciduric organisms.

Repeated attempts in the present investigation to establish *B. bulgaricus* in the alimentary canal of albino rats through the administration of extremely large numbers (5 cc. of suspensions) of this organism were entirely unsuccessful. At no time was *B. bulgaricus* recovered from the feces or from any portion of the digestive tract, whether the organism was fed alone or together with a utilizable carbohydrate. The contention of Metchnikoff and his followers that *B. bulgaricus* lends itself readily to implantation in the digestive tract of man receives no support whatever from the present observations upon white rats.

Although considerable doubt must be cast upon Metchnikoff's explanation of the merits of the sour-milk therapy, we may be compelled to accept the fundamental principle of his arguments. That is, bacterial implantation with the concomitant transformation of the intestinal flora, is without doubt, both desirable and possible, but it is possible only when living cultures of properly qualified organisms are employed, or when such specific diets are administered which will stimulate and favor such a transformation. *B. bulgaricus* is not of intestinal origin and is incapable of accommodating itself to intestinal conditions. *B. acidophilus*, on the other hand, is of intestinal origin and has shown itself to submit readily to implantation in the intestine of the white rat at least.

In many of the experiments of Metchnikoff and his pupils large quantities of milk were consumed as milk cultures of *B. bulgaricus*. Hull and Rettger showed that milk, when given in sufficient amounts to rats causes a rapid development of *B. acidophilus* in the intestine which may result in the complete suppression of other forms of bacteria. These observations were confirmed in the present investigations, not only for rats but also in numerous milk-feeding experiments on man. Milk owes this property to the lactose. It is not at all unlikely that what Metchnikoff and his pupils regarded as *B. bulgaricus* in the feces of animals and of human subjects was in reality *B. acidophilus*. These two organisms resemble each other so closely that one may easily be mistaken for the other. The above assumption is strengthened further by the statement of Belonowsky (1907) that the intestinal flora of nursing mice was of the same character as that of the older mice receiving the *B. bulgaricus* (milk) culture.

The feeding of *B. bulgaricus* without due regard to the use of milk can have little or no importance attached to it. The beneficial results which have been attributed to yogurt and other oriental sour-milk products have in all probability been due to the milk as such, rather than to the acid-producing bacteria contained in these products.

For a full account of the present investigation the reader is referred to the doctorate thesis of Harry A. Cheplin in the Yale University Library. The methods and results of these experiments, together with those conducted on man, will be published at an early date in unabridged form.

ON A SINGLE NUMERICAL INDEX OF THE AGE DISTRIBUTION OF A POPULATION[1]

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Read before the Academy, April 27, 1920

It is an obvious fact that the crude death rate of any community is influenced in a marked degree by the age distribution of the living population of that community. Before any critical conclusions about the true force of mortality can be drawn some sort of correction must be applied to take account of the age distribution of the population. If one desires to make any analysis of the correlation of death rates with each other or with sundry environmental factors it is an absolutely essential prerequisite that there be found some single numerical expression which will be an approximate index of the age distribution of the population of each locality dealt with.

From a mathematical viewpoint the problem presented is one incapable of *exact* solution. The problem mathematically is to find a single parameter which will fully describe so complex a curve as that of the age distribution of a population, the general form of which is indicated in figure 1. But this is impossible. No single parameter can possibly describe fully such a curve. The best that can be done is to approximate as closely as may be to the impossible ideal. It is the purpose of this paper to describe and illustrate an approximation which comes very close to the requirements, indeed quite sufficiently so for all practical statistical purposes in all cases where it has yet been tested out, and is easy to calculate.

In a recent paper,[2] I proposed as an index of differences in age composition of populations, and applied it to 40 American cities, the expression

$$\chi^2 = S\left(\frac{\Delta^2}{P}\right) \qquad (1)$$

"where Δ is the deviation for each of six age groups (viz, 0–4, 5–14, 15–24, 25–44, 45–64, 65 and over) of the percentage of the actual population of each city in 1910 in each age group, from the percentage in the same group in the Standard Population of Glover's[3] Life Table, denoted in the formula by P. S denotes summation of all six values. The value χ^2 measures the extent to which each city deviates in the age constitution of its population from a fixed standard, but does not tell the nature or kind of the deviation."

In the paper cited it was shown empirically that in spite of the obvious defect of this index mentioned in the last sentence, it was adequate for the restricted material and purposes there involved. Since that paper was published it has occurred to me that the outstanding defect of the former index will, to that degree of accuracy which is requisite for all practical statistical purposes, be entirely removed by adopting as an age-constitution index the function

$$\phi = S\left\{\frac{\Delta^2}{P}\right\}(M - M_p) \qquad (2)$$

where S, Δ and P have the same significance as before, and M = mean age of living population in any community, M_p = mean age of persons in a stationary population unaffected by migration and which, assuming the mortality rates of Glover's Life Table, would result if 100,000 persons were born alive uniformly throughout each year (M_p calculated from L_x line of Glover's Table (p. 16) = 33.796 years).

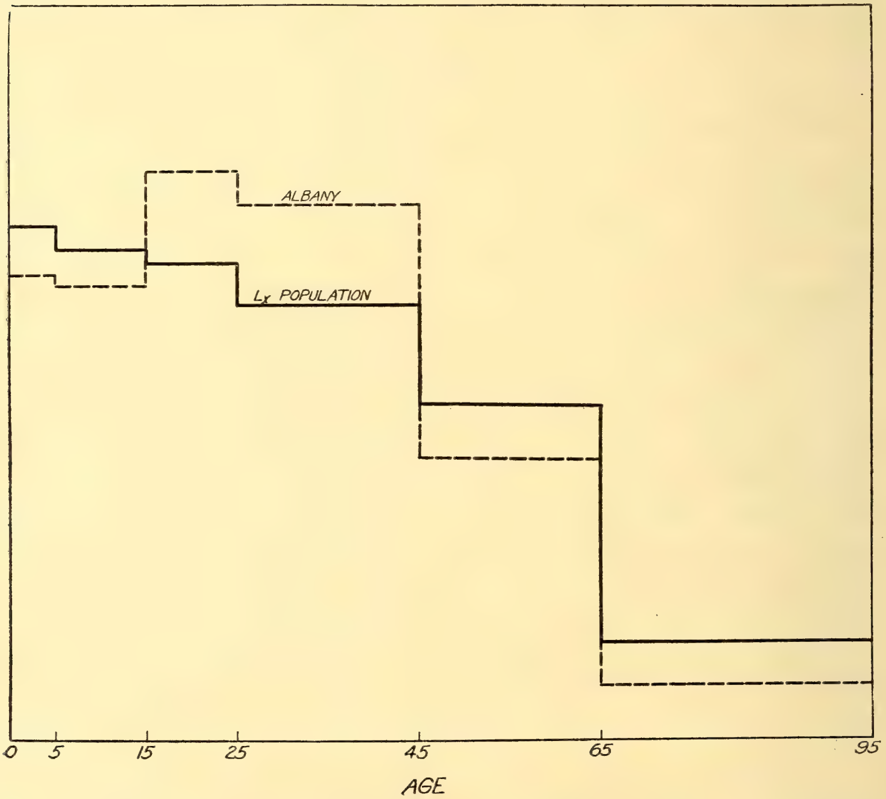

FIG. 1. Diagram showing age distribution of population, grouped in six age classes. The last class in this and other diagrams is assumed to have its ending at age 95. The solid line gives the stationary life table population, and the broken line the population of Albany. Total area under each curve = 100%.

This procedure simply multiplies our former index χ^2 by the difference (given its proper sign) between the mean age of the observed population and the mean age of the standard population on the basis of which χ^2 was calculated. Since in fact the mean age of any actual urban population is never likely to be as great as the mean age of the stationary population chosen as a standard of reference the actual values of ϕ will practically always be negative for cities. The smaller these negative values are numerically the greater will be the proportion of older persons in the population concerned. In short this function ϕ tells us not only the degree to which a given population deviates in its age distribution from a fixed standard age distribution, but also the nature of this deviation, whether on the one hand in the direction of a relative excess of aged, or on the other hand in the direction of a relative excess of the young. Theoretically it is possible for two populations differing from one another in a compensatory way to give the same values for the index ϕ. But two populations which differ in age distribution in any fundamental respect which could affect appreciably crude death rates will, in all populations I have been able to test, give different values of ϕ, provided the age classification from which the function is calculated is finely enough divided.

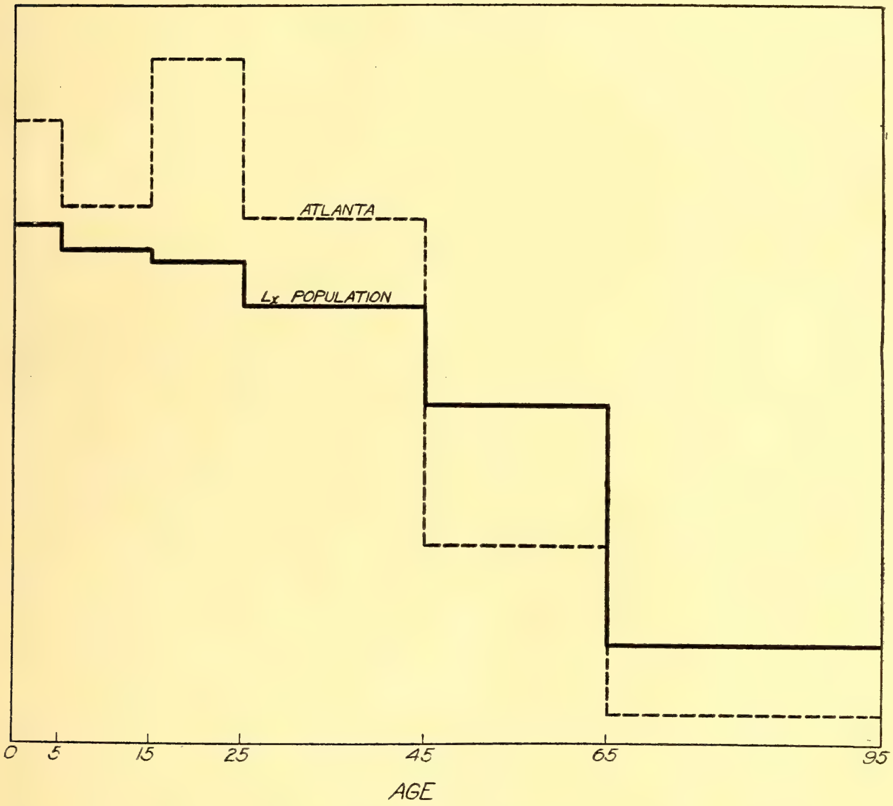

FIG. 2. Diagram showing age distribution of population, grouped in six age classes. The solid line gives the stationary life-table population, and the broken line the population of Atlanta. Total area under each curve = 100%.

II

The numerical values of the index ϕ for a series of American cities, on the basis of the age distribution of the 1910 census are given by way of illustration in table 1. The cities are arranged in descending order of departure from the L_x population.

The extent of the departures from the standard population denoted by the several values of the indices may be indicated graphically by plotting the age distribution curve, on a percentage basis, for the two extreme cities in the table, Albany and Atlanta, against the L_x population. This is done in figures 1 and 2.

The general characteristics of an urban population as compared with a stationary life table population are well shown in these diagrams. In general a city has population under 40 to 45 in excess, and population older than that in defect. The reason is obvious.

TABLE 1

SHOWING AGE-CONSTITUTION INDICES OF THE POPULATION OF AMERICAN CITIES

CITY	ϕ	CITY	ϕ
Albany	−10.73	Lowell	−34.91
Los Angeles	−13.61	Philadelphia	−39.51
Oakland	−17.18	San Francisco	−43.35
Washington	−20.67	St. Louis	−44.57
Cincinnati	−22.93	Nashville	−48.32
Dayton	−24.10	Buffalo	−48.61
Indianapolis	−24.23	New Orleans	−52.44
Rochester	−26.62	Minneapolis	−55.39
Grand Rapids	−27.95	Milwaukee	−62.17
Columbus	−29.08	Newark	−62.54
Providence	−30.05	Chicago	−68.76
Cambridge	−30.26	St. Paul	−68.95
Boston	−31.05	Pittsburgh	−71.77
Baltimore	−31.80	Fall River	−73.39
Louisville	−32.19	New York	−74.42
Toledo	−33.21	Cleveland	−74.51
New Haven	−33.70	Atlanta	−82.71

III

It will be well to see in detail how the age distributions of some cities having nearly the same value for ϕ compare in respect of actual age distribution of their populations. Table 2 furnishes such comparisons for four pairs of cities, covering fairly the whole rank of values of ϕ shown in table 1.

From this table it is seen that as we pass from cities having a value of ϕ of about 24 to cities having ϕ equal to about 74 there is a steady change from populations having relatively many persons in the higher age groups

TABLE 2

Percentage Age Distribution of the Population of Certain Cities in 1910

AGE CLASSES (YEARS)	DAYTON	INDIANAPOLIS	PROVIDENCE	CAMBRIDGE	NASHVILLE	BUFFALO	NEW YORK	CLEVELAND
	$\phi = -24.10$	$\phi = -24.23$	$\phi = -30.05$	$\phi = -30.26$	$\phi = -48.32$	$\phi = -48.61$	$\phi = -74.42$	$\phi = -74.51$
0– 4	9.1%	8.0%	9.7%	10.3%	9.2%	10.0%	10.6%	11.1%
5–14	15.4	15.3	16.5	17.5	17.8	18.3	18.1	17.4
15–24	19.5	19.4	19.0	18.4	21.9	20.6	20.8	20.5
25–44	34.6	35.9	34.2	33.3	32.2	32.3	33.9	34.4
45–64	17.0	17.0	16.2	16.0	15.1	15.5	13.7	13.4
65 & over	4.4	4.3	4.2	4.4	3.8	3.4	2.8	3.0
Totals	100.0	99.9	99.8	99.9	100.0	100.1	99.9	99.8

to populations having relatively few persons in those groups. Furthermore, it is evident that in each of the four pairs of cities compared the agreement between the two cities having nearly identical values of ϕ is very close in respect of actual percentage distribution of the population. Dayton and Indianapolis were for all practical statistical purposes identical in the age distribution of their populations in 1910. So were Providence and Cambridge, Nashville and Buffalo, and New York and Cleveland. In each case the curve for one distribution of the pair winds in and out about the path set by the other. Of course, we should get finer differentiations brought out by the ϕ function if we used 15 or 20 age classes instead of the 6 here employed. But for the purposes of the investigation in which I am using these functions, and for purposes of illustration of method 6 classes are sufficient. A word of caution must, however, be emphasized here. The reliability of ϕ as an approximate index of differences in age distributions of population is greater as we pass in either direction towards the ends of its range of values. In the case of populations giving values of ϕ near the mean (say in the thirties for American cities) it may be necessary in order to get really differentiant values to calculate from a rather fine age grouping.

In general it is believed that the function of an age distribution of a population here proposed will give, in a single numerical expression, a substantially accurate indication of the essential nature of that age distribution, and will facilitate the differentiation and classification of populations in respect of this characteristic for statistical studies, particularly by the method of multiple correlation.

[1] Papers from the Department of Biometry and Vital Statistics, School of Hygiene and Public Health, Johns Hopkins University, No. 16.

[2] Pearl, R., "Influenza Studies. I—On Certain General Statistical Aspects of the 1918 Epidemic in American Cities," *Public Health Repts.*, **34,** 1919 (1743–1783).

[3] Glover, J. W., *United States Life Tables,* 1910, Bureau of the Census, 1916.

UPON AN IMPORTANT METHOD OF STUDYING PROBLEMS OF RELATIONSHIP AND OF GEOGRAPHICAL DISTRUBUTION

By Maynard M. Metcalf

The Orchard Laboratory, Oberlin, Ohio

Communicated by H. S. Jennings, May 28, 1920

In the course of the study of one hundred and thirty-four species and twenty subspecies of Opalinidae I have found data of great significance as to the geographical distribution and the former migrations of the Anura, and I wish to call attention to the importance of the method of studying such problems, in the case of each group of animals and plants, from the point of view both of the members of that group and of their parasites. A couple of illustrations will suffice to show how such study of a double series of data may present indisputable evidence as to the interpretation of the phenomena of present distribution of a family of animals.

The Leptodactylidae are the characteristic "frogs" of tropical and south-temperate America. They are found also in fair abundance in Australia and Tasmania and nowhere else. This has been taken to indicate former land connection between Patagonia and Australia by way of Antarctica. There are those, however, who have questioned this conclusion, believing the resemblance between these American and Australian "frogs" to be due to convergent or parallel evolution. But the latter hypothesis is definitely excluded by a study of the Opalinid parasites of the Leptodactylidae.

There is a genus of flat binucleated Opalinidae abundant in South America and Central America and spreading into the Antilles and a little into the southern part of North America. This genus, which I am naming *Zelleriella*, is the characteristic Opalinid parasite of the Leptodactylidae. The point of interest in the present connection is the fact that both American and Australian Leptodactylids have *Zelleriella* parasites, and the Australian Zelleriellas so closely resemble the American forms that it is difficult to separate them specifically. It might be conceivable that the Australian and American "frogs" assigned to the family Leptodactylidae may not be closely related and that their resemblances are due to parallel or convergent evolution, but it is not possible that both the Leptodactylids and their Opalinid parasites have evolved in parallel or convergent lines on the American and Australian continents. Such a coincidence is altogether improbable. *Zelleriella* is confined to Southern America and Australia.

Study of the Opalinid parasites of numerous families and subfamilies of Anura has given indication of a number of interesting things. It indicates, for example, that at the time when Patagonia was connected with Antarctica there was, some bar to free migration between the northern part of South America and Patagonia, this bar being probably a shallow sea stretching across the middle of South America from ocean to ocean. Toads of the genus *Bufo* are abundant in tropical and temperate America, as they are in tropical and temperate Asia and the East Indies, but they have not reached Australia either by way of Antarctica or via New Guinea. The Bufos of South America carry *Zelleriella* parasites. Why did they not migrate to Australia by way of Antarctica carrying their Zelleriellas with them? The Leptodactylidae are a southern family having arisen apparently in the Patagonian region. The toads are a northern family. It seems clear that the toads were not present in Patagonia at the time when this region was connected with Australia by way of Antarctica. Apparently the shallow sea across the middle of South America separated the toads in the north from the Leptodactylids in the south. As Patagonia let go of Antarctica by subsidence of the intervening land area, there was a probably concomitant rise of land through what is now middle South America and the northern and southern portions of this continent came together. This allowed the toads and Leptodactylidae to meet, and the toads which are hospitable to all genera of Opalinidae accepted the Zelleriella parasites from the Leptodactylids. But it was now too late for the toads to migrate to Australia, the Antarctic route being barred by a wide stretch of newly formed ocean.

Numerous other problems of distribution are similarly illuminated by the study of the Anura and their Opalinid parasites. Other groups of Anuran parasites besides the Opalinidae should be studied with reference to problems of distribution, and every group of animals and of plants should similarly be studied with their parasites. In this way many questions of genetic relationship can be settled beyond doubt and many problems of geographical distribution and of migration routes can be solved. The method is one which should be used to the full because of the definiteness of the conclusions to which it leads. In all likelihood this method of study will in time settle the question of former land connection between Africa and northern South America as it has settled the question of former land connection between South America and Australia. The study of my own African and South American Opalinid material is not yet complete, so I do not discuss this problem here.

This paper is written merely to illustrate and emphasize the value of the method of studying relationships between groups of animals and plants, and their geographical distribution and migration routes, by means of comparison of their parasites and comparison of the distribution of the hosts with that of their parasites. The results in many cases are sure to be as surprisingly convincing as they have proven to be in the study of the Anura and their Opalinidae.

THE INFLUENCE OF COLD IN STIMULATING THE GROWTH OF PLANTS

By Frederick V. Coville

Smithsonian Institution, U. S. National Museum, Washington, D. C.

Communicated by C. D. Walcott. Read before the Academy, April 27, 1920

In regions having a cold winter like ours, with prolonged or repeated freezing, the native trees and shrubs, according to the general belief, become dormant because of the cold. It is also the general belief that warm weather is of itself the sufficient cause of the beginning of new growth in spring. Both these ideas are erroneous. The evidence now presented shows: first, that in our native trees and shrubs, dormancy sets in before cold weather, and that cold weather is not necessary for the establishment of complete dormancy; second, that after such dormancy has begun, the exposure of the plants to an ordinary growing temperature does not start them into growth; third, that these plants will not resume normal growth in the warm weather of spring unless they have been subjected previously to a period of chilling.

Dormant trees and shrubs which have had two or three months of chilling, either out of doors or in artificial cold storage, start into growth in the normal manner when spring comes, but if the dormant plants have been kept warm all winter they do not start into growth at the usual time in spring but continue their dormant condition for weeks or months, sometimes for a whole year, and when finally they do begin to grow their growth is of an abnormal character. One of these long dormant plants can readily be started into healthy growth, however, even after the expiration of a year, by subjecting it to a period of chilling. The best temperature for chilling is 32° to 40° Fahrenheit. It may be applied in either light or darkness.

In one of the greenhouses of the Department of Agriculture refrigerating machines have been installed in such a manner that plants supplied with normal daylight can be subjected at any period of the year to winter temperatures, even below freezing if desired.

By subjecting one part of a dormant bush to a chilling temperature and keeping the other part of it warm, the chilled portion can be brought into full leaf and flower while the other part remains completely dormant.

An important change takes place in the plant during the process of chilling. The starch stored in the cells is transformed to sugar, and not until this has been done can the plant utilize its store of starch in making its spring growth. Furthermore, the transformation of the starch into sugar creates high osmotic pressures within the plant. Certain sugar-exuding glands, called extra-floral nectaries, are interpreted as safety valves for the relief of excessive internal pressures which might burst the cells of the plant or otherwise derange its physiological activities.

The fact that our northern trees and shrubs after they become dormant in the fall require a period of chilling before warm weather will start them into growth again, is a protective adaptation of the highest importance to these plants, for if warmth alone would start them into growth they would begin growing in Indian summer and the stored food that the plant requires for its normal vigorous growth in the following spring would be wasted in a burst of new autumn growth, which would be killed by the first heavy freezes, and would be followed by a winter of weakness and probable death.

Further investigations on the effects of chilling are urged upon those engaged in experimentation bearing on the improvement of horticultural and agricultural practices. It is desirable especially to determine the proper temperatures for the storage of different kinds of seeds, bulbs, cuttings, and grafting wood; proper temperatures for the treatment of plants which are to be forced from dormancy to growth at unusual seasons; and proper temperatures for the storage of nursery stock, so that the nurseryman may have plants in proper condition for shipment on any date he desires.

ON THE NATURE OF THE NEGATIVE CARRIERS PRODUCED IN PURE HYDROGEN AND NITROGEN BY PHOTO-ELECTRONS

BY LEONARD B. LOEB*

RYERSON PHYSICAL LABORATORY, UNIVERSITY OF CHICAGO

Communicated by R. A. Millikan, June 5, 1920

As the result of an investigation of the cause of the abnormalities of the negative ions in air at low pressures (which is to appear shortly), it was found that the results obtained could be explained quite satisfactorily on the basis of a theory proposed by Sir J. J. Thomson.[1] This theory assumes that the electron does not attach to a neutral molecule to generate a negative ion on its first impact, but that on the average it will have a chance of uniting in one out of n impacts with a given type of molecule, where n is a constant which is a characteristic of the type of molecule considered. For air this constant was found to be in the neighborhood of 2.5×10^5. If the oxygen molecule is the molecule to which the negative electron attaches in air—a point of view for which there is considerable evidence—this means that in only one out of 5×10^4 encounters with oxygen molecules does the electron have a chance of attaching itself to a molecule to form a negative ion. It accordingly seemed of interest to see how nitrogen and hydrogen molecules behaved in respect to this theory, and to determine n for them if possible.

The method used for the investigation of this question was to measure the mobilities of the carriers generated as photoelectrons from one plate of a condenser using the Rutherford[2] alternating current method. Light from a quartz mercury arc passed through a quartz window sealed into the side of the brass case housing the condenser and was focussed upon the lower plate of the condenser by means of a quartz lens. The diameter of the plates was 10 cm. and the distance between them 1.65 cm. The upper plate of the condenser was connected to a quadrant electrometer of sensibility 3700 mm. per volt. The lower plate led to a commutator fed by a large battery of dry cells and capable of giving an alternating potential of square wave form of frequency from 15 cycles per second to 750 cycles per second. Connections were so arranged that the positive, or retarding, side of the wave was always about 20% higher than the corresponding negative, or accelerating, side of the wave.

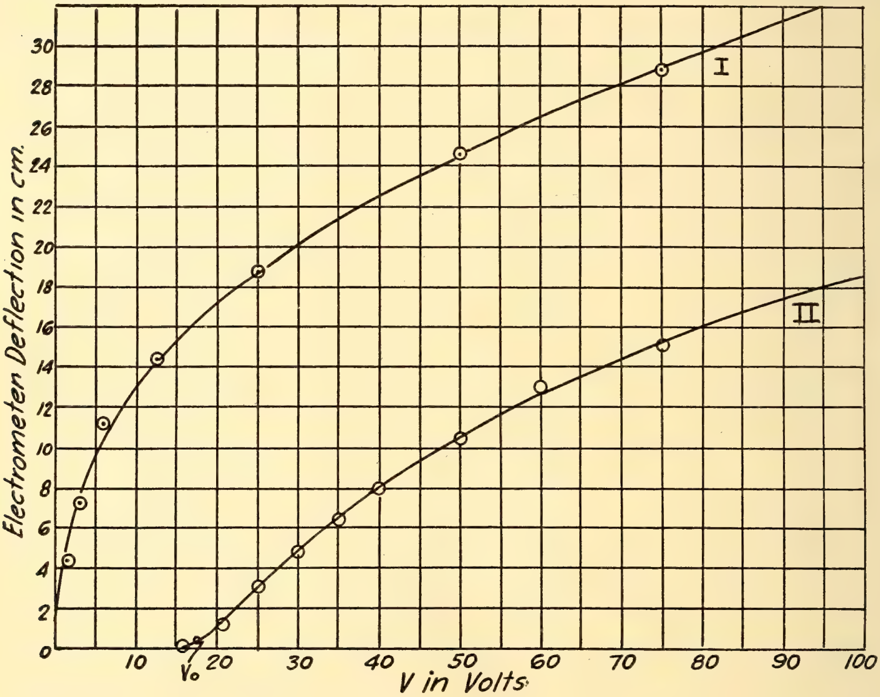

FIG. 1

Air. Press = 95 mm. I—Saturation; II—N = 148

The nitrogen gas which came from a tank of commercial nitrogen was passed over red hot CuO, red hot Cu, over KOH, and $CaCl_2$, over P_2O_5 and finally through a liquid air trap. The commercial hydrogen used was purified in the same way omitting naturally the tube of CuO. The system including the metal housing for the condenser plates was set up so as to eliminate as far as possible all organic vapors such as those coming from stopcock greases and sealing waxes. The filling of the chamber was accomplished by exhausting to 1.5 cm. pressure and filling slowly with the purified gas six or seven times.

The curves numbered I in figures 1, 3 and 4, give the characteristic photoelectric saturation curves obtained in air at 95 mm. pressure, and in hydrogen and nitrogen at about 750 mm. pressure, when the current to the electrometer plate, under a *constant* accelerating potential is plotted against the potential. When the measurement of the current to the upper plate was made using the alternating potentials the results obtained in air, and in hydrogen contaminated with O_2 are entirely different. The current remains nearly 0 until a certain critical voltage V_o is reached, and then rises rapidly eventually approximating a curve of the same shape as the saturation curve, but of half the ordinate. A curve of this type obtained in air at a pressure of 95 mm. using a frequency of alternation of 148 cycles is shown in figure 1, curve II. Figure 2, curves II and III show the results obtained in a mixture of 98.4% hydrogen and 1.6% oxygen at pressures of 293, and 151 mm., respectively. The frequency of the alternating potential was 362 cycles per second. Taking the value of the intercept of these curves with the voltage axis (V_o), one can de-

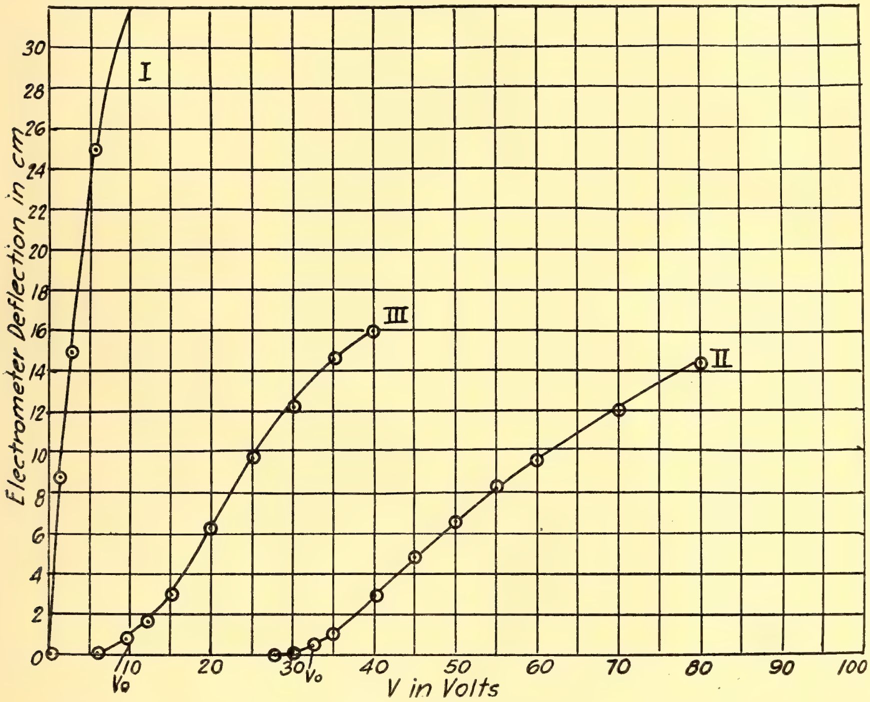

FIG. 2

H_2 743 mm. O_2 12 mm. I—Saturation; II—Press = 293 mm., N = 360; III—Press = 151 mm., N = 364 termine the velocity (u) of the ion in unit field from the equation $u = Nd^2/V_0$ (where d is the distance between the plates and N is the frequency of commutation). By multiplying u by the ratio $p/760$ (where p is the pressure at which the measurement was made) one obtains the mobility constant k of the ion. Now the mobility constant of an ion under normal conditions is of the order of magnitude of 2 cm. per second while that of the electron, though not definitely known, is of the order of magnitude of 200 cm. per second or greater. It is obvious then that by determining the mobilities of the carriers in the gases the nature of the carriers may be determined.

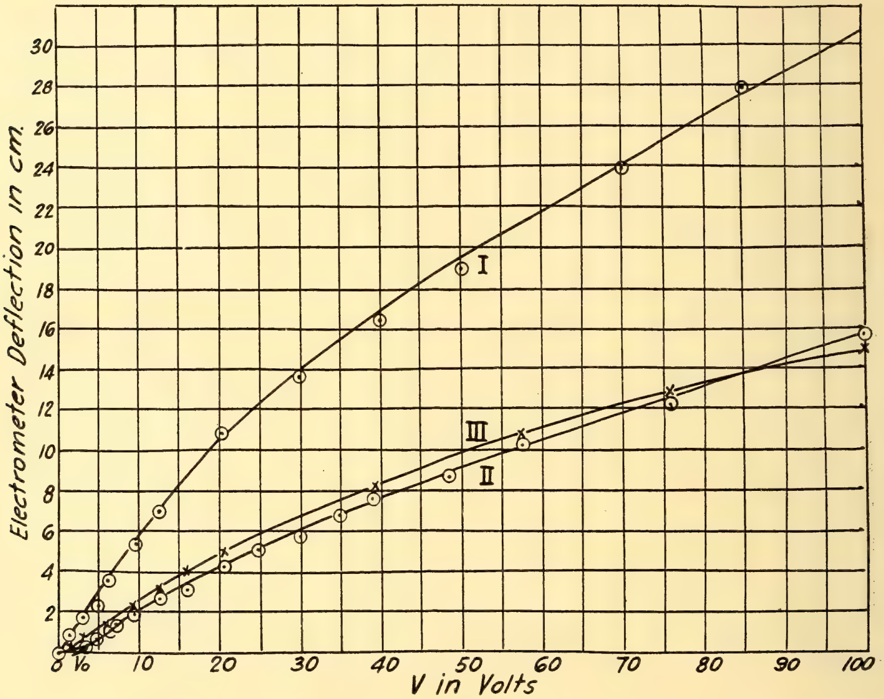

FIG. 3

Pure H_2. Press = 730 mm. I—Saturation; II—N = 712; III—N = 14.5

The value of k obtained from the curves for air is 3.25 cm./sec. The values of k obtained in the mixture of hydrogen and oxygen above are 12.9 and 20.3† cm/sec. respectively, for the two pressures 293 and 151 mm. Since the normal values of k in air and in hydrogen are of the order of 2 cm./sec. and 7.5 cm. /sec. respectively, while those for the electrons in these gases are above 200 and 750 cm./sec., it is to be concluded that the carriers observed above are chiefly ionic in character.

When, however, these measurements were carried out in the carefully purified gases N_2 and H_2 the results were very different. These are illustrated in curves II and III, figure 4 for N_2, using frequencies of 714 and

35.7 cycles per second at a pressure of 753 mm. Curves II and III, figure 3, show the curves obtained in pure H_2 at a pressure of 730 mm. and using frequencies of 712 and 14.5 cycles, respectively.

These curves are practically saturation curves, their intercept with the voltage axis lying very close to the origin and being nearly independent of the frequency of alternation. Their ordinates are, however, but half of those for the corresponding saturation curves. This is to be expected since the time during which the electrometer deflection was measured was the same for the saturation curves as for the curves taken with alternating potentials, while with the alternating potentials the accelerating field was on but half the time. The ordinates of the curves taken with the lower frequencies are in general slightly greater than those with the higher frequencies. This slight difference is probably due to the fact that the contacts at the commutator were slightly better at lower frequencies of alternation. The difference observed in the case of Nitrogen was due to a change in the intensity of the light from the mercury arc whose operating potential changed by two volts between the two determinations. Such a change easily accounts for the difference in the ordinates observed.

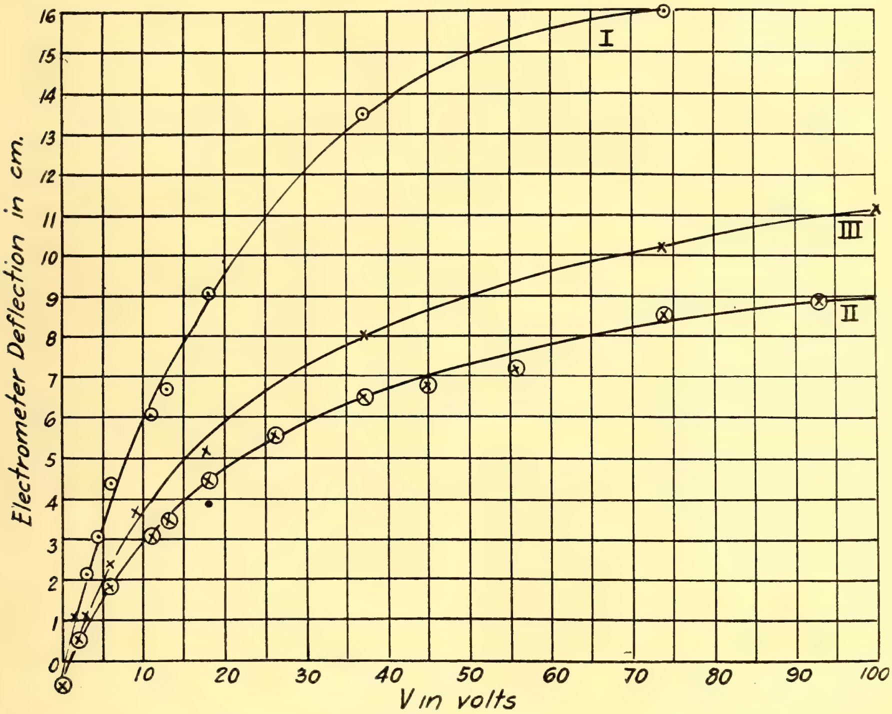

FIG. 4

Pure N_2. Press = 753 mm. I—Saturation; II—N = 714; III—N = 35.7

It is obvious that the carriers must be entirely electronic for the curves cut the voltage axis at values of V_0 so close to 0§ that the values of the mobility computed therefrom are greater than 750 cm./sec. The conclusion to be drawn is that in pure nitrogen and hydrogen gas the electrons do not attach themselves to the molecules to form ions in any appreciable quantities.

The results obtained in nitrogen are in agreement with those of Franck.[3] The latter used a much more laborious method for the purification of his nitrogen than did the writer. He also used electrons generated by the action of alpha particles on the gas molecules. It is possible that the use of liquid air in the process of purification employed above simplified that process. Both Wellisch[4] and Haines[5] found that in hydrogen even at atmospheric pressure a considerable number of their carriers were electronic in nature. Both of them, however, also found a large proportion of normal negative ions. That they did not obtain the complete absence of ions here obtained is not surprising. For if the purity of their gases had been a little less than that used by the writer, the fact that they generated their ions in an auxiliary chamber where they had considerable opportunity in picking up molecules of the impurity before reaching the measuring field would have given them appreciable numbers of ionic carriers. In his monograph on positive rays J. J. Thomson[6] states that he never observed a single instance of negatively charged nitrogen molecules in his positive ray spectrographs. He did in some cases find molecules of oxygen carrying a negative charge. In a few vary rare instances he found molecules of hydrogen with a negative charge. It is, of course, doubtful whether the results of Thomson with positive rays are in any sense contradictory to the results obtained above for hydrogen. The carriers which Thomson observed were generated in electrical fields of much greater intensity and at much lower pressures than were used in this work. Furthermore Thomson was able to observe carriers whose existence must have been much more transitory than those that the mobility measurements could detect.

According to Bohr's[7] theory the hydrogen molecule permits the incorporation of a third electron into the system to form a stable negative hydrogen ion. It is interesting to note that this does not occur to any noticeable extent under the conditions of the writer's experiments.

In conclusion the writer wishes to thank Professor R. A. Millikan for his very kind advice and criticism.

* NATIONAL RESEARCH FELLOW

† The values of k obtained (p. 438) are somewhat greater than those given for normal ions. These abnormal values of the mobilities are the result of the low pressures used. Their explanation on the basis of the Thomson theory will constitute the body of a later paper.

§ It is impossible to locate the value of V_0 for the curves in Nitrogen. They are too small. The curve in hydrogen obtained with 712 cycles appears to cut the axis near 2 volts. The value of k for the hydrogen carrier deduced from this lies above 900.

However, too much faith cannot be placed on intercepts taken so near the origin, with small deflections.

[1] Thomson, J. J., *London Phil. Mag.*, Sept., 1915.

[2] Rutherford, E., *Proc. Cambridge Phil. Soc.*, **9**, 1898 (401).

[3] Franck, J., *Verh. Deuts. Phys. Ges.*, **12**, 1910 (613).

[4] Wellisch, E. M., *Phil. Mag.*, **34**, 1917 (199); also *New Haven, Amer. J. Sci.*, July, 1917.

[5] Haines, W. B., *Phil. Mag.*, Oct., 1915; also July, 1916.

[6] Thomson, J. J., *Rays of Positive Electricity*, Monographs on Physics, Longmans, Green & Co., 1913.

[7] Bohr, N., *Meddelanden fran k. Vetenskapsakademiens Nobel Institute*, **5**, 1919, No. 28.

SHOCK OR WATER RAM IN PIPE LINES WITH IMPERFECT REFLECTION AT THE DISCHARGE END AND INCLUDING THE EFFECTS OF FRICTION AND NON-UNIFORM CHANGE OF VALVE OPENING

By W. F. Durand

Leland Stanford Jr. University

Read before the Academy, April 27, 1920

The classic treatment of the problem of shock in pipe conduits, as developed by Joukovsky, Allievi and others, assimilates in effect the condition of the water in the pipe line during the manifestation of the phenomena in question to that of a column of air in a closed organ pipe in longitudinal vibration, the reservoir end of the line corresponding to the mouth end of the pipe and the valve end of the line to the closed end of the pipe. On this basis the theory has been developed in some detail, especially by Allievi.

In the treatment thus developed and in subsequent study of the problem generally, it has been common to omit the following factors, the existence of which must affect the result in actual cases:

(1) The influence of the velocity head $v^2/2g$.

(2) The influence of friction.

(3) The loss of energy through the discharge valve considered as a nozzle.

(4) The influence due to a time rate of valve area closure irregular, or other than uniform.

(5) The influence due to the fact that the valve end of the line is not closed completely so long as the valve is partly open (as in the *operation* of opening or closing). The analogy with the air in a closed organ pipe is, therefore, imperfect and in particular the reflection at this end must, under these conditions, be incomplete rather than complete, as commonly assumed.

The paper discusses the effect of the neglect of these conditions and a more general treatment, as an extension of the original treatment by Joukovsky, is then developed for taking care of conditions (1), (2), (3), (4).

Based on this treatment a further extension is then developed, including the influence due to imperfect reflection at the valve end.

Four different bases for the specification of the amount of reflection realized are presented and the treatment is developed to include any of these according to choice.

These four methods are as follows:

(1) A constant fraction or percentage of complete reflection.

(2) A fraction or percentage of complete reflection defined as $(a_1 - a)/a_1$ where a_1 = area of full valve opening and a = area of opening at any instant during valve movement.

(3) A fraction or percentage of complete reflection defined as $(A - a)/A$ where A = cross-section area of pipe and a = area of valve opening at any instant during valve movement.

(4) A fraction or percentage of complete reflection defined as $(v_1 - v)/v_1$ where for closure v_1 = full initial velocity and for opening v_1 = full final velocity, while v = velocity at any instant during valve movement.

The treatment throughout the paper includes both closure and opening of the valve, either in whole or in part, or in general any amount of valve movement either in closure or opening.

The paper is illustrated with diagrams showing typical numerical cases.

Of the various conditions thus brought within the scope of a form of analytical treatment, the author considers that part of the paper relating to No. 5 of more especial interest, representing, so far as he is aware, a distinct extension of earlier treatments of the problem and in such manner as to bring under some form of analytical control an important condition which must always prevail in actual cases of the development of shock or water ram in liquid conduits.

REPORT OF THE ANNUAL MEETING

PREPARED BY THE HOME SECRETARY

The annual meeting of the Academy was held in Washington, April 26, 27 and 28, 1920, at the Smithsonian Institution. The following eighty-eight members were present: C. G. ABBOT, BAILEY, A. GRAHAM BELL, BOAS, BOGERT, BRIDGMAN, BUMSTEAD, W. W. CAMPBELL, CARTY, CASTLE, CATTELL, F. W. CLARKE, J. M. CLARKE, CONKLIN, CREW, CROSS, CURTIS, DALL, DAVENPORT, DAVIS, DAY, DICKSON, DONALDSON, DUNN, DURAND, FEWKES, FORBES, FROST, GOMBERG, HALE, E. H. HALL, G. S. HALL, HALSTED, HARPER, HARRISON, HENDERSON, HILLEBRAND, HOLMES, HOWARD, HOWE, HOWELL, IDDINGS, JENNINGS, JEWETT, KAS, ner, Langmuir, Leuschner, Lewis, Lindgren, Lusk, Lyman, Meltzer- Mendel, C. E. Mendenhall, C. H. Merriam, J. C. Merriam, Merritt, Michelson, Millikan, E. F. Nichols, A. A. Noyes, W. A. Noyes, Parker, Pearl, Ransome, Reid, Remsen, Rosa, Russell, Schlesinger, Setchell, Erwin F. Smith, Squier, Stieglitz, Stratton, Thomson, Thorndike, Trowbridge, Veblen, Walcott, Webster, Welch, Wheeler, D. White, H. S. White, Edwin B. Wilson, R. W. Wood, Woodward.

BUSINESS SESSIONS

The President announced the deaths of two members since the Autumn Meeting: Louis V. Pirsson and Horatio C. Wood. The preparation of the biography of Louis V. Pirsson was assigned to Joseph P. Iddings.

The resignation of E. W. Morley as Chairman of the J. Lawrence Smith Fund was presented by the President, and accepted.

Sections and Committees.—The election of J. J. Carty to succeed Gano Dunn as Chairman of the Engineering Section, term expiring in 1923, and of H. A. Bumstead to succeed E. L. Nichols as Chairman of the Section of Physics, term expiring in 1923, were announced.

The President announced the following committee appointments:

COMMITTEE ON	APPOINTED	TO SUCCEED	TERM EXPIRING
Henry Draper Fund	H. N. Russell	Joseph S. Ames	1925
J. Lawrence Smith Fund	W. Cross, *Chairman*	E. W. Morley	1924
	Walter S. Adams	E. W. Morley	1924
	Frank Schlesinger	A. O. Leuschner	1925
Comstock Fund	John J. Carty	Elihu Thomson	1925
Marsh Fund	H. A. Brumstead	Edward S. Dana	1923
Murray Fund	Alfred G. Mayor	George H. Parker	1923
Marcellus Hartley Fund	David W. Taylor	Arthur G. Webster	1923
	William H. Welch	Theobald Smith	1923
Mary Clark Thompson	Waldemar Lindgren	Gano Dunn	1923

Reports.—The Annual Report of the President to Congress for 1919 was presented and distributed.

The report of the Treasurer was presented in printed form and approved.

The Home Secretary presented the following report:

The President of the National Academy of Sciences.

Sir:

I have the honor to present the following report on the publications and membership of the National Academy of Sciences for the year ending April 26, 1920.

Two parts of Volume 14 of the Memoirs of the National Academy of Sciences have been completed and distributed: the second memoir, "Complete Classification of Triad Systems," by H. S. White, F. N. Cole and L. D. Cummings, and the fourth memoir, "Minor Constituents of Meteorites," by G. P. Merrill.

The third memoir, "Tables of Minor Planets," by A. O. Leuschner, A. E. Glancy, and S. H. Levy, and the fifth and final memoir of Volume 14, "Tables of the Esponential Function," by C. E. Van Orstrand, are now in page proof and will be issued shortly, as will also Volume 15, "Psychological Examining in the United States Army," by Robert M. Yerkes.

Volume 16, first memoir, "Lower California and its Natural Resources," by E. W. Nelson, and the second memoir, "Studies upon the Life Cycles of Bacteria," by F. Löhnis, are now in galley proof. The third memoir, "A Recalculation of Atomic Weights," by F. W. Clarke, is now in the hands of the printer.

Volume VIII of the Biographical Memoirs has been completed with the publication of the biographies of Benjamin Osgood Peirce, and Cleveland Abbe, and the bound volume distributed. The following biographies forming a part of Volume IX have been completed and distributed: William Bullock Clark by John M. Clarke; Arnold Hague by Joseph P. Iddings; Eugene Waldemar Hilgard by Frederic Slate; James Dwight Dana by L. V. Pirsson; James Mason Crafts by Charles R. Cross; Lewis Boss by Benjamin Boss; and Alpheus Spring Packard by T. D. A. Cockrell. That of Charles Sedgwick Minot is now in page proof.

The Report of the National Academy of Sciences has been issued and the fourth Annual Report of the National Research Council will be issued in separate form in a few days. The Proceedings have reached the third number of the sixth volume.

Since the last meeting, two members have died. Louis V. Pirsson, elected 1913, died on December 8, 1919, and Horatio C. Wood, elected in 1879, died in 1919. This leaves an active membership of 175 members, 1 honorary member, and 31 foreign associates. Two foreign associates are reported dead; Gustav Retzius, elected in 1909, and Ivan Petrovich Pavlov, elected in 1908. The former died July 21, 1919.

(Signed) C. G. ABBOT, *Home Secretary.*

Reports from Trust Fund Committees.—The following telegraphic report from A. A. Noyes, Chairman of the Committee on the Barnard Medal, was read: Committee recommends awarding Barnard Medal to ALBERT EINSTEIN for highly original and fruitful development of the fundamental concepts of physics through application of mathematics.—A. A. NOYES.

The report of the Committee on the MURRAY FUND, singed by the Chairman, WM. H. DALL, was presented. The Committee recommended the award of the Agassiz medal to ADMIRAL C. D. SIGSBEE, U. S. N., retired, for his important contributions to oceanography, both by actual research, by publication of his results, and invention of new methods.

The Directors of the WOLCOTT GIBBS FUND reported the unexpended income of the fund as $965.56, of which $500.00 is at present invested in Liberty bonds.

The Trustees of the WATSON FUND recommended the following grants:

No. 20.—An appropriation of $500 to JOHN A. MILLER, Sproul Observatory, Swarthmore College, for the employment of assistance in measuring and reducing plates for the determination of parallaxes. This is a renewal of No. 17.

No. 21.—A further appropriation of $250 to J. A. PARKHURST for the employment of assistance in measuring and reducing plates for photographic and photovisual magnitudes in the north polar sequence and in the parallax field on which the Yerkes Observatory is working.

The cash balance of the Watson Fund on March 31, 1920, was $1,663.52, not including $1,000 accrued income invested in bonds.

The following report from H. F. OSBORN, Chairman of the ELLIOT FUND, was read:

The Committee is requesting recommendations for the award for the year 1919, the award to be made for the best zoölogical work of the year.

The Treasurer reports that on March 31, 1920, there was a cash balance of $564.81 in this fund, and that on the same date the sum of $300 of income was invested in Liberty Bonds.

The only appropriation from the GOULD FUND for the current year has been $600 to BENJAMIN BOSS for the purpose of meeting part of the expenses of the *Astronomical Journal*. The cash balance of this fund is $840.01; the invested income, $6,250.

The Committee on the HENRY DRAPER FUND report was presented. The Committee recommended the award of the DRAPER GOLD MEDAL to ALFRED FOWLER, F.R.S., Professor of Astrophysics, Imperial College, South Kensington, London, for contributions to our knowledge of sunspots, comets and the stars—especially red stars of Secchi's type III—on the basis of spectroscopic observations, and his interpretations of celestial phenomena.

The following grants were recommended:

$400 to S. A. MITCHELL, of the University of Virginia, to complete the purchase of a measuring microscope for use in the photographic determination of stellar parallaxes, on the basis of observations made with the 27-inch refracting telescope. The Academy awarded the sum of $250 from the Draper Fund to apply on the purchase of this instrument and the proposed grant of $400 will complete the purchase. The microscope, costing $650, becomes in effect the property of the Academy. Professor Mitchell will devote an equivalent sum, $400, to other needs of his parallax research.

$300 to JOEL STEBBINS, Professor of Astronomy in the University of Illinois, to assist in the further development of the photo-electric-cell photometer.

$400 to FRANK SCHLESINGER, Director of the Allegheny Observatory to enable him to test an automatic zenith camera for the determination of terrestrial latitudes, with the expectation that the results will be more accurate than any hitherto obtained by other means. It is proposed that this instrument be mounted temporarily at the International Latitude Observatory at Ukiah, California, where the astronomer in charge will operate it for a year or two as a labor of love. The grant is needed to install the instrument at Ukiah and to make certain auxiliary apparatus required in its operation. The Allegheny Observatory is loaning the objective and the photographic plates obtained will be measured by Dr. Schlesinger himself or under his immediate direction.

$175 to E. B. FROST, Director of Yerkes Observatory, for the purchase of a Hess-Ives tint photometer for use in the Yerkes Observatory, to supplement the Hartmann micrometer in the measurement of various illuminants, of the transmission of filters for various wave-lengths, of the absorption of photometric gratings, and of other phenomena and subjects.

$500 to Dr. ANTONIO ABETTI, Director of the Arcetri Observatory, Florence, Italy, to apply on the cost of a combined spectrograph and spectro-heliograph for use in combination with a 60-foot tower telescope now under construction. It is planned that this instrument shall be used by the son of the Director, Dr. Giorgio Abetti, well known to many American astronomers, recently transferred from the Observatory in Rome to the Arcetri Observatory.

$200 to MAJOR WILLIAM BOWIE, Chief of the Division of Geodesy, U. S. Coast and Geodetic Survey, in temporary support of the International Latitude Observatory at Ukiah, California, to assist in meeting an emergency due to the failure of the Observatory's regular source of funds.

The cash balance of the Draper Fund on March 31, 1920, was $1,065.04. Additional income to the amount of $917.50 is invested in bonds.

The following communications, presented by the Foreign Secretary, were referred to committees for further report:

1. A communication from the Académie des Sciences, Paris, signed by the President and the Perpetual Secretaries, requesting that the National Academy of Sciences use its influence to secure the adoption of the metric system of weights and measures in America.
2. A communication from Vienna, signed by the Actuary and other employees of the Akademie der Wissenschaften, requesting aid for themselves and their families in the way of food drafts, to relieve the situation now existent in Vienna because of the shortage of food.
3. A communication from the Oesterreichische Gesellschaft für Meteorologie, requesting funds for the support of its official publication, the "Zeitschrift für Meteorologie," and for the continuance of the work of its meteorological observatory. The total amount needed is 10,000 francs, or about $1,930.00.

Elections.—ARTHUR L. DAY and T. H. MORGAN were elected members of the COUNCIL to succeed E. G. Conklin and A. A. Noyes.

The following were elected to membership in the Academy:

JAMES ROWLAND ANGELL, Chairman of the National Research Council.
HENRY PRENTISS ARMSBY, State College, Pennsylvania.
WILDER DWIGHT BANCROFT, Cornell University.
HANS FREDERIK BLICHFELDT, Leland Stanford Jr. University.
ANTON JULIUS CARLSON, University of Chicago.
WILLIAM DUANE, Harvard University.
LEWIS RALPH JONES, University of Wisconsin.
ELMER PETER KOHLER, Harvard University.
CHARLES KENNETH LEITH, University of Wisconsin.
CLARENCE ERWIN MCCLUNG, University of Pennsylvania.
ELMER VERNER MCCOLLUM, Johns Hopkins University.
HARRIS JOSEPH RYAN, Leland Stanford Jr. University.
JOEL STEBBINS, University of Illinois.
BAILEY WILLIS, Leland Stanford Jr. University.

The following foreign associates were elected:

FRANK DAWSON ADAMS, President of McGill University.
MARIE ENNEMOND CAMILLE JORDAN, College of France.
FRANÇOIS ANTOINE ALFRED LACROIX, Musée d'Histoire Naturelle, Paris.
HEIKE KAMERLINGH Onnes, University, of Leyden.
SIR DAVID PRAIN, Royal Botanic Gardens, Kew, Surrey.
SANTIAGO RAMON Y CAJAL, University of Madrid.

Previous to adjournment, the following motion was adopted: That the Home Secretary be requested to transmit the thanks of the Academy to the Smithsonian Institution and to the Cosmos Club for courtesies extended to the members of the National Academy of Sciences during the meeting.

SCIENTIFIC SESSIONS

A public lecture-discussion in the series of William ELLERY HALE LECTURES was delivered on *The Scale of the Universe,* on April 26 at 8.15 P.M., by HARLOW SHAPLEY, of the Mount Wilson Observatory and by HEBER D. CURTIS, of the Lick Observatory

Four public scientific sessions were held on Monday and Tuesday, April 26–27, at which the following papers were presented:

JOHN M. CLARKE: Conservation of natural resources as a proper function of the National Academy.

RAYMOND PEARL: On the rate of growth of the population of the United States since 1790 and its mathematical expression.

FRANZ BOAS: Growth and development as determined by environmental influences.

CHARLES B. DAVENPORT: Plural births in man.

SAMUEL J. MELTZER: The importance of the presence of both sympathetic superior cervical ganglia to the maintenance of life; and their possible relations to respiratory diseases.

CHARLES D. WALCOTT: Structure of Marrella and allied Middle Cambrian crustaceans.

JAMES R. ANGELL: The National Research Council.

ROBERT M. YERKES (introduced by E. L. Thorndike): A psychological study of the medical officers in the Army.

ROBERT W. WOOD: Spectroscopic phenomena of very long vacuum tubes.

L. T. E. THOMPSON, C. N. HICKMAN and N. RIFFOLT (introduced by Arthur G. Webster): The measurement of small time intervals and some applications, principally ballistic.

ROBERT A. MILLIKAN: The effect of molecular structure upon the reflection of molecules from the surface of liquids and solids.

ARTHUR G. WEBSTER: The Springfield rifle and the Leduc formula.

ARTHUR G. WEBSTER: Some new methods in internal ballistics of the Springfield rifle.

GEORGE E. HALE: The 100-inch Hooker telescope of the Mt. Wilson Observatory.

A. A. MICHELSON: (1) The vertical interferometer. (2) Preliminary tests in an attempt to measure the diameter of the stars. (3) A modification of the Foucault method adapted to long-distance measurement of the velocity of light.

ARTHUR G. WEBSTER: Preliminary measurements on the pressures in the "Onde de Choc."

ARTHUR G. WEBSTER: On the connection of the specific heats with the equation of state of a gas.

EDWIN H. HALL: Thermal conductivity of metals.

C. HART MERRIAM: Distribution and villages of the Indian tribes of the Klamath river region, California.

JOHN C. MERRIAM: Significance of correlation in function between the dentition and skeleton of the Sabre-tooth tiger.

GEORGE H. PARKER: On the colonial nervous system of Renilla.

DOUGLAS H. CAMPBELL: The genus Botrychium and its relationships.

FREDERICK V. COVILLE: The influence of cold in stimulating the growth of plants.

Thomas B. Osborne and Lafayette B. Mendel: Some common foods as sources of vitamines.

Lawrence J. Henderson: The physico-chemical properties of haemoglobin.

William A. Noyes and George H. Coleman: The direct combination of nitrogen and chlorine.

Gilbert N. Lewis: Valence and chemical affinity.

William F. Durand: Shock or water ram in pipe lines with imperfect reflection at the discharge end and including the effects of friction and non-uniform change of valve opening.

Leonard E. Dickson: Recent notable progress in the theory of numbers.

Edward Kasner: Geodesics and relativity.

F. E. Pernot (introduced by G. O. Squier): The use of alternating currrent for submarine cable transmission.

George O. Squier: Multiplex telephony and telegraphy over open-circuit bare wires laid in the earth or sea.

Lyman J. Briggs: The air resistance of spheres.

Robert H. Goddard: The possibilities of the rocket in weather forecasting.

H. Fielding Reid: The distribution of land and water on the earth.

Waldemar Lindgren: The alterations of limestones in contact-metamorphism.

Henry Norris Russell: Notes on the internal constitution of the stars.

Charles Greeley Abbot: New observations on the variability of the sun.

Raymond Pearl: On a single numerical index of the age distribution of a population (by title).

Edward S. Dana: Biographical memoir of George Jarvis Brush (by title).

Mabel Weil: Reports on the researches of the late Professor C. C. Trowbridge. (*a*) Auroras and magnetic disturbances. (*b*) Auroral phenomena and the meteor train zone. (*c*) An investigation of meteor trains. (*d*) Theories on the nature of meteor trains (by title).

Theodore Holm: The vegetation of the Alpine region of the Rocky Mountains in Colorado (by title).

S. W. Williston: Osteology of the reptiles (by title).

Irving Langmuir: The "quantel" theory: a new theory of the ether, matter and electro-magnetism.

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CONTENTS

Page

PHYSIOLOGY.—THE DYNAMICS OF A FLUCTUATING GROWTH RATE.............. *By H. S. Reed* 397

BIOLOGY.—ANALYTICAL NOTE ON CERTAIN RHYTHMIC RELATIONS IN ORGANIC SYSTEMS..*By Alfred J. Lotka* 410

MATHEMATICS.—ON THE CLASS NUMBER OF THE FIELD $\Omega(e^{2i\pi}/p^n)$ AND THE SECOND CASE OF FERMAT'S LAST THEOREM............*By H. S. Vandiver* 416

GENETICS.—OBSERVATIONS ON THE STERILITY OF MUTANT HYBRIDS IN DROSOPHILA VIRILIS ....*By C. W. Metz* 421

ZOOLOGY.—STUDIES ON THE TRANSFORMATION OF THE INTESTINAL FLORA, WITH SPECIAL REFERENCE TO THE IMPLANTATION OF BACILLUS ACIDOPHILUS I. FEEDING EXPERIMENTS WITH ALBINO RATS........................ *By Harry A. Cheplin and Leo F. Rettger* 423

STATISTICS.—ON A SINGLE NUMERICAL INDEX OF THE AGE DISTRIBUTION OF A POPULATION.......................................*By Raymond Pearl* 427

ZOOLOGY.—UPON AN IMPORTANT METHOD OF STUDYING PROBLEMS OF RELATIONSHIP AND OF GEOGRAPHICAL DISTRIBUTION....*By Maynard M. Metcalf* 432

BOTANY.—THE INFLUENCE OF COLD IN STIMULATING THE GROWTH OF PLANTS.. *By Frederick V. Coville* 434

PHYSICS.—ON THE NATURE OF THE NEGATIVE CARRIERS PRODUCED IN PURE HYDROGEN AND NITROGEN BY PHOTO-ELECTRONS..*By Leonard B. Loeb* 435

ENGINEERING.—SHOCK OR WATER RAM IN PIPE LINES WITH IMPERFECT REFLECTION AT THE DISCHARGE END AND INCLUDING THE EFFECTS OF FRICTION AND NON-UNIFORM CHANGE OF VALVE OPENING......*By W. F. Durand* 441

REPORT OF THE ANNUAL MEETING.. 442

VOLUME 6 AUGUST, 1920 NUMBER 8

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PROCEEDINGS
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Volume 6 AUGUST 15, 1920 Number 8

ON CHONDRULES AND CHONDRITIC STRUCTURE IN METEORITES[1]

By George P. Merrill

U. S. National Museum, Washington, D. C.

Communicated by J. M. Clarke, May 12, 1920

The term "chondrit," from the Greek χονδρος, a grain, was first used, so far as I am aware, by Gustav Rose[2] to designate a class of stony meteorites characterized by the occurrence of small granules or "*kugeln.*" "Sie ist durch kleine Kugeln ausgezeichnet die aus einem noch nicht bestimmten Magnesia-Silicate bestehen, und in einem fein kornigen Gemenge eingemengt sind," etc. The word, with the addition of the terminal *e*, as *Chondrite*, has been very generally adopted, with its original meaning, by English and American writers. Unfortunately, as it would seem, a further modification of the word as *chondros*, *chondrule*, *chondrus*, or *chondrum* has been introduced, at first apparently synonymous in meaning with *kugel* as used by Rose though it is to be noted that he did not define the word quite as clearly as might be desired. He wrote: "* * * in Bruche erscheinen sie *theils uneben*, *theils fasrig*, im letzern Fall jedoch stets nur sehr feinfaserig, indessen doch immer bestimmt erkennbar fasrig, besonders unter der Lupe * * nie radial, sondern immer excentrisch fasrig." No further reference is made to those of "*uneben* Bruche" and one is left only to surmise that they may have been of a granular or porphyritic rather than fibrous structure. The fact that Rose's work was written before the day of thin sections doubtless accounts for the undetermined character of the magnesian silicate.

Tschermak in his *Mikroskopische Beschaffenheit* (1885) was little more explicit in his use of terms than was Rose. He wrote: "Kugelchen und *überhaupt rundliche Körper*, welche bald aus einem einzigen krystallindividuum, bald aus mehreren bestehen, öfters auch aus verschiedenen Gemengtheilen zusammengestzt sind, bilden das Gestein fast allein (Borkut) oder sie lagern unverletzt, öfters auch zersplittert in einer lockeren bis festen Tuffmasse." Elsewhere he includes all the rounded forms under the term *chondren,* though in his plate legends and descriptions he designates both as *Kugelchen,* thus using the two terms synonymously.

A perusal of the literature shows that by English and American writers, the terms *chondrule, chondrus, chondrum* or *chondros* are now and have for some years been applied to the rounded and oval granules presenting a considerable range in mineral composition and still wider range in internal structure, thus making the terms synonymous with kugel or kugelchen as used by Tschermak above. Of later years and as illustrated in the generally adopted scheme of classification,[3] there has seemed a disposition to use the term kugel in a descriptive adjective sense, as *kugelchen chondrit,* under which name are included stones containing chondrules (or chondri) having a radiate structure—the *spherulitic*[4] chondrites of American writers. There has thus apparently arisen in the minds of many a confusion which, as it seems to the writer, has been in part at least responsible for the diverse views expressed concerning the origin of these peculiar bodies. In other words, there has been a failure to recognize or discriminate between the *kugelchen* with radiate structure and the often irregular polysomatic forms with the "uneben Bruche."

The present paper, then, represents an attempt on the part of the author to make this discrimination and to show how far proposed theories may apply to the various forms presented.

NATURE OF THE CHONDRULE

At the outset and for the purpose of making clear what is to follow, it will be well to figure and describe a few characteristic forms of the individual chondrules. This notwithstanding the previous most excellent and comprehensive work of Tschermak and Cohen.[5]

Mineralogically, the chondrules, using the word in its broadest and most comprehensive sense, in nearly all meteorites are composed chiefly of the minerals olivine or pyroxene, the latter in either orthorhombic or monoclinic forms, or both. Some are largely of an undifferentiated glass. Feldspars occur but rarely except in the form known as maskelynite. In addition are occasional enclosures of metal or metallic sulphides, chromite or other minor constituents. The metallic iron sometimes occurs in rounded chondrite-like blebs, though it is doubtful if this should be referred to under that name. Structurally, the chondrules in the same meteorite may vary from densely cryptocrystalline, almost amorphous, to those that are part glassy and porphyritic or even holocrystalline.

I. *Glassy, Cryptocrystalline and Radiated Forms.*—In figures 1 and 2, are shown examples of cryptocrystalline forms from the stones of Barratta, Australia, and Cullison, Kansas. That of figure 1 is of a peculiar brownish translucency and very dense, resembling the "felsitic" structure of the early petrologist. In the Cullison stone, figure 2, the chondrules, also of a brownish color, are not completely isotropic but between crossed nicols break up into several illy defined areas over which the dark cloud sweeps faintly and irregularly as the stage revolves. The material seems to be a partially devitrified glass in a condition of optical stress as from sudden cooling. Chondrules of this type and those next to be described more nearly resemble the spherulites of the terrestrial rocks than any others which have come under my notice. Their outlines are at times as sharply demarked from the matrix in which they are embedded as are the spherulites in the rhyolitic obsidians of the Yellowstone National Park.

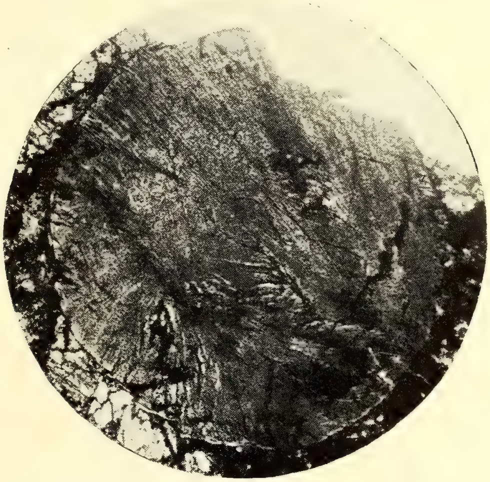


FIG. 1

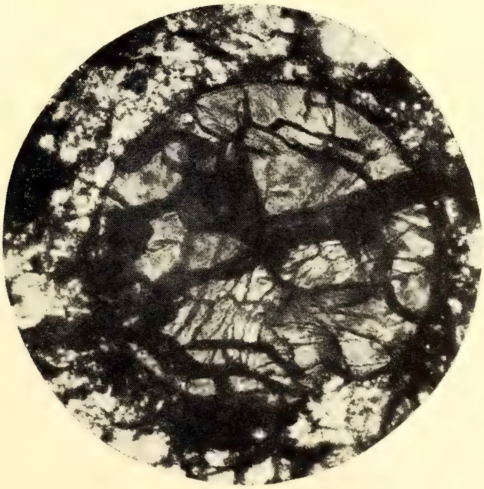


FIG. 2

Chondrules of the radiating type are shown in figures 3, 4 and 5, from the meteorites of Elm Creek, Hessle, and Parnallee. The mineral in all cases is enstatite[6] and the outline of the spherule as sharp and clean as though it had been turned on a lathe. In the Elm Creek example crystallization evidently began at one point on the surface of the spherule and extended inward throughout, but the cooling proceeded too rapidly for the production of an optically perfect crystal. In the Hessle stone (fig. 4), there were evidently several initial points of crystallization. Forms like these grade imperceptibly into such as are shown in figure 5, in which the radiating bars have unmistakably the crystallographic properties of enstatite.

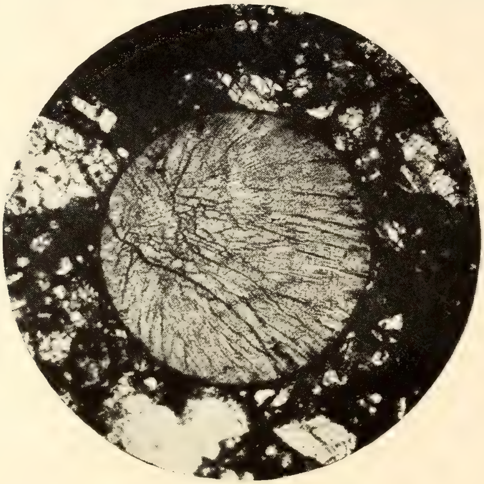


FIG. 3

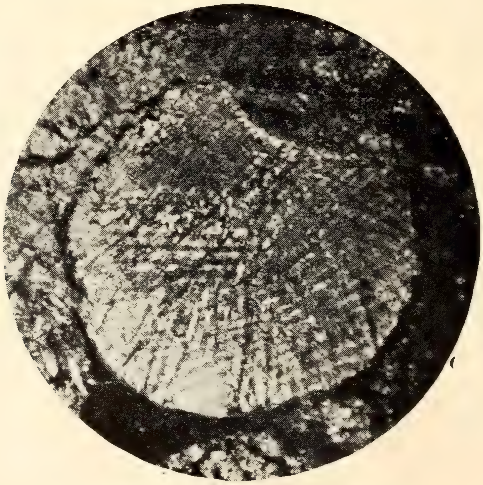


FIG. 4

II. *Half Glassy, Barred and Porphyritic Forms.*—Porphyritic forms are characteristic of both olivine and enstatite chondrules, while the barred forms, such as are shown in figures 6 and 7, are limited mainly, if not wholly, to monosomatic forms composed of olivine. In figure 6, from the Beaver Creek stone, the white portions are olivine which extinguish practically as a single unit; the black portions are glass. It is to be noted that the outlines, though sharp are not smooth as in those described above, but have projecting particles extending out into the ground; also that this border portion often contains enclosures. In figure 7 from the Hendersonville stone, the bars are bent and curved and do not all extinguish simultaneously, as the stage is revolved, the dark cloud sweeping

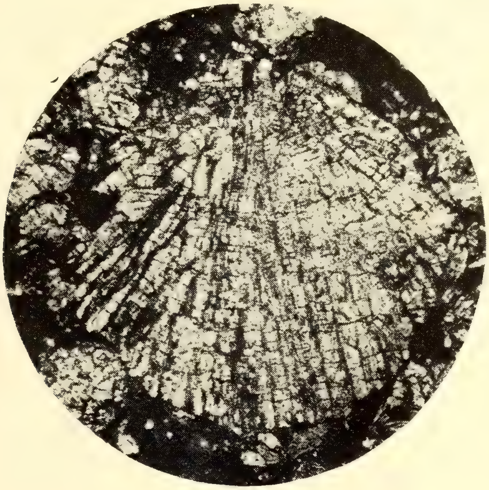


FIG. 5

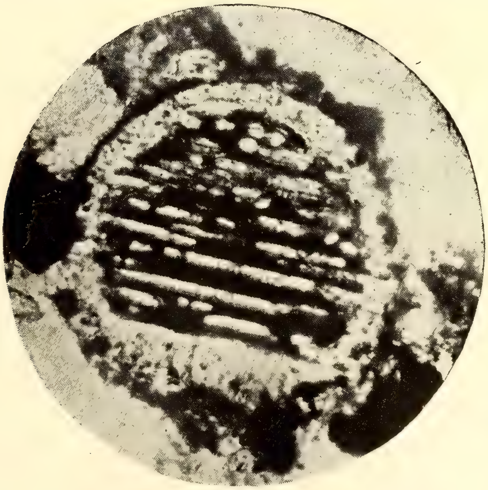


FIG. 6

over it irregularly, indicating a condition of stress. Here, as in the last, the border is not sharply demarked from the ground and it is often impossible to state if a certain crystal particle belongs to one portion or the other. It should be noted that this stone is a crystalline spherulitic chondrite. According to Tschermak, in chondrules of this nature the olivine bars are sometimes interlaminated with plagioclase (e.g., in the Dhurmsala stone). In the porphyritic form shown in figure 8 from the Tennasilm stone, the granular ground abuts sharply against the black glass of the chondrule with only on one side a manifested tendency to penetrate into and beyond the border. It is to be noted that the enstatite

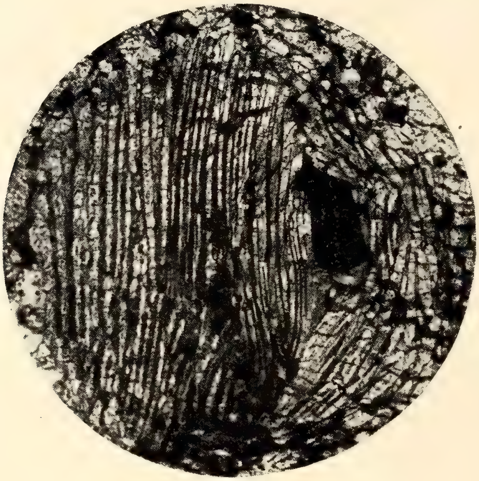


FIG. 7

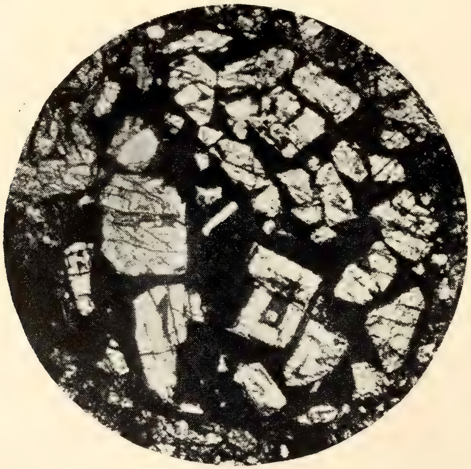


FIG. 8

phenocrysts within the chondrule and near the border are often cut off sharply as though the sphere, originally much larger, had been uniformly reduced by abrasion. This will be referred to later.

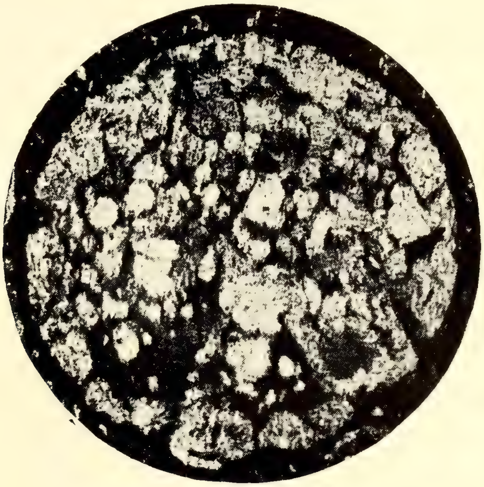


FIG. 9

III. *Holocrystalline Chondrules.*—As would naturally be expected, these porphyritic forms, through a reduction of the proportional amount of glass, pass gradually into those which are almost or quite holocrystalline and polysomatic as shown in figures 9 and 10 from the Barratta and Elm Creek stones respectively. Of peculiar interest are those of the polysynthetically twinned pyroxene (fig. 10). For some unexplained reason, these rarely grade into the half glassy porphyritic forms, the entire chondrule consist-

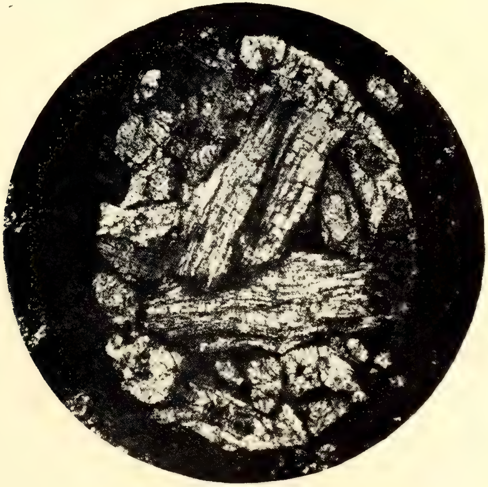


FIG. 10

ing of the closely crowded pyroxenes with comparatively little, if any, interstitial glass. In figure 11, from the Parnallee stone, it will be noted the crystals are in some instances slightly curved, their vertical axes lying approximately parallel with the circumference of the circle which forms the border of the section. The appearance is as if the chondrules had been molded by external forces after the crystals had formed but while yet in a more or less plastic condition. Again, the pyroxene crystals abut sharply against the border and are cut off at the margin as in the half glassy, porphyritic forms mentioned above, and as shown in figures 10 and 11. Occasional forms are met with which have all the appearance of fragments, slightly rounded, of holocrystalline granular rocks, which as noted later, I believe them to be.

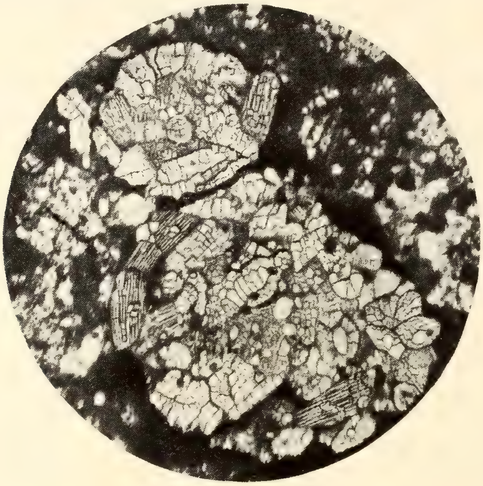


FIG. 11

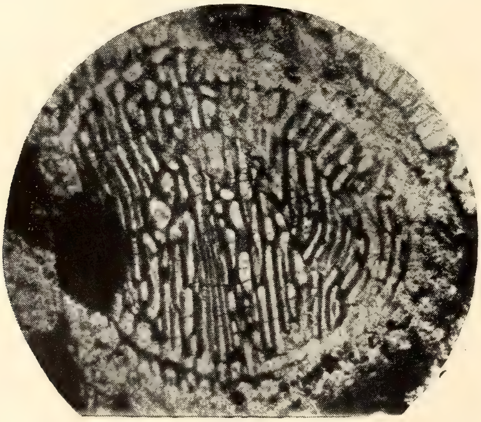


FIG. 12

IV. *Secondary Borders about Chondrules.*—A not uncommon feature of the chondrule is the narrow border or rind about the circumference. These borders as a rule, are of lighter color than the interior, of a clear, more pellucid nature, though it may be including portions of the minerals characteristic of the matrix in which they are embedded. This is well shown in the olivine chondrule, figure 12. This border has an appearance at once suggestive of the secondary intergrowth or enlargement often seen in feldspars and other minerals of terrestrial rocks. The later portions sometimes, though not always, have the same optical orientation as the interior. I am not certain if this border is of a like nature to that described by Tschermak about some of the chondrules of the Grosnaja stone and which he considered of secondary origin. In some instances the chondrules are surrounded by an irregular border of metal or metallic sulphide.

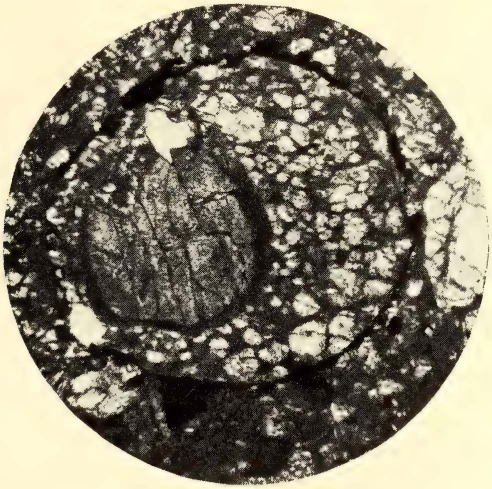


FIG. 13

V. *Double or Compound Chondrules.*—Occasional forms are met with in which a large crystal of olivine or pyroxene is inclosed by a border of finer crystals of the same mineral but suggestive of a later generation. More common are forms such as are shown in figure 13. The crystals are of enstatite but the larger one is filled with minute cavities which are lacking in the smaller forms. Of greater interest is the occasional occurrence of a chondrule within the mass of a second or larger form, as figured by Tschermak, on plate 8, figure 1, of his *Beschaffenheit.* I will refer to this also, later.

The descriptions thus far given are of the most perfectly formed and outlined chondrules only. The broken, angular and, in some cases, distorted radiate enstatite forms are plainly mechanical derivatives, and while they have a bearing upon the origin of the masses in which they occur, they can, except as noted below, have little bearing upon the subject of the origin of the chondrules themselves. They are, therefore, passed over for the present.

THE ORIGIN OF THE CHONDRULE

I. *Theories of Origin.*—In this review it will perhaps not be necessary to go back much beyond the time of the introduction of the microscope and thin sections into the study of rock structures since obviously little that was accurate could be told of them by the naked eye alone.

A brief glance at the literature is sufficient to suggest that many of the opinions expressed have been based upon examinations of but a limited number of occurrences which quite failed to yield the information necessary for building satisfactory hypotheses or conclusions.

Reichenbach, as early as 1860[7] wrote, "Aus allerdem wird es klar, das die Einschlusse in den Meteoriten, als die Trümmer und die geschicbartigen Knollen und Kugeln darin, keine einfach nachen Bestantheile, sondern nichte anderes sind als auch wie die Meteoriten. Meteoriten nur von anderer Anordung ein und derselben naturn Bestandtheile" And again, "Es sind also die Einschlusse theils kleine meteoriten, theils Trümmer von meteoriten von hohenen Alter als diejenigen meteoriten es sind, in welche sie eingeschlossen vorkommen; es sind altere kleinere meteoriten in jungerern grossern meteoriten." In brief, and in plain English, he believed each particle as now found to represent a minute but independent meteorite derived from the breaking up of some older preëxisting stone and now included as a constituent part of one new formed.

In discussing the microscopic structure of meteoric stones, H. C. Sorby, in 1864, wrote,[8] "It would, therefore, appear that, after the material of the meteorites was melted, a considerable portion was broken up into small fragments subsequently collected together, and more or less consolidated by mechanical and chemical action. * * * Apparently this breaking up occurred in some cases when the melted matter had become crystalline, but in others the form of the particles lead me to conclude that it was broken up into detached globules while still melted. This seems to have been the origin of some of the round grains met with in meteorites; for they occasionally still contain a considerable amount of glass, and the crystals which have been found in it are arranged in groups radiating from one or more points on the external surface in such a manner as to indicate that they were developed after the fragments had acquired their present spheroidal shape." In continuation of this same idea in 1877,[9] Sorby wrote: "As is well known, glassy particles are sometimes given off from terrestrial volcanoes, but on entering the atmosphere they are immediately solidified and remain as mere fibres, like *Pele's hair*, or as more or less irregular laminae, like pumice dust. The nearest approach to the globules in meteorites is met with in some artificial products. By directing a strong blast of hot air or steam into melted glassy furnace slag, it is blown into spray, and usually gives rise to pear-shaped globules, each having a long, hair-like tail, which is formed because the surrounding air is too cold to retain the slag in a state of perfect fluidity. Very often the fibres are of the chief product. I have never observed any such fibres in meteorites. The formation of such alone could not apparently occur unless the spray were blown into an atmosphere heated up to near the point of fusion, so that the glass might remain fluid until collected into globules. The retention of a true vitreous condition in such fused stony material would depend on both the chemical composition and the rate of cooling, and its permanent retention would in any case be impossible if the original glassy globule were afterwards kept for a long time at a temperature somewhat under that of fusion. The combination of all these conditions may very well be looked upon as unusual, and we may thus explain why grains containing the glass are comparatively very rare; but though rare they point out what was the origin of many others. In by far the greater number of cases the general basis has been completely divitrified, and the larger crystals are surrounded by a fine-grained stony mass. Other grains occur with a fan-shaped arrangement of crystalline needles, which an uncautious, non-microscopical observer might confound with simple concretions. They have, however, a structure entirely different from any concretions met with in terrestrial rocks, as for example that of oolitic grains. In them we often see a well-marked nucleus, on which radiating crystals have been deposited equally on all sides, and the external form is manifestly due to the growth of these crystals. On the contrary the grains in meteorites now under consideration have an external form *independent of the crystals* which do not radiate from the centre, but from one or more places on the surface. They have, indeed, a structure absolutely identical with that of some artificial blowpipe beads which become crystalline on cooling. With a little care these can be made to crystallize from one point, and then the crystals shoot out from that point in a fan-shaped bundle, until the whole bead is altered. In this case we clearly see that the form of the bead was due to fusion, and existed prior to the formation of the crystals. The general structure of both of these and the previously described spherical grains also show that their rounded shape was not due to mechanical wearing. Moreover, melted globules with well-defined outline could not be formed in a mass of rock pressing against them on all sides, and I, therefore, argue that some at least of the constituent particles of meteorites were originally detached glassy globules, like drops of fiery rain." In this Sorby would appear to have had reference only to *kugels* with radiate, internal structure.

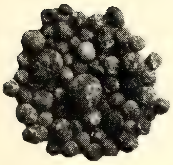
Tschermak, who together with Haidinger, was one of the first to pronounce on the tuff-like character of the chondritic meteorites, announced in 1874[10] the opinion that the individual chondrules (kugelchen) were but rock particles which became simply rounded under conditions similar to such as might exist in the throat of a terrestrial volcano. "Ich widerhole hier nur das Eine, dass Ich die Chondrite fur Zerreibungs-Tufe, und die Kugelchen derselben fur solche Gesteinspartikelchen hatte, welche wegen ihrer Zähigkeit bei Zerreiben des Gesteines nicht in Splitter Aufgelöst, sondern, abgerundet wurden." And again in 1875[11], "Man kann sich allenfalls vorstellen das die Steinmassen, welche der Zerreibung ausgesitz weren, ziemlich weich gewesen sein und würde sich dadurch der Vorstellung Daubrees nähern, welcher an ein Gestein denkt welches in einer Gas masse wirbelnd erstarrte; doch is es sicher, das die Kugelchen das Resultat einer Zerreibung sind." In both of these quoted expressions Tschermak seems to have had in mind only the granular and porphyritic polysomatic forms, and the fragmental "kugelchen."

Three years later[12] after a consideration of the depressions and excrescences occurring in and on the chondrules of the Tieschitz meteorite, he came to a partial agreement with Sorby conceiving that "Die Kügelchen sind nach wie vor wegen der tuffartigen Beschaffenheit der Meteorsteine als Resultate volcanischer Eruptionen und Explosionen anzusehen, aber ihre Form dürfte doch eher von einem plastischen Zustande, als von der Zerreibung starrer Partikel abzuleiten sein." And again, after another four years[13] he announced, "Ich hatte * * * zu der Ansicht gefuhrt wurde, dass die Kügelchen der chondrite als erstarrte Tropfen anzusehen sind, während die aus Splittern bestehende Grundmasse nach wie vor als vulkanischer Detritus zu betrachten wären."

Daubree[14] seems also to have held the opinion that the majority of chondrules were simply debris particles rounded by attrition. "J'ai montre," he wrote, "que la structure globulare telle qu'elle se presente dans certains types * * * a ete imitee artificiallement, et s'explique par une sort de granulation, aperee au moment ou la substance se solidifie. Mais le plu souvent, les globules des meteorites parraissent etre des simples debris arrondis par frattiment."

F. Rinne[15] by means of a simple electric device was able to fuse the silicate constituents of meteorites and by abrupt alterations of the strength of the current produce a "spratzen" of the melt resulting in the projection from the crucible of small drops which quickly cooled in the form of "kugels." To some such action he would ascribe the formation of meteoric chondrules. Later, by the aid of an oxygen blast and a Linnemann burner he was able to produce enstatite beads evidently in every way comparable with meteoric chondrules.[16] These it will be observed are really synthetic demonstrations of the possible correctness of Sorby's views. Berwerth in 1901[17] announced his conviction that the chondritic stones were tuffs more or less completely metamorphosed by heat, and seemed to regard the individual chondrules as portions of the melt that cooled in globular form. Borgström[18] in his description of the Hvittis

[MERRILL'S PAPER]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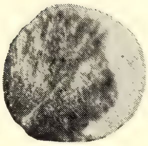
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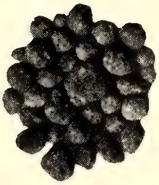
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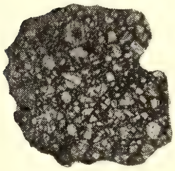
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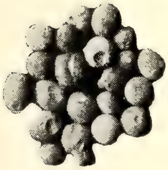
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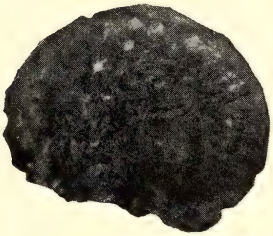
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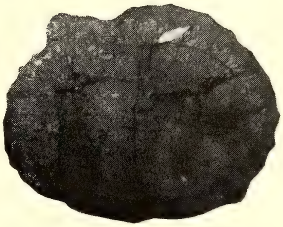
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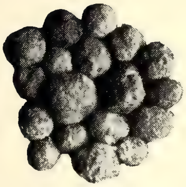
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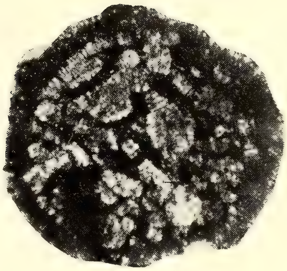
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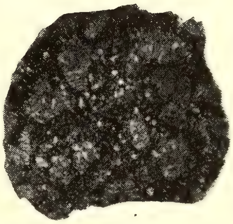
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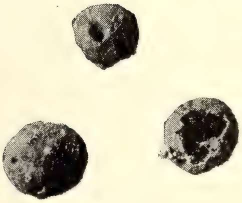
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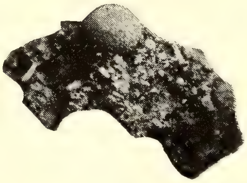
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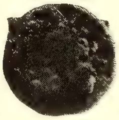
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PLATE 1

Fig. 1.—Cluster of smooth chondrules out of Allegan stone.

Fig. 2.—One of the same chondrules broken and showing radial structure.

Fig. 3.—Cluster of rough chondrules out of Allegan stone.

Fig. 4.—Thin section of one of same.

Fig. 5.—Cluster of smooth chondrules out of Bjurböle stone.

Figs. 6 and 7.—Thin sections of same.

Fig. 8.—Cluster of rough chondrules out of Bjurböle stone.

Figs. 9 and 10.—Thin sections of same.

Figs. 11–13.—Chondrule-like bodies from Bessemer convertor, Sparrow Point, Md., Steel Works.

stone (1903) (a crystalline chondrite), states that the chondrules are always so firmly intergrown with the ground that it is often impossible to determine where the one leaves off and the other begins. In many instances, the enstatites of a chondrule extend out into the ground mass with which they are intergrown. As noted, the Hvittis stone is a crystalline chondrite; this might suggest either a crystallization of the chondrule *in situ* or a case of secondary enlargement. In writing of the Shelburn stone, a grey chondrite, however, he says,[19] "Each individual chondrule represents a structure of cooling and crystallization from a molten state, and as their structure shows an intimate relation to the boundary of the chondrule it must be supposed that each, at the time of its solidification, was a separate unit. Because chondrules of the same chemical composition have a different structure, they must have been formed under different physical conditions. Since such a variety of conditions cannot have existed in the narrow space in which the different structures now are met with, the chondrules must have accumulated after solidification." Such a condition is well shown in figure 14, from the stone of Cedar, Texas. Meunier[20] basing an opinion apparently upon the theoretic work of M. Faye suggests the probability of the chondrules resulting from the sudden condensation of a cyclonic vapor. "Il parait difficile de ne pas admettre que les chondres sont aux roches de precipitation gazeuse ce que les dragees de Carlsbad et le fer en grains sont aux roches de precipitant aqueus * * * Conformement a la terminologie dont font usage les paleontologistes a propos du vent fossiles du soleil fossile, de la pluie fossile, on serait tente de les qualifier de *cyclones photospherique fossiles*." This is conceivable, to the present writer, only in the case of radiate enstatite or monosomatic forms.

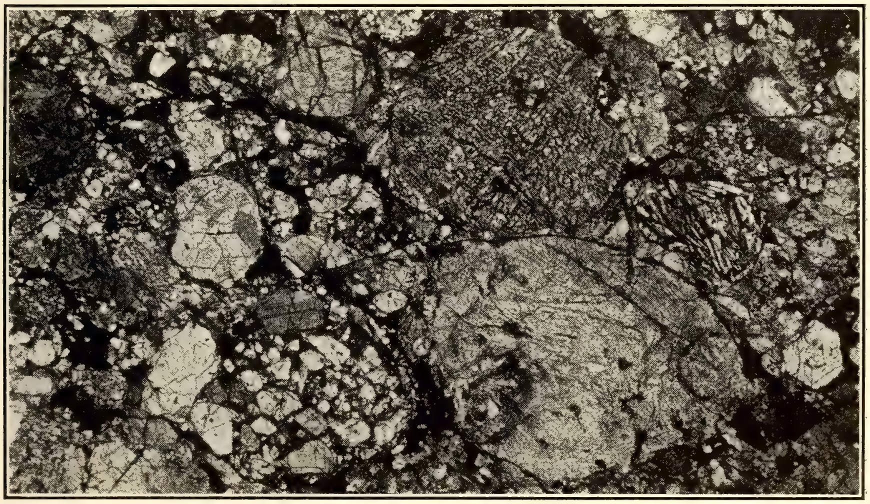

FIG. 14

Brezina, to whom is so largely due the building up of the magnificient collection at Vienna, concludes a review[21] of the subject with the statement, "Durch die vorangefuhrten Beobachtungen können wohl die alteren Ausschauugsweisen als beseigtig betrachteh werden, und wir können wohl mit Bestimmtheit die Meteoriten als gestörge über hastete Krystallbildungen in einem einzigen gemengten magma bezeichen." This I understand includes both the ground mass and its chondrules.

Hussak, basing an opinion on experimental work by himself and Dolter[22] suggests that chondritic meteorites, like that of Uberaba, Brazil (a crystalline chondrite), originate through the long continued immersion of meteoric stones in a nickel-iron-rich magma, and are to be regarded as true volcanic ejectamenta, "Ich möchte demnach die Meteorsteine durch ultrabasische Eruptivgesteine vergleichen und die Bildung der Chondren wie der Trümmerstrutur und der schwarzen Adern als eine magmatische Eirnwirkung vor der Ejektion ansehen. Daher die vollstandigen Ubergange in Siderite und die Deutlichen Korrosionserscheinungen an den grossen Olivinkristallen der Pallasite."

C. Klein, in 1906,[23] evidently basing an opinion largely on figures of chondrules in the works of Hahn and Tschermak, affirmed that there occur many ideally perfect forms that lack the eccentric radiating structure, but are "radial strahlig," from a centre, equally in all directions and are *true spherulites*. Those not having this perfection of structure are considered fragments. It may be well to note before going further, that Klein apparently stands alone in holding these views though they may be correct for certain forms.

Wahl[24] would explain the formation of the chondrule as due to the cooling of a silicate melt in a heated atmosphere, the resultant drop crystallizing from the surface inward. "Die Entstehung der Chondren lässt sich also ganz allemein als durch Zerstäubung von Silikatschmelz fluss innerhalb einer heissen Atmosphäre und Kristallization der hierdurch enstandenen Tropfen von aussen nach innen zu erklaren." This again would seem to refer only to the cryptocrystalline, radiating enstatite, and the barred and monosomatic olivine chondrules.

Finally, in 1913, Fermor,[25] of the Indian Survey, suggested that the chondrules are remelted garnets.

In my preliminary considerations of this subject, I felt that in order to arrive at a solution of the problem it would be necessary to begin with a study of the crystalline chondrites, since in case the chondrules were spherulites, it would be here that they would be found in their primary condition of development. A study of all the thin sections of stones belonging to this group, now in the Museum collections has, however, brought about a change of view since in none of them do I find chondrules developed in the variety and perfection of forms existing in those meteorites which are plainly tuffaceous. This fact and others to be mentioned later have led me to regard the larger part if not all chondritic stones as originally tuffaceous and owing their more or less crystalline condition, where such exists, to heat and pressure in a nonoxidizing or even reducing atmosphere. The study of the origin of the chondritic meteorites must begin then with the study of the chondrules themselves as found in the tuffs and a gradual tracing of them back through the crystalline "kugelchen" forms to the holocrystalline types.

Disregarding the fragmental, angular and more or less splintered radiate forms concerning the fracturing and secondary origin of which there can be no doubt, and confining ourselves to those which so far as can be determined have the form and structure assumed at the time of their formation and are of a more nearly perfect spherical, oval or "kugelchen" shape, their study in thin section brings out very clearly two important fundamental distinctions. 1st. The most perfectly spherical and oval forms, sharply differentiated from the matrix, are those showing a cryptocrystalline or radiating internal structure, and are mineralogically of pyroxene (enstatite or bronzite). These forms often show excrescences or saucer-shaped depressions, as through shrinkage or interference during solidification. 2nd. Those of a polysomatic nature, composed of phenocrysts of olivine or pyroxene in a more or less glassy base, or of an almost holocrystalline aggregate of one or more minerals, are of irregular outline, have more the appearance of fragments and never show the saucer-shaped depressions and excrescences. Further than this, it becomes very quickly evident that a theory of origin which will account for the first, must fail in the case of the second. It is not possible for instance, that a chondrule of phenocrysts in a glass base like that shown in figure 8, should have originated under the same conditions and in the same manner as that in figure 3.

It is possible to conceive of a basic magma containing the necessary constituents for the formation of olivine or enstatite to be in a sufficiently liquid condition to cause, or allow it, when thrown into a hot or thin atmosphere, to take the form of spherical drops which on cooling will become glassy or crystalline, according to conditions. Should the drop begin cooling on one point of its surface, the crystals might radiate from this point outward, or rather inward with reference to the chondrule, giving rise to the radiated form like those of the Elm Creek and Parnallee stones (figs. 3 and 5). In case the cooling began at several points on the surface, the interior of the chondrule would be broken into areas each of which represents an independent crystallization, as shown in figure 15. It would thus be fairly easy to account for the monosomatic glassy, cryptocrystalline and radiate forms[26] (Kugels), and even the barred forms so characteristic of olivine chondrules in the stones of Hendersonville and Tennasilm.

The porphyritic polysomatic forms composed of phenocrysts of pyroxene or olivine in a glass base like those of Tennasilm (fig. 8), or Cabarrus (Monroe) offer, as it seems to me, insuperable difficulties to any such theory of origin and I am unable to agree with Dr. Wahl in his, at first thought, lucid and satisfactory explanation. It is obviously impossible that such could have originated from the cooling from the surface inward of a molten drop. Even in the hot air of Dr. Wahl's imagination the crystallization must almost certainly have begun at the surface and progressed inward giving rise to an exterior crust or border as in figure 16. This seems almost too self-evident to need mention. One must consider such

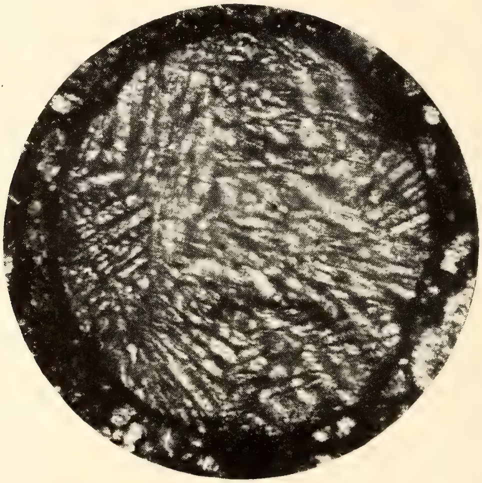

FIG. 15

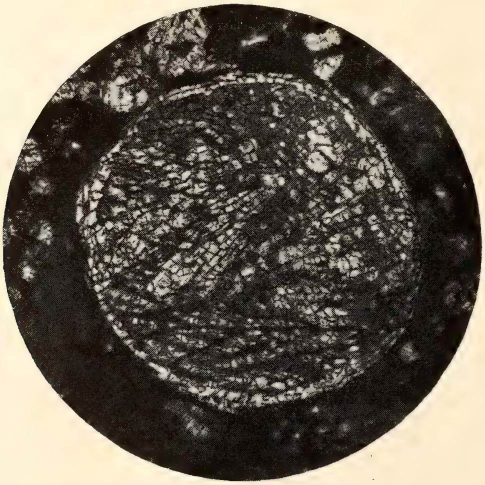

FIG. 16

(i. e., the porphyritic forms) in the light of knowledge gained from a study of terrestrial rocks, and as due to the gradual cooling of a melt of some considerable proportions to the point of crystallization of the phenocrysts, followed by an abrupt refrigeration resulting in the production of the glassy base. This crystallization and final cooling can scarcely have taken place after the drop or chondrule took on its present form. It would seem doubtful, also, if a melt which had cooled sufficiently to allow the formation of the phenocrysts would be sufficiently fluid to permit the formation of the spherical drops under any probable conditions. It is true the separating out of the crystals would leave a magma more fusible than was the original, but it would still require an almost impossible condition to bring about a formation of the pellets as suggested.

It is questionable further if conditions could have been such as to bring about a practically instantaneous, or rather simultaneous cooling throughout the entire chondrule. If cooling as an isolated drop of molten matter in which were floating the phenocrysts there would seemingly, almost for a certainty, be occasional indications of an earlier cooling on the outer surface manifested by the formation of the crust as already mentioned, or conditions of strain or, possibly, signs of incipient crystallization. Such I do not find, the glass being uniform in physical properties and apparently in composition from centre to circumference of the chondrule as it appears in the section. The same holds true of chondrules which are nearly or quite holocrystalline, like those of twinned pyroxene figured[27] by Tschermak from the Renazzo stone, or those from the Cullison, Kansas, stone described by myself.[28] In these cases one has to consider a much more uniform and gradual reduction of temperatures, such as would give the crystallographic forces full opportunity to perform their task. But were the material in the form of an isolated drop, and the forces given sufficient time the exterior form of the aggregate can scarcely be conceived as smoothly spherical, but must have been irregular with numerous projecting angles such as are frequently to be seen in spherical concretions of pyrite and marcasite. Such forms I do not find though I have sought for them diligently. In forms like figures 8, 9 and 10, it is, I think, self-evident that the original surface has been reduced by attrition, and whatever character it may have had has been lost. This same probability of reduction in size is suggested by the twinned pyroxene chondrules in the Parnallee stone. It is obvious that such cannot have resulted from the cooling and crystallization of a molten drop unless it were one of considerable greater magnitude. The occasional curved-enwrapping character of the outer crystals (fig. 11) can only be accounted for on the supposition of Tschermak and Daubree, already noted, that the material was somewhat soft and plastic.

It is notable further, so far as my own observation goes and so far as

I can learn from the descriptions given by other writers, that the peculiar saucer-shaped depressions (fig. 4, p. 452 and fig. 5, pl. 1) which were surely formed at the time of solidification are limited to the cryptocrystalline and radiated enstatite types. They are never found in the polysomatic, porphyritic and holocrystalline forms although these may be of like mineral composition. The cause of this cannot then be chemical. It would seem fair to assume that it was fundamental and due to their origin under quite different conditions.

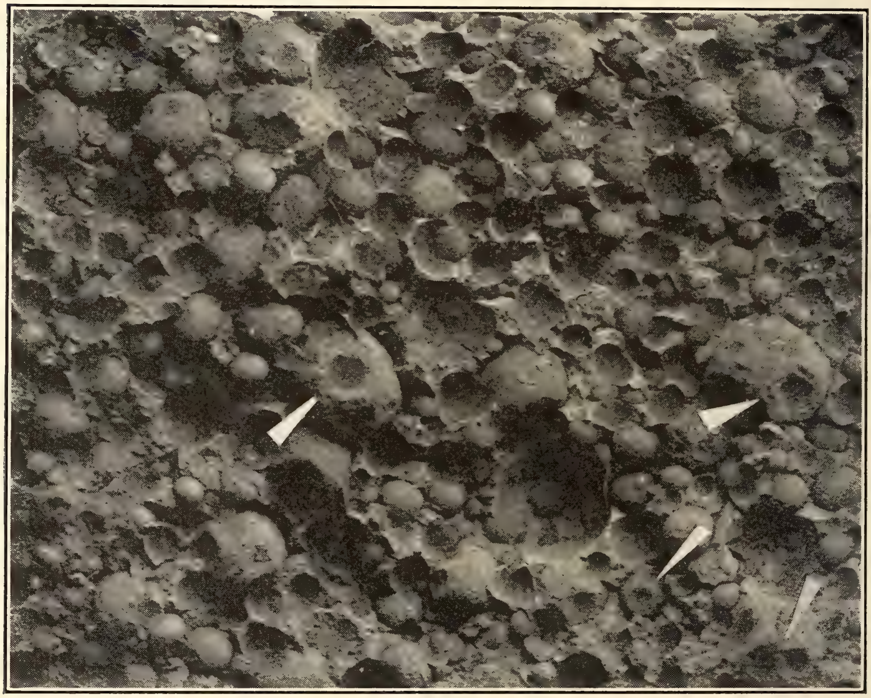

FIG. 17

Chondrule-like blebs of metallic iron in furnace slag showing, in some instances, depressions similar to those of the enstatite chondrules in meteorites. The white pointers mark some of the more prominent forms.

Concerning the origin of these depressions and indeed as bearing upon the general subject of chondritic formation, it may be well to add incidentally that in a fragment of furnace slag received from Lawrence County, Tennessee (exact locality unknown), I find abundant beautifully spherulitic chondrules of metallic iron embedded in the glassy slag. These are from one to three mm. in diameter and often so densely crowded as to have mutually interferred so that when separated there is left on one of them a concavity similar to that of the enstatite chondrules. Indeed the simulation of exterior form is almost perfect (see fig. 17). If, however, one were to assume that the mode of formation of the concavity was the same in the two cases it will apparently be necessary to further assume that the chondritic forms, the fused drops of "fiery rain," accumulated in such quantities before all were fully indurated as by their weight to allow those most firm to indent those still plastic. Were it not for the rarity of the double or excrescent bearing forms referred to elsewhere, it might be suggested that the concavities were due to the breaking apart, one from another, after solidification. It should be added that I have not yet found any evidence even suggesting that the enstatite kugels with radiate structure are basic segregations out of a more acid magma as are the metallic forms out of the slag, nor indeed, to indicate that they result from the disintegration of previously solidified magmas of any kind.

II. With these facts in mind it seemed advisable to study the isolated chondrule, freed from its matrix. For this purpose the loose textured Allegan and Bjurböle stones, of pronounced tuffaceous type, afforded most excellent material, both in quality and quantity. From the disintegrated Allegan stone there were picked out a considerable series of the rounded granules such as are ordinarily classed as chondrules. These, under a hand lens, it was found, could be readily divided into two classes. First, those of regular spherical or oval form with comparatively smooth though lusterless surfaces, usually with one or more depressions as in figure 1, pl. 1, and sometimes with deeper pittings. These were of a light green color, in a few instances, but mainly dark, nearly black. When broken, such show a straight smooth fracture and distinct eccentric radial internal structure (fig. 2, pl. 1). Of a number of thin sections made none showed a holocrystalline or porphyritic structure. Some were so finely cryptocrystalline that it was only by analogy that they were determined as pyroxenic. Others were distinctly eccentric radiating, the bars possessing sufficient crystalline development to make it possible to determine their orthorhombic pyroxenic nature.

Second, the second series was composed of rounded and irregular granules with rougher surfaces, lacking the shell-like rind or casing of the first (Fig. 3, pl. 1). They were hard, undergoing no further disintegration under ordinary friction, sometimes quite spherical, but grading into angular fragments with angles blunted or rounded and with an uneven fracture. Sections of these showed a quite different structure from the first mentioned. Some were wholly crystalline, granular or of short radiating enstatites, while others were beautifully porphyritic consisting of olivine or pyroxene in a glass base. The porphyritic marginal crystals showed plainly that the present form of the granule was due to mechanical attrition (fig. 4, pl. 1). These forms are undoubtedly the ones that in the thin section give the forms shown in figures 8, p. 454, and 10 on p. 455. There can be no question but what they owe their present form and dimensions to attrition as suggested by Tschermak, though whether the original forms were spheroidal or that of an ordinary angular fragment there is no absolute means of determining. From the examination of a large number of cases, I am inclined to the belief that these last are altogether of fragmental origin, broken fragments rounded by mechanical action.

From a lot of disintegrated Bjurböle material, generously furnished by Dr. J. J. Sederholm, there were in like manner picked out the two series shown in figures 5 and 8, pl. 1. It will be noted here that there is a greater contrast between the two forms than in the case of Allegan. Those in figure 5 are typical blebs of fiery rain, smooth, with a rind-like coating showing frequent depressions and more rarely excrescences. A large number of like forms were broken with a hammer and yielded smooth, cleavage-like fractures in part with a radiate structure. In thin sections all showed either a dense cryptocrystalline or radiate internal structure (figs. 6 and 7).

The forms shown in figure 8 were, as in the case of Allegan also, less symmetrically oval or spherical, rough exteriorly, never encrusted or showing depressions or excrescences.[29] On breaking with a hammer some of these yielded a smooth, straight, cleavage-like fracture and in the section showed a radiate structure; others and the larger part showed an uneven fracture and in the section were irregularly granular, glassy and porphyritic (figs. 9 and 10).

I find a similar, though not always so plainly marked, distinction in the chondrules separated out from the meteorites of Ochansk, San Emigdio and Soko Banjo. A further study of a large number of sections of chondritic stones fully confirms these observations. Stated in brief the conditions and conclusions are as follows:

(1) Only the chondrules of glass and cryptocrystalline or radiating enstatites (kugelchen) present the rounded or oval form with smooth rind-like crust and surfaces, with often one or more saucer-like depressions or excrescences such as are consistent with a theory of origin as fused drops of "fiery rain" (Sorby).

(2) Chondrules of a compound, holocrystalline nature, and those porphyritic through the development of olivine or pyroxene phenocrysts in a more or less glassy base are lacking in smooth exteriors and though often quite spherical in outline, are as a rule more or less irregular and in many instances show unmistakable evidences of an origin of form through mechanical attrition. These distinctions are well shown in figures 3 and 5, and in the general view from a thin section of the stone of Cedar, Texas (p. 462).

Many of the irregular forms, it is true, are found to be of the radiating or cryptocrystalline enstatite. This is evident from an examination of the thin sections and has also been found to be the case in some of the forms freed from the matrix. They are plainly fragments of the encrusted forms and need no explanation. In no case, however, have I found a part glassy and porphyritic or holocrystalline structure developed in a polysomatic chondrule which does not suggest, from an examination of its external form, an origin as a rock fragment rather than a molten drop.

That all the forms described cannot be attributable to a common origin, I consider self-evident, as already noted. The objections to an acceptance of the "fiery rain," or "fused drop" hypothesis in accounting for these last forms may be concisely stated as follows:

(1) They are irregular in outline.

(2) They never show the smooth, rind-like surfaces so characteristic of the enstatite kugels.

(3) The outlines are not those of the original body from which they were derived, but for the most part plainly due to abrasion.

(4) They show no gradations in crystallization from border to center.

(5) They never show the peculiar surface depressions so characteristic of the enstatite radial and cryptocrystalline forms.

(6) In brief, their present structural peculiarities, both external and internal are entirely inconsistent with any conceivable theory of origin but that of detrital particles from solidified magmas.

It is perhaps questionable if such forms should be considered true chondrules and perhaps the term pseudochondrule or *chondroid* had best be applied to them.[30] The true chondrules are those of the spherulitic or *kugel* type; others are rock fragments reduced to their present form through mechanical attrition.

As bearing still further upon the question attention may be called to somewhat similar forms of known and artificial origin. In figures 11 to 13, pl. 1, are chondrule-like forms found among the iron furnace slags at the Sparrow Point, Md., works of the Bethlehem Steel Co. Such are certainly strongly suggestive, in form, to meteoric chondrules though it is to be remarked that they are, interiorly, vesicular, almost pumiceous. Like forms I have found being blown from the Bessemer converters in the more energetic periods of operation. They formed true drops of "fiery rain." In figure 12 is shown one of these imbedded in the scoriaceous slag. Qualitative tests show these to consist largely of silica and oxides of iron, a little lime, and included minute flecks of metallic iron.

In the above discussion, the fact has not been overlooked that very many of the glass and enstatite chondrules relegated above to the fiery rain hypothesis have likewise suffered abrasion through external agencies. This is plainly evident from the figures given and the multitude of fragmental forms found in many chondritic stones. The encrusted form of so large a number of chondrules of this type and the complete absence of such crust on the porphyritic and holocrystalline forms I have just been discussing are in themselves sufficient to enable one to safely differentiate between the two.

The conclusions here reached are not quite so sweeping as those reached by Merrick in his humorous poem on the changeable colors of the chameleon, where he announces that "All are right, and all are wrong," but as is so frequently the case it seems to show that two or more of the opinions advanced were essentially correct and their apparent differences due to failure to recognize all of the facts I have here presented, and that of the many varietal forms of chondrules not all may have a similar origin.

Before leaving this section of the subject, it may be well, however, to present reasons for the non-acceptance of certain of the views presented and enumerated. It will be noted that the conclusions I have reached, bear out for the varying types the ideas of both Sorby and Tschermak, those of the first mentioned supported by the experimental work of Rinne. With the views of Borgström and of Ramsay, so far as they bear upon the subject in the papers quoted, I am in agreement. With those of Hussak, so far as relates to the production of chondritic structures through the corrosive action of a nickel-iron-rich magma, I find no confirmation. Not only is there little evidence that such a magma would corrode the silicates, but as in the case of meteorites of Brezina's Röckiky group (Admire and Eagle Station) there is direct evidence that it would not. Further than this, there is evidence that in my own opinion, tends to show that the iron of meteorites is altogether secondary and due to a reduction of some easily reducible compound like lawrencite, as I have elsewhere frequently stated.[31] The expressed conclusions of Berwerth and Wahl are not sufficiently detailed to admit of close comparison other than to say that they both seem applicable to the chondrules of the encrusted enstatite (kugel) type first mentioned, but not to the holocrystalline and porphyritic forms. With reference to Dufrenoy's comparison of this structure to perlitic glass[32] it can only be said that while at first thought it might seem applicable to the glassy and half glassy porphyritic types, I find nothing whatever to sustain it and several facts which militate against it. First there are in no instances indications of perlitic cracks and second, in the terrestrial perlites, so far as my own observation goes, the cracks always pass around any existing phenocrysts, rather than through them as they must to have produced the forms shown in figure 8. It may be added that perlitic structure in terrestrial rocks is regarded as due to shrinkage and limited to acidic magmas containing a considerable quantity of moisture. The structure is quite unknown in rocks as basic as are the meteorites.

With reference to the spherulitic nature of the chondrules as advocated by Klein, I can only say that I find no evidence, with a single possible exception, satisfactory to me, to bear out the idea. This exception occurs in the stone of Barratta, Australia, in one slide of which I find the spherulitic bodies shown in figure 1, p. 451. These are as sharply outlined and as easily detached from the matrix as any spherulites and so closely resemble the true spherulite from one of the acid glasses as on casual inspection to strongly suggest their similar nature. Indeed, I have thus far failed to find evidence to completely satisfy me that such would not be so classed. It is to be noted, however, that between crossed nicols these break up each into several more or less well-defined areas showing an illy defined structure and over each of which the dark cloud sweeps irregularly as the stage is revolved. They lack the definite radial structure of typical spherulites and never between crossed nicols show the black cross, though how much reliance can be placed upon this distinction, I am not prepared to say. They are sufficiently distinct from the other chondrules in the stone to suggest an entirely different mineralogical nature, but the amount of material at my disposal is not sufficient to allow a determination. Liversidge,[33] in his description, makes no mention of these peculiar forms.

Finally with reference to Fermor's fused garnet theory it may be stated that in no case have garnets been proven to actually occur in meteorites, and further, did the chondrules so originate it would be a perfectly safe assumption that we should sometime and somewhere find residual traces. So far as my observation goes, no such cases are on record.

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[1] Published by permission of the Secretary of the Smithsonian Institution. This paper is supplementary to the author's work on "Minor Constituents of Meteorites" made under a Grant from the J. Lawrence Smith Fund of the National Academy of Sciences (see Memoirs of the Academy, **14**, 1916, pp. 1–29; 1919, pp. 1–15; and these PROCEEDINGS, March and June 1915, April 1918, and January 1919).

[2] Geschreibung u. Eintheilung der Meteoriten, etc., *Abhand. d. K. Akad. Wiss. Berlin*, 1863.

[3] See Farrington, *Meteorites*, p. 200.

[4] Or "globular," see *Proc. Amer. Phil. Soc.*, **43**, 1904 (238).

[5] See particularly *Die Mikroscopische Beschaffenheit der Meteoriten*, and *Meteoritenkunde*, respectively.

[6] No attempt in these studies has been made to distinguish between enstatite and the ferruginous variety bronzite.

[7] *Pogg. Ann.*, **111**, p. 384.

[8] *Proc. Royal Soc.*, June 1864.

[9] *Nature, London*, April 5, 1877 (296).

[10] Die Trümmerstructur d. meteoriten, etc., *Sitz. k. Acad. Wiss., Wien*, **70**, 1874 (4).

[11] *Ibid.*, **71**, 1875.

[12] *Denk. Math. Natur. Classe kaiser. Akad Wiss*, **39**, 1878.

[13] *Sitz. k. k. Akad. Wiss., Wien*, **95**, 1882 (205).

[14] *Geologie Experimentale*, 1879 (530).

[15] *Neues Jahrbuch Min. Pet.*, **2,** 1895 (229–246).

[16] *Ibid.*, —, 1897 (259–61).

[17] *Centralblatt Min.*, etc., No. 21, pp. 641–47.

[18] Die Meteoriten von Hvittis u. Marjalatti, Helsingfors, 1903.

[19] *Trans. Royal Astr. Soc., Canada,* 1904.

[20] *C. R. Paris Acad. Sci.*, **96,** 1883 (868).

[21] *Die Meteoriten in Sammlungen*, etc., 1885.

[22] *N. Jahrb. Min.*, etc., **I,** 1884 (18–43). They immersed fragments of an "olivinefels" for many hours in a slowly cooling melt of nephelin basalt. The stone was strongly attacked and the outer portions, in close contact with the melt, shattered and corroded, the olivine granules becoming filled with embayments and enclosures of a secondary colorless glass, all strongly suggestive of meteoric chondrules.

[23] *Studien uber Meteoriten,* p. 35.

[24] *Zs. Anorg. Chem.*, **69,** 1910 (52–96).

[25] *Records Geol. Survey India,* **43,** 1913 (45).

[26] Such forms may readily be artificially reproduced by fusing to a bead some readily fusible mineral like stibnite. Although unfortunately opaque the radiate structure may be plainly seen on a broken surface of the globule.

[27] *Mikro. Beschaff, der Meteoriten,* plate 15.

[28] *Proc. U. S. Nat. Mus., Washington,* **44,** 1913 (326).

[29] Borgström and Ramsay in their description of the Bjurböle stone (*Bull.* No. **12,** *Com. Geologique Finland*) mention the irregularity in form of the chondrules, as seen in thin sections. They say, p. 18 under (6) *Polysomatische Olivinchondren mit Nickel eisen* "Die Chondren dieser Art haben selten regelmässig runde Form." and again, on p. 19 under (8) *Chondren aus Glas mit wohlbegrenzten dicken Olivinen,* "Sehr selten besitzen die Chondren von Typus (8) regelmassige rundliche Begrenzung, sondernerscheinen in den meisten Fallen mit Zufalligen eckigen oder buckligen Formen."

[30] In figure 1, of plate 8, of his *Mikroskopische Beschaffenheit,* Tschermak shows a small monosomatic chondrule inclosed in a larger porphyritic form of the same mineralogical composition, from the stone of Dhurmsala. It is to be noted, however, that while the smaller monosomatic form is evidently the direct product of a cooling molten magma, it has no necessary connection with the ground in which it is embedded and there is nothing in the appearance of the larger form, so far as can be judged from the figure, to indicate that it is not merely a rounded fragment of some pre-existing chondritic stone. This explanation will not, however, hold good for the double chondrule in the Borkut stone shown in figure 2 of his plate 19. In the case of our figure 3, plate 3, from the Okechobee stone there can be no question but that the chondrule is merely a fragment inclosing a large enstatite.

[31] I find no reason in the criticisms of Quirke (*Economic Geology,* **14,** 1919 (621)) to change my oft expressed views regarding the origin of the metallic iron through the reduction of lawrencite or some other easily reducible compound. Chlorine is a gas and in a dry atmosphere would readily escape of itself and without evident action on the silicate constituents. If means were to be "provided" it would have to be for the retention rather than for getting rid of this constituent.

[32] *C. R. Paris Acad. Sci.*, **13,** 1841 (48).

[33] *Jour. Proc. Roy. Soc., New South Wales,* **36,** 1902.

THE VERTICAL INTERFEROMETER

By A. A. Michelson

Ryerson Physical Laboratory, University of Chicago

Read before the Academy, April 26, 1920

The investigation of the distribution of intensity in spectral lines by interference methods while somewhat laborious as compared with the direct methods, visual or photographic, has the important advantage of freedom from instrumental uncertainties such as arise from optical imperfections, finite slit width and diffraction, which are especially objectionable in the case of lines or groups so narrow as to be barely within the resolving power of the spectroscopic appliances employed.

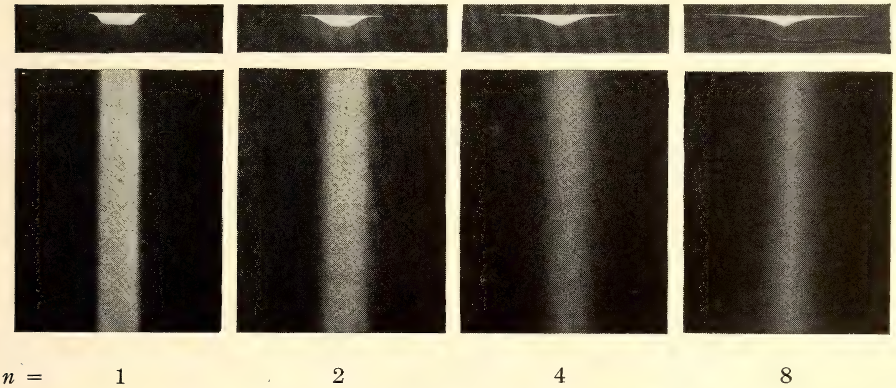

FIG. 1.

Besides the theoretical value of such investigations as affecting the interpretation of such distribution as based on various assumptions of kinetic theory, effect of temperature, pressure, electric and magnetic fields, a practical application may be mentioned, namely the accurate description of the appearance of spectral lines, which at present are characterized as broad, narrow, diffuse, etc. Thus, for symmetrical lines this distribution may be expressed by $y = 2^{-(x/\Delta)^n}$, in which Δ is the half width of the line and n determines the "square shouldered" character, as illustrated in figure 1.

One of the difficulties in the experimental determination of the visibility of the interference fringes as a function of the difference in path, from which the distribution of intensity may be inferred, is the maintenance of parallelism of the moving mirror, and the vertical interferometer is designed to obviate this objection.

For this the moving mirror is the level surface of mercury in a shallow dish (at least 7 cm. in diameter to avoid curvature due to capillarity).

The tremors of such a surface which would otherwise make observations impossible, are avoided by the simple device of amalgamating the surface of the shallow dish and covering with a layer of mercury a half millimeter or less in depth, whereby the vibrations are very rapidly extinguished. A photograph of the instrument is shown in figure 2.

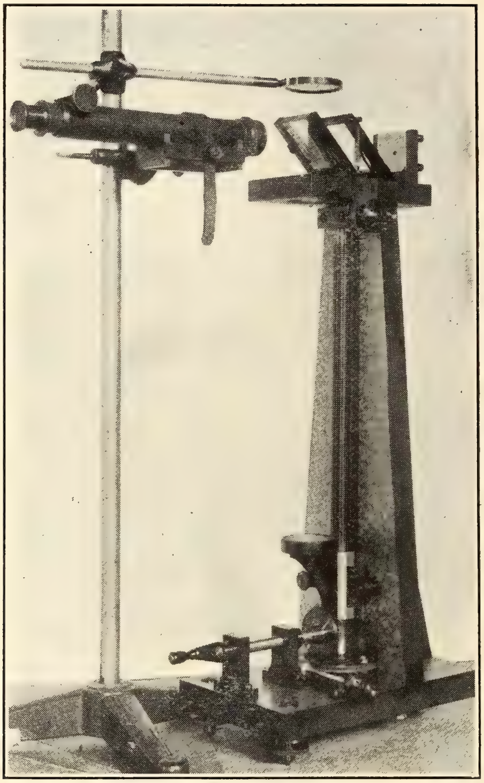

FIG. 2

ON THE APPLICATION OF INTERFERENCE METHODS TO ASTRONOMICAL MEASUREMENTS

By A. A. MICHELSON

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Read before the Academy, April 26, 1920

In order to test the practicability of using an interferometer with large base, for the purpose of measuring close double stars, and possibly the diameter of some of the larger stars in the manner set forth in the *Philosophical Magazine* for July 1890, a trial was made at the Yerkes Observatory using two apertures in front of the 40-in. refractor.

The interference fringes at the focus remained fairly steady, with "seeing" only 2 to 3 on a scale of 5.

On invitation of Dr. George E. Hale the test was repeated with the same success with the 60-in. reflector.

With the 100-in. reflector the interference fringes were produced by a modification consisting in placing the screen with the two apertures at a short distance from the focus, which made the control of the distance between them and their position angle much easier. The interference fringes were again fairly steady, even when with "seeing" but 2 on a scale of 10, and consequently very considerable "boiling" of the images under the high magnification necessary (2000 to 3000 diameters).

The following is a resumé of observation of Capella by J. A. Anderson, showing that measurements may be obtained with an error of but a few ten-thousandths of a second of arc.

STATEMENT OF RESULTS FROM OBSERVATIONS OF CAPELLA WITH THE MICHELSON INTERFEROMETER APPARATUS

DATE OF OBSERVATION	POSITION ANGLE	DISTANCE
Dec. 30, 1919	148°.0	0″.0418
Feb. 13, 1920	5.0	0.0458
Feb. 14, 1920	1.0	0.0451
Feb. 15, 1920	356.4	0.0443
Mar. 15, 1920	242.0	0.0505

The constants of the apparent ellipse fitting these observations are as follows:

Semi-major axis	0″.0511
Semi-minor axis	0.0417
Position angle of major-axis	49°.2

The spectroscopic orbit being practically a circle ($e = 0.016$) the angle between the orbit plane, and the tangent plane to the celestial sphere is 35° (or better 145°, since motion is retrograde).

Parallax of Capella a little under 0″.050.

Preparations are now under way at Mt. Wilson for testing the possibilities of the interferometer method with a base of 18 to 20 feet.

Further details will appear in the *Astrophysical Journal.*

A MODIFICATION OF THE REVOLVING MIRROR METHOD FOR MEASURING THE VELOCITY OF LIGHT

By A. A. Michelson

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Read before the Academy, April 26, 1920

The accuracy of any measurement of the velocity of light by the revolving mirror depends on that of three factors:

(1) The distance between stations.

(2) The speed of rotation.

(3) The angle described by the revolving mirror while light goes to the distant station and returns.

It is estimated that (1) and (2) may be determined to within one part in 200,000 and possibly one in a million.

The third measurement may be eliminated by constructing a revolving mirror in the form of a prism with a square or octagonal base, the angles of which may be made equal to 90° (or 135°) to this same order of accuracy.

For this a test angle is made by cementing a prism of approximately 90° (or of 45°) to a true plane. The square (or octagon) is applied to the two surfaces, producing interference bands when illuminated by the light from a mercury lamp.

If these are straight and parallel to the axis of rotation, the angles are equal; if not the difference, α, is measured by $\epsilon/2b$, if ϵ is the fraction of the fringe width and b the width of the mirror face. Thus if $\epsilon = \lambda/20$ and $b = 15$ mm., $\alpha = 0.000001$, which will, therefore, be the order of accuracy of the measurement under these conditions. For a wider face the accuracy would be still greater. If the angle of the test prism is θ_0 and the measured differences are α_0, $\alpha_1 \ldots \alpha_n$, then completing the 360° gives $\theta_0 = (360 - \Sigma\alpha)/n$, whence $\theta_1 = \theta_0 + \alpha_1$, $\theta_2 = \theta_0 + \alpha_2$, etc.

The faces are corrected until the angles are all equal within the limit of error assigned. This method of constructing reflecting surfaces inclined at equal angles, which may readily be extended to any number of surfaces, may prove useful in the construction of standard test angles, which may thus be made accurate to a tenth of a second or less; and also in the subdivision of meridian circles, etc., in which the limit of error is of the order of one or two-tenths of a second.

Such a revolving mirror has been constructed in form of an octagon with faces of 15 by 20 mm. and with air under pressure of one atmosphere, a speed of revolution of over 1000 turns per second has been attained. This makes it possible to obtain a rotation of 45° while light is passing twice over the distance between stations $12^1/_2$ miles apart; or of 90° if the stations are 25 miles apart.

It might be thought that with so small a mirror face the intensity of the return image would be insufficient; but by placing the revolving mirror in the focus of a very long focus lens or mirror, and placing the slit source at a distance $\nu = f^2/D$, in which f is the focal length and D the distance between stations, the intensity is a maximum and yet a small mirror is sufficient to return all the light which enters the lens. This last (or the concave mirror which replaces it) should be as large as possible as also, of course, the distant mirror.

Incidentally the eight reflections from the revolving mirror, will give four times as much light as a two-faced plane mirror.

Preparations are now in progress for a preliminary test of the method at Mt. Wilson, where it is hoped a distance of 25 miles may be utilized.

ON THE K SERIES OF X-RAYS

BY WILLIAM DUANE AND WILHELM STENSTRÖM

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Communicated July 1, 1920

In the research reported in this paper the authors have measured the wave-lengths of the emission lines and that of the critical absorption in the K series of tungsten. In order to obtain as precise values of the wave-lengths as possible they employed spectra of the first, second, third, fourth and, in one instance, the fifth order.

The object of the research has been to provide data for testing the following points: (*a*) The existence of a third line in the α-group; (*b*) the separation of the critical absorption from the line of shortest wave-length in the emission spectrum, namely the γ line; (*c*) the experimental and theoretical relations between the various lines in the K, L, M, etc., series; (*d*) the relative intensities of the emission lines; and (*e*) the equations for the wave-lengths that may be deduced from theories of the structure of atoms and the mechanism of radiation.

The X-ray spectrometer, the generating plant used to operate the X-ray tube, the instruments for controlling the current and voltage and the general method of making the measurements have been described in papers from this laboratory published during the last few years in the

Physical Review. The chief difference between this and previous researches lies in fact that we have used spectra of higher orders, thus obtaining greater dispersion and precision.

Measurements of the electric current in the ionization chamber of the spectrometer were made at intervals of 30″ of arc on the scale which gives the grazing angle of incidence of the X-rays that strike the crystal. The curves in figures 1, 2 and 3 represent the ionization current as a function of the grazing angle of incidence, θ, in typical experiments.

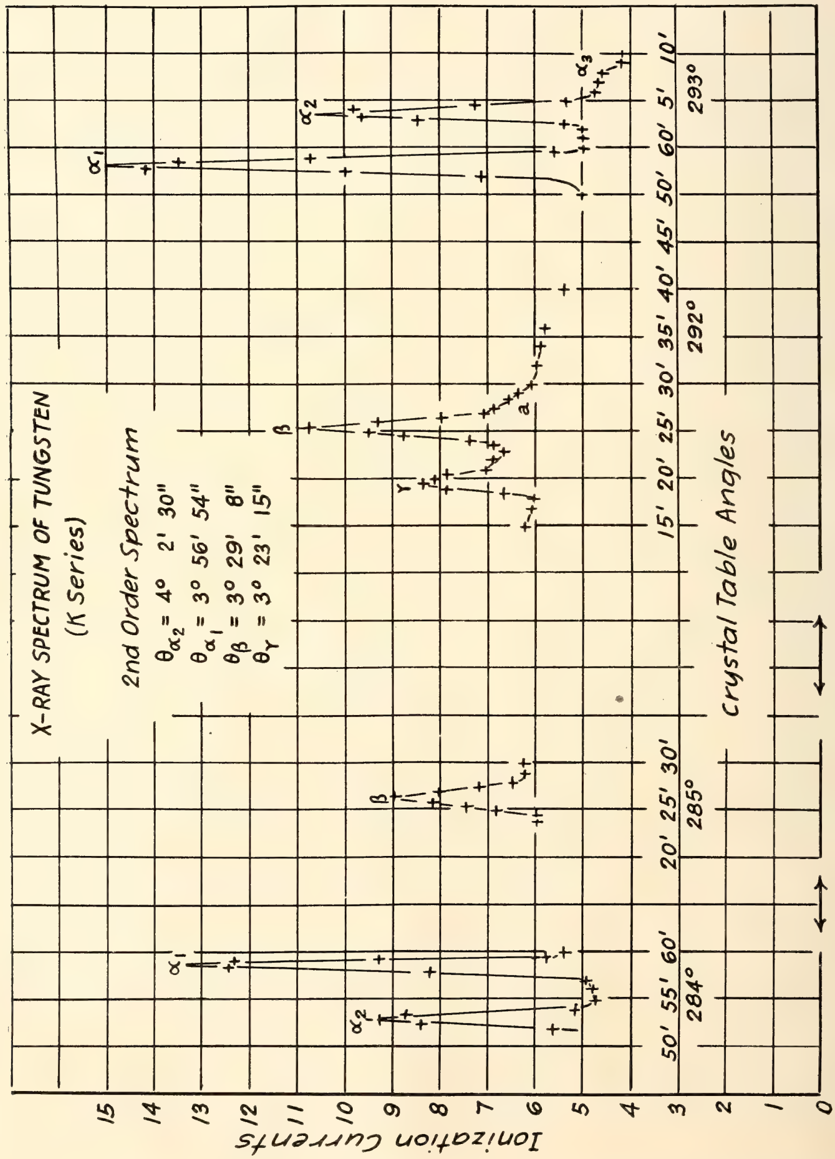

FIG. 1.

The tips of the peaks in these curves correspond to the lines in the characteristic X-ray spectra. It is possible to determine with certainty between which two settings of the crystal a peak lies, and also about how far from each it is. This means that, when the apparatus is functioning well, the grazing angle of incidence can be estimated to within about 7″ of arc. For the tungsten K series the grazing angles of incidence in the first order spectrum amount to about 2° = 7200″, and, therefore, the error in a satisfactory determination of a wave-length ought not to exceed $^1/_{10}$, $^1/_{20}$, $^1/_{30}$, $^1/_{40}$, etc., per cent in the spectra of the first, second, third, fourth, etc., orders, respectively.

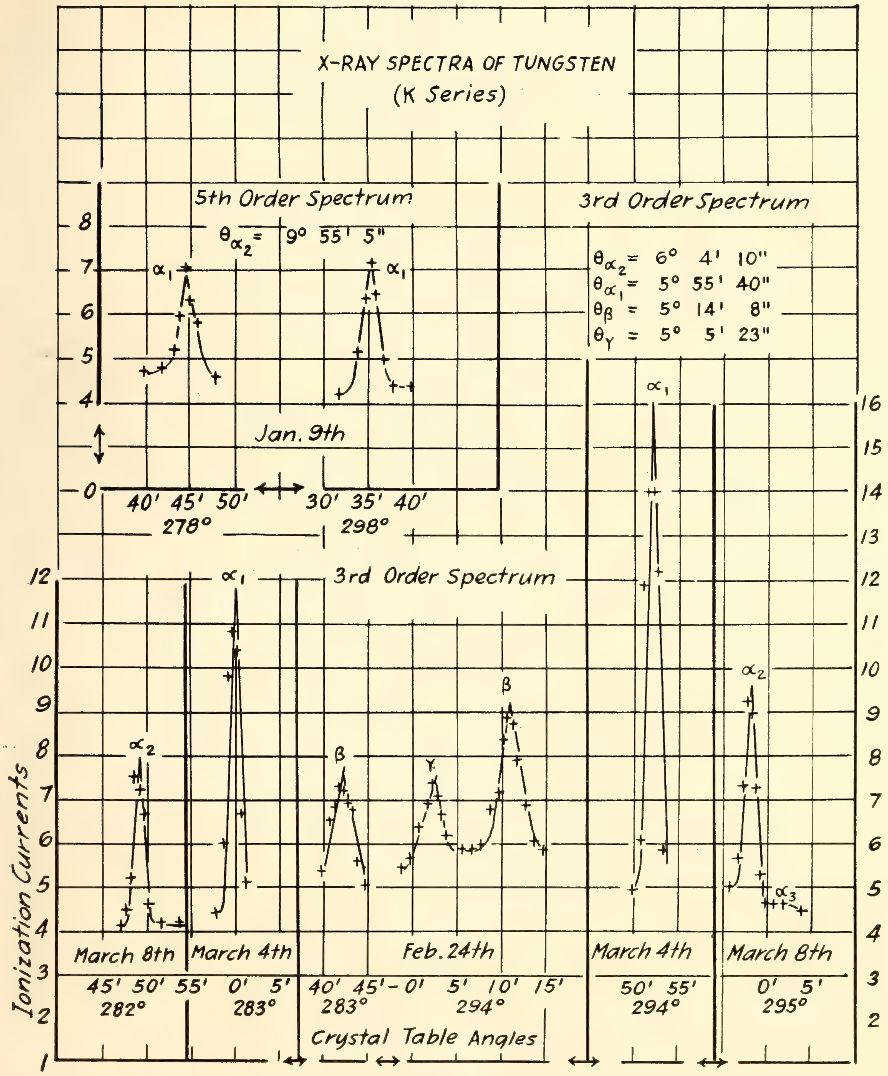

FIG. 2

In estimating the wave-lengths, etc., only those measurements may be used that are made on the same date, for the zero of the instrument may have shifted between the dates recorded in the figures.

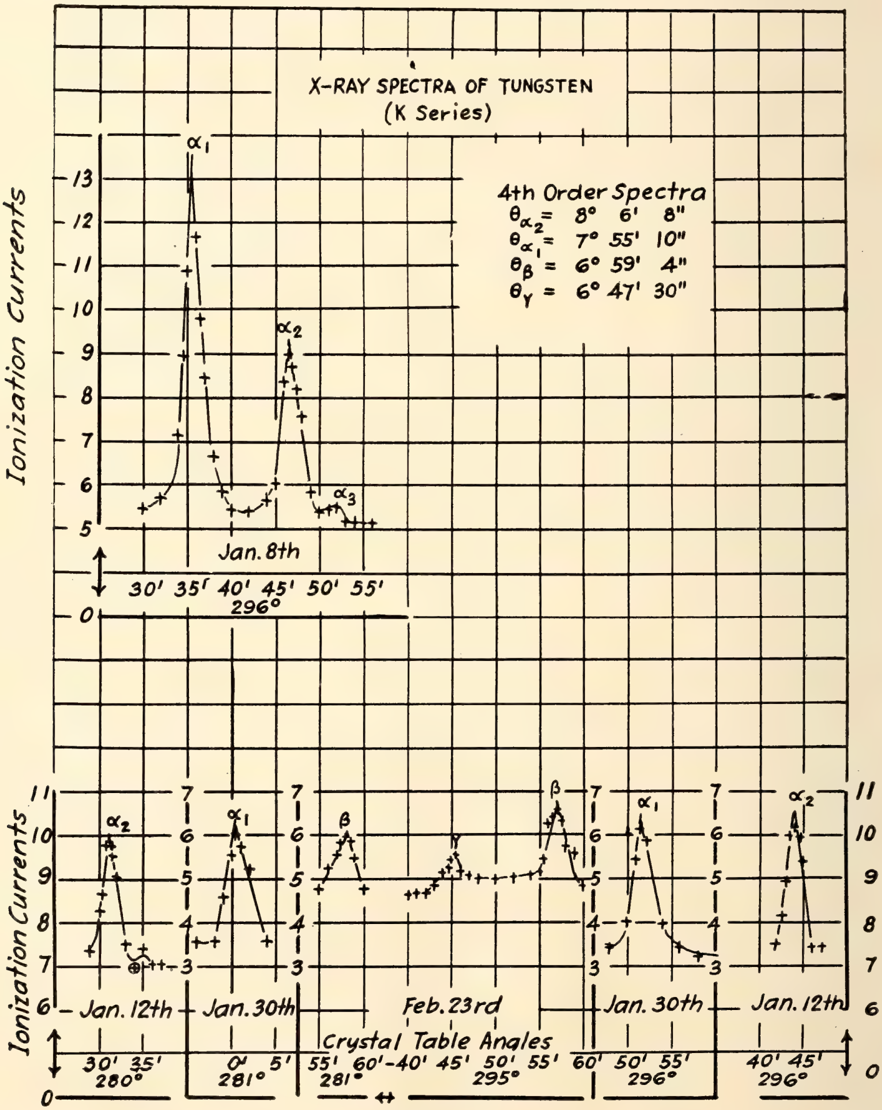

FIG. 3

The following table contains the wave-lengths, λ, calculated from the average grazing angles of incidence, θ, obtained from all the measurements we have made in the spectra of the various orders, respectively.

TABLE 1

K SERIES OF TUNGSTEN

$n\lambda = 6.056 \times \sin\theta \times 10^{-8}$ cm.

ORDER, n, OF SPECTRUM	$\lambda \times 10^8$ cm.					
	α_3	α_2	α_1	β	γ	ABSORPTION
1				0.18441	0.17898	0.17833
2		0.21342	0.20850	0.18415	0.17880	0.17810
3		0.21339	0.20848	0.18420	0.17909	0.17795
4	0.215	0.21341	0.20862	0.18418	0.17905	
5			0.20862			
Weighted Means	0.215±1	0.21341±3	0.20860±4	0.18420±3	0.17901±6	0.17806±7
Last Year		0.2134	0.2087	0.1842		0.1785
Siegbahn		0.21345	0.20878	0.18430	0.17934	
$\frac{1}{\lambda}$	4.65	4.6858	4.7938	5.4290	5.5863	5.6160
$\frac{\nu}{\nu_\infty}$	4239.	4270.3	4368.7	4947.4	5090.9	5118.0

The seventh line in the table contains the weighted mean values of the wave-lengths, together with a rough estimate of the precision of the measurements. The γ wave-length is more difficult to measure than the others owing to the fact that the critical absorption wave-length lies so near it. The target of the X-ray tube absorbs some of its own rays.

In order to estimate the absolute accuracy of the wave-lengths we must, of course, as in all work in X-ray spectra, take account of the errors in the grating constant of the reflecting crystal. These appear to add up to about 0.06%. (See a "Report on Data Relating to X-Ray Spectra," published by the National Research Council.)

For purposes of comparison we have added, in the eighth and ninth lines, respectively, the values obtained last year by the ionization method (*Physical Review*, July, 1919, p. 67) and those given by Siegbahn (*Phil. Mag.*, Nov., 1919, p. 639), which he measured by photographic methods. A small correction ($^1/_{3000}$) has been made in these wave-lengths corresponding to the value of the grating constant of calcite we use.

In order to test general relations between the wave-lengths and also theoretical equations it is convenient to have at hand the wave-numbers, $1/\lambda$, and the ratios, ν/ν_∞, of the frequencies, ν, to the fundamental Rydberg frequency, ν_∞. In calculating these ratios we have used the Rydberg fundamental wave-number, $\nu_\infty = 109737$, calculated for heavy atoms from the data obtained by Paschen in the spectra of hydrogen and helium. Line 10 in table 1 contains the values of $1/\lambda$, each divided by 10^8, and line 11 contains the values of ν/ν_∞, calculated from our values of λ.

The critical absorption wave-lengths appear in the last column of table

1. Dr. R. A. Patterson coöperated with us in making the measurements of these wave-lengths. We used an X-ray tube with a molybdenum target in these experiments.

It appears from the data that the wave-length of the γ line and that of the critical absorption differ from each other by about $^1/_2$ of 1%. In the K series spectrum of rhodium (see *Physical Review*, Oct., 1919, p. 369) the two wave-lengths differ from each other by about $^1/_4$%. If we adopt the theory of electrons revolving in atomic orbits we can explain this difference in wave-length as follows: According to the theory the critical absorption frequency equals the amount of energy required to carry an electron from the K orbit out to the periphery of the atom divided by Planck's constant, h. The γ line is due to electrons falling into the vacancies in the K orbit from orbits outside of those that we may class together and call the M orbits. The amount of energy set free and radiated during one of these transfers of an electron is less than the amount of energy required to lift the electron all the way from the K orbit to the periphery of the atom. Since the frequency of the X-ray emitted during the transfer equals this energy divided by h, it follows that the frequency must be less than that of the critical absorption, and, therefore, its wave-length greater than that of the critical absorption. The γ line is probably complex, and the wave-length we measure must be a kind of centre of gravity of the wave-lengths of its components.

In the second order spectrum (fig. 1) a drop occurs in the curve at a certain point marked "*a*." This does not belong to the K series of tungsten. It represents the critical ionization of iodine in the first order spectrum, the wave-length of which is 0.3737×10^{-8} cm. We used methyliodide in the ionization chamber of the spectrometer.

In a research described in the *Physical Review* for July, 1919, p. 67, Mr. Shimizu and one of us obtained experimental evidence showing that the difference between the K critical absorption frequency and any one of the L critical absorption frequencies equals the frequency of one of the emission lines in the K series. Theoretically this relation ought not to be exact, if orbits in some of the atoms are elliptical and in other atoms of the same chemical element, circular. The atoms with elliptical orbits contain amounts of energy that are different from the amounts of energy in atoms with circular orbits, and hence the change in energy when an electron is removed from the K orbit will not be the same in the two cases.[1] This means that the K critical absorption frequency should be complex, and that what we measure is a kind of average frequency. Strictly, according to the theory, the frequency difference law should apply to those atoms only that are exactly alike.

To test this point we have taken the absorption measurements made on the L series of tungsten by Dr. R. A. Patterson and one of us. The law applies, of course, to the wave-numbers as well as to the frequencies.

The wave-numbers of the three critical absorptions in the L series of tungsten are

$$La_1 = 0.8240 \pm 1 \quad La_2 = 0.9323 \pm 4 \quad La_3 = 0.9766 \pm 30,$$

each multiplied by 10^8. Subtracting these from the critical absorption wave-number in the K series and comparing the differences with the wave-numbers of the three α emission lines we get

WAVE-NUMBERS, $1/\lambda \times 10^{-8}$

$Ka - La_1 = 4.792 \pm 0.003$	$K\alpha_1 = 4.7938 \pm 0.001$
$Ka - La_2 = 4.685 \pm 0.003$	$K\alpha_2 = 4.6858 \pm 0.0007$
$Ka - La_3 = 4.639 \pm 0.006$	$K\alpha_3 = 4.65 \pm 0.02$

It appears that in each case the difference between the K critical absorption wave-number and one of the L critical absorption wave-numbers equals the wave-number of one of the α emission lines to within the limits of error of the measurements. This agrees with the results obtained last year. If the above mentioned effect due to elliptic orbits exists at all, it appears to be too small to detect, with our present methods of measurements, in atoms of as high an atomic number as that of tungsten.

The existence of the third faint line, α_3, and the agreement of its wave-number with the difference between the Ka and La_3 wave-numbers is a point of some theoretical interest. According to Sommerfeld's theory of elliptic orbits the $K\alpha_1$ line is due to the transfer of an electron from the L orbit to the K orbit, if the L orbit is circular, and the $K\alpha_2$ line is due to a similar transfer, if the L orbit is elliptic. The difference between the wave-numbers of $K\alpha_1$ and $K\alpha_2$ calculated from the formulas he gives (*Atombau und Spektrallinien*, chapter 5) amounts to 0.1072×10^8, and this agrees very well with our experimental value for the same difference, namely, 0.1080×10^8. There appears to be no explanation on this theory for the third critical absorption, La_3, in the L series, nor for the third line $K\alpha_3$ in the K emission series. It would seem to be necessary to assume that there are at least two L orbits.

The peak corresponding to $K\alpha_3$ seems to be quite well marked on the curves representing spectra of higher order than the first, especially on that representing the fourth order. The line is very faint, having only about $^1/_{25}$ of the intensity of the $K\alpha_1$ line.

The $K\beta$ line is supposed to be due to electrons falling from the M orbit (or orbits) into the K orbit. We can not test the above frequency difference law in this case directly, for the critical absorption frequencies have not been measured in the M series of tungsten. Dr. Stenström, however, has observed three critical absorption wave-lengths in the M series of thorium and uranium. He has measured also the wave-lengths of emission lines in the M series of uranium, thorium and tungsten. Since the relative positions of the critical absorption and certain of the emission lines are about the same in the high frequency spectra of the different chemical elements, we can estimate approximately where the critical absorption wave-lengths lie in the M series of tungsten. Making this estimate, and calculating the three critical absorption wave-numbers we obtain the following values, each multiplied by 10^8,

$$Ma_1 = 0.152 \quad Ma_2 = 0.157 \quad Ma_3 = 0.182.$$

Subtracting these from the K absorption wave-number, and comparing the differences with the Kβ emission wave-number we get

$$\begin{array}{ll} Ka - Ma_1 = 5.464 & \\ Ka - Ma_2 = 5.459 & K\beta = 5.429 \\ Ka - Ma_3 = 5.434 & \end{array}$$

One of the differences between absorption wave-numbers, $Ka - Ma_3$ lies very close to that of the Kβ line. This suggests that the Kβ line is due chiefly to electrons falling from the third M orbit to the K orbit. If electrons also fell from the second M orbit to the K orbit they would produce a line on the short wave-length side of the principle Kβ line. Faint lines on the short wave-length sides of the Kβ lines have been observed in the spectra of a few of the chemical elements of low atomic number by Mr. Hjalmar working in Professor Siegbahn's laboratory.[2] The separation of these components of the Kβ line from each other amounts to a small fraction of 1%.

According to Rubinowicz' "Principle of Selection" transfers of electrons from the first M orbit to the K orbit ought not to occur.[3]

The faint component that has been observed by Siegbahn,[4] de Broglie[5] and Hjalmar on the long wave-length side of the Kβ line in the spectra of a few of the chemical elements was predicted by Sommerfeld[6] by reasoning from his theory of elliptic orbits.

These components of the Kβ line may account for some of the irregularities in the β peaks on our curves (figs. 2 and 3).

Some years ago Kossel suggested that the difference between the frequencies (or wave-numbers) of the Kβ and Kα lines should equal that of a line in the L series. It is well known that this relation, although approximately true, is not quite exact. An explanation for this discrepancy may be found in the supposition that the electrons producing the Kβ line may not come from the same M orbits as those producing the Lα and Lβ lines.[7] Dr. R. A. Patterson and one of us recently presented to the American Physical Society an account of some experiments which show that the wave-numbers of the $L\alpha_1$, $L\alpha_2$ and $L\beta_1$ lines of uranium and thorium equal the differences between certain of the L and M critical absorption wave-numbers. These relations may be expressed by the following equations:

$$La_1 - Ma_1 = L\alpha_1, \; La_1 - Ma_2 = L\alpha_2 \text{ and } La_2 - Ma_2 = L\beta_1.$$

In terms of the transfer of electrons between orbits these equations mean that the $L\alpha_1$, $L\alpha_2$ and $L\beta_1$ lines are due to electrons falling from the M_1 and M_2 orbits into L orbits, whereas we inferred above that the $K\beta$ line was mainly due to electrons falling from the M_3 orbit into the K orbit. A rough quantitative test for this explanation may be obtained as follows: From the above three equations and the three equations representing the transfers of electrons producing the $K\alpha$ and $K\beta$ lines, namely

$$Ka - La_1 = K\alpha_1,\ Ka - La_2 = K\alpha_2 \text{ and } Ka - Ma_3 = K\beta,$$

we get the equations

$$K\alpha_1 + L\alpha_1 - K\beta = Ma_3 - Ma_1 \text{ and } K\alpha_2 + L\beta_1 - K\beta = Ma_3 - Ma_2.$$

The left-hand members of these equations have the values 0.043 and 0.039, respectively, for tungsten. The right-hand members, which are the differences between quantities that we have estimated by extrapolation, have the values 0.030 and 0.025, respectively. These small differences are of the same order of magnitude, which indicates that the explanation is approximately correct. We must remember, however, that the M series is probably more complicated than we have supposed. There are reasons for believing, for instance, that another critical absorption exists, of slightly higher frequency than Ma_3.

The ionization spectrometer furnishes us a method of approximately estimating the relative intensity of the lines in X-ray spectra. At present, however, we are not able to correct for the change with wave-length in the absorption of the rays by the target, the glass walls of the X-ray tube and ionization chamber, the reflecting crystal, etc. Nor do we know exactly how the coefficient of reflection of the crystal varies with the wave-length. It is not safe, therefore, to compare spectral lines that differ very much from each other in wave-length.

By estimating the heights of the peaks corresponding to the lines of the K series in the spectra of the first, second, third and fourth orders we have arrived at the following numbers representing approximately their relative intensities:

Line..................	α_3	α_2	α_1	β	γ
Intensity.............	4	50	100	35	15

It is interesting to note that, if we apply Sommerfeld's theory of the relative intensity of lines (Atombau und Spektrallinien, chapter 6) to the α_1 and α_2 lines, we get either 3 : 2, or 2 : 1, as the ratio of their intensities. Our experiments favor the latter ratio.

In an address delivered at the St. Louis meeting of the American Association for the Advancement of Science, last December (see *Science,* May 21, 1920, p. 505) one of us presented a set of calculations of the K critical absorption frequencies for a number of chemical elements. The calculations were based on Bohr's theory, with the additional assumption that the electrons were distributed among the orbits in much the same way as they are distributed in the layers and shells of the Lewis-Langmuir static atom. The ratio, ν/ν_{∞}, of the K critical absorption frequency for tungsten to the Rydberg fundamental frequency is 5118 according to the theory, which is exactly our experimental value (see table 1). The agreement to four figures between the two values must be regarded as a matter of chance, however, as the errors in both the experimental and calculated numbers amount to a fraction of 1%.

[1] See Sommerfeld's paper on the fine structure of the β-line (*Münchener Akademie,* June 1, 1918 (367)).

[2] Professor Siegbahn kindly communicated these results by letter.

[3] *Physik. Zs.,* **19,** 1918 (441, 465).

[4] *Phil. Mag.,* June, 1919 (601).

[5] *Comptes Rendus,* May 25, 1920 (1245).

[6] *Sitz. Ber. Bayr. Akad. Wiss.,* June, 1918.

[7] Since this explanation was presented to the Physical Society in February, papers by W. Kossel and A. Sommerfeld (*Zeitschrift für Physik,* Jan., 1920) have reached us. In these papers the same explanation for the discrepancy is discussed as that here put forward.

STUDIES OF MAGNITUDE IN STAR CLUSTERS, XII. SUMMARY OF A PHOTOMETRIC INVESTIGATION OF THE GLOBULAR SYSTEM MESSIER 3[1]

By Harlow Shapley and Helen N. Davis

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Communicated by G. E. Hale, June 24, 1920

1. The study at Mount Wilson of the distances and structure of stellar clusters is of necessity based upon observations extensive in number and considerably varied in character. When general and comprehensive results have been obtained for a few typical systems, it becomes possible to interpret more accurately the less detailed observations on other clusters, and to place greater confidence in the hypothesis of the galactic system that has developed from the investigations of cluster and variable stars.

For two open clusters, Messier 67 and 11, and for two globular clusters, Messier 13 and 3, fairly complete surveys of the photographic and photovisual magnitudes of the individual stars have now been carried through and published in some detail; for about forty other systems the photometric and spectral investigations have been of a special character, and, in publishing the results, the observational data have for the most part only been summarized.

2. The catalogue of magnitudes and colors of 848 stars in Messier 3, which is appearing as *Mount Wilson Contribution* No. 176,[2] was begun in 1915. Fourteen photographs, made at the primary focus of the 60-inch telescope, have been used for the ten thousand measures of magnitude involved in the investigation. Special care was taken to look into the

accidental and systematic errors; the average probable error of a magnitude is about 0.04. Since the distance of Messier 3 appears to be known with higher accuracy than that of any other stellar cluster, we have in this photometric catalogue a useful collection of data on the *absolute* brightness of stars—data that will become still more valuable as spectral observations are extended.

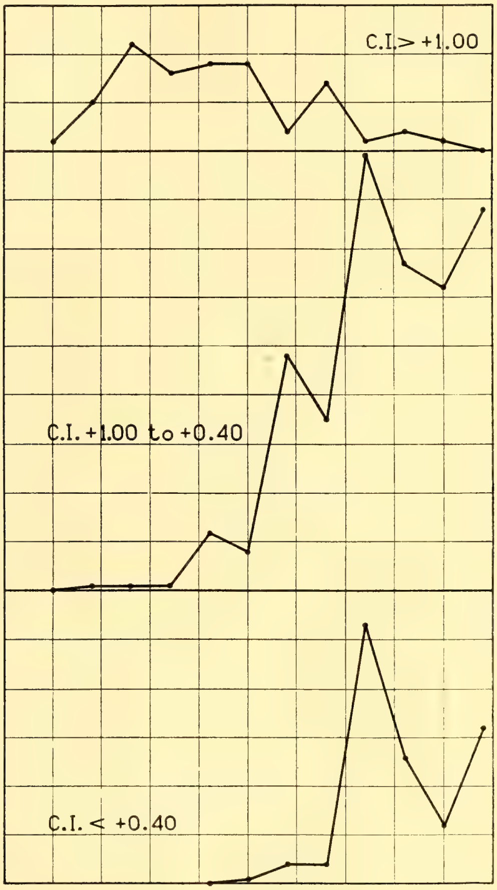

FIG. 1

Frequency of absolute photovisual magnitudes (abscissae) in Messier 3. Upper, middle, and lower curves are for red, yellow, and blue stars, respectively. Ordinates are numbers of stars for intervals of 0.4 in absolute magnitude.

3. Von Zeipel's catalogue of positions[3] is used as the basis of the system of coördinates and of the system of numbering. For 340 stars fainter than the limit of his catalogue approximate positions have been determined from Mount Wilson plates.

The magnitudes of stars within 1′.8 of the center have not been measured. Outside this crowded central region the catalogue includes practically all cluster stars to the apparent photovisual magnitude 16.8, and it is nearly complete to the seventeenth magnitude. Excluding questionable values, and also results for 64 stars fainter than pv. mag. 17, we have 750 magnitudes available for statistical discussion.

4. By accepting the parallax of the cluster as 0″.000072 (*Mount Wilson Contribution* No. 152), zero absolute magnitude is found to correspond to apparent magnitude 15.72. The frequency of stars may be determined throughout the interval of absolute photovisual magnitude from the brightest star in the cluster, −3.5, to a little fainter than +1.0. All the stars studied, therefore, are giants, the faintest ones catalogued being some twenty times as bright as the sun.

5. The frequency curves of absolute magnitude, which are graphs of the so-called luminosity law, are shown in the accompanying figure for red, yellow, and blue stars, separately. The curves are comparable with the combined results for three clusters in figure 4 of the preceding communication of the series,[4] except that in the present case a fainter magnitude is reached.

The preliminary maximum in the curve for blue stars (and less distinctly for yellow stars) at absolute magnitude −0.3 shows the highly significant excess of stars at the median magnitude for cluster-type variables.[5] The variety and lack of symmetry in these luminosity curves, and the certain existence of preliminary maxima, suggest the impracticability of using a small fragment of the general luminosity curve (all colors combined) as a means of estimating the distances of globular clusters—a procedure recently followed by Schouten and by Coeberg with surprizing results.

6. No conspicuous dependence of mean brightness or color on distance from the centre is found in Messier 3. The characteristic relation of mean color to absolute brightness appears, much as in figure 3 of the communication referred to above; and the maximum frequency of color index falls between +0.4 and +0.8, quite in agreement with the earlier results for Messier 13.

7. A separate investigation of the color curves and the constancy of period is under way for the numerous Cepheid variables in Messier 3. It will include an examination of the small light variations that appear in a number of stars very similar in color and absolute brightness to the typical cluster-type Cepheid, but not heretofore recognized as variable.

8. The apparent visual magnitude of the cluster as a whole is 6.6. The integrated absolute magnitude is, therefore, $6.6 - 15.7 = -9.1$, which is equivalent to 360,000 times the light emission of the sun, or to a total energy of radiation of 1.4×10^{39} ergs a second.

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[1] Messier 3 = G. C. 3636 = N. G. C. 5272; R. A. = $13^h\ 37^m\ 35^s$, Decl. = +28° 52′ 56″, 1900; galactic longitude = 8°, galactic latitude = +77°.

[2] Shapley, Harlow and Davis, Helen N., *Astrophys. J., Chicago, Ill.*, **51**, 1920 (140–178).

[3] Von Zeipel, H., *Annales de l'Observatoire de Paris, Memoires*, **25**, 1908 (F1–F101).

[4] Shapley, Harlow, These PROCEEDINGS, **6**, 1920 (293–300).

[5] *Ibid., Mt. Wilson Contribution* No. 155, 1917 (1–12); No. 175, 1919 (1–13), p. 10.

THE INFLUENCE OF ENVIRONMENT UPON DEVELOPMENT

BY FRANZ BOAS

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Read before the Academy, April 27, 1920

During the last ten years I have carried on a number of investigations on the relative influences of heredity and environment upon human types. On the whole the results show that each hereditary type can be considered as stable only in a stable environment, and that with a change of environment, many of the characteristic features of the body undergo changes. These results have been corroborated later on by investigations on immigrants in Boston and in certain respects also by Dr. Hrdlička's observations on Americans whose ancestors have been residents of this continent for several generations.

Ever since Gould and Baxter's investigations on the soldiers enlisted during the war of the Rebellion and since Bowditch's investigations of Boston school children, it has been known that the bulk of the body differs not only according to descent, but also according to social position of the child. The children of the poor are not so tall as the children of the well-to-do. We have, however, never been able to establish definitely whether children who are small for their ages will also be small as adults, or whether a compensatory growth occurs during the period of adolescence. I have shown in previous investigations that children who are retarded will at a later time have a much more rapid growth than children who in the early years of their lives are accelerated. This observation has been confirmed by a study of material which I placed at the disposal of Dr. Wissler. At that time, however, we were not in a position to extend these observations up to the adult stage.

Through the courtesy of Mr. Wilson Farrand, Headmaster of the Newark Academy, I have been able to collect observations on about 120 individuals whose measurements extend from the 11th year to adult life. The study of this series shows conclusively that from 11 years on, a short child will also be on the average short as an adult, while a tall child will be tall as an adult. The absolute amount of increment for a short child, from the age of 11 years up is, however, considerably larger than the total increment for a tall child of the same age, so that the previous observation may be extended for the whole period of growth. It seems likely that the relation between initial stature and increment will be much slighter for earlier years, but no material is available relating to this question. A more detailed study of the relation between increment and stature shows that the increment cannot be represented as proportional to the deviation of stature from the norm, but that a more complex relation prevails. This is evidently due to the fact that at a given age there are, among short individuals, a considerable number who are retarded in development and that the number of those retarded decreases rapidly with the increase of stature so that among tall children of a given age those of accelerated development prevail. Since the increments of stature from ten years on to the adult stage cannot be expressed by a linear equation dependent upon age, it follows that the correlation between increment and adult stature as dependent upon initial stature can also not be expressed by a linear equation.

In other words we have to consider two factors. On the one hand there is the effect of retardation and acceleration which is most prominent in children of low and of high statures respectively and which results in a high increment for short children and a low increment for tall children and on the other hand there is the effect of hereditary stature which results in a low increment for short children of normal rate of development and in a high increment for tall children of normal rate of development.

It would be unreasonable to assume that among individuals who by heredity would grow to be tall, there should be a few only who would be retarded; since the causes of retardation are present in the whole socially uniform group which has been studied. The children are all children of well-to-do parents, well-nourished and living in a fairly open country, attending a school in which their health is carefully looked after. The retardation must, therefore, be due partly to an hereditary determination of the period of development; partly to individual pathological or semi-pathological conditions, which are not likely to be dependent upon hereditary stature. If then we find that those individuals who are short as adults, show from 11 and 12 years on a considerable degree of retardation, this can only be explained by the fact that the retardation has a permanent effect upon the development of the bulk of the body. Considering these conditions we must conclude that the loss of stature due to retardation in early childhood is not made up during the period of adolescence. In other words, that the ultimate stature is an effect of both hereditary and environmental causes.

I have investigated the same question with a different kind of material. I had an opportunity to examine a large series of Jewish children in New York City, living under different conditions. One group were students in a private school, the children of well to do parents; the second lived under normal family conditions on the East Side of New York City; the third group were boarded children under the care of charity organizations; the fourth group were placed in an educational institution in the open country and organized on the cottage system, being placed in houses in groups of 30 to a house. The fifth group were children who were brought up in large institutions located in the city each institution containing more than 1000 children. A comparison of the children in the private school with all the other groups brought out very great differences. I have not been able to obtain adequate material covering the whole period of growth, but at the ages from 11–13 years the differences in both males and females are very considerable indeed. Judging from the observations made by Jacobs in London, it is safe to assume that these differences would be found to continue up to the adult stage.

In groups of children under public care continuous records were available and a comparison of the measurements of the various series of children measured on the date of commitment to public care showed that these series and that of children in normal East Side homes are strictly comparable. The statures and weights of individuals committed to a large institution, to an institution run on the cottage system and those placed in families under the care of charity organizations equaled the development of normal children from the East Side. When, however, the development of children in each group was examined, it was found that the institutionalized children lagged behind regularly. There is no doubt that the general conditions of nutrition in the large institutions, are as good as may be expected, but the whole institutional atmosphere has the effect of retarding development. Examination of adults who have passed through the asylums showed a similar effect. The average stature of adult men and women belonging to this group is about 2 cm. less than that of normal individuals of the East Side. The results obtained in an institution run on the cottage system are more favorable. While the average statures and weights are not quite up to normal, the differences are very slight. Children who are boarded in families, are in every respect comparable to the normal Jewish child in East Side families. There is only one exception to this rule. Children who are excluded from one of the institutions referred to and who are boarded in families are considerably less favorably developed than institutional children. This, however, is obviously due to the method of selection, because only children with pronounced pathological conditions are boarded out by the institution in question.

I was interested in the question whether the differences in development are due to physiological retardation and, partly for this reason, partly on account of our lack of knowledge of conditions of dentition, I collected extensive data on the eruption of teeth among the children here referred to. The observations were made by Dr. Milo Hellman to whose interest in the investigation I am greatly indebted. I have calculated before from data at hand what seems the most probable time of eruption for different teeth. In 1915, I collected similar material in Porto Rico which was elaborated by Mr. Leslie Spier. Other material was placed at my disposal by Dr. Carl Roese of Dresden, whose investigations on the relation between anthropological form and the condition of teeth are of fundamental importance, and finally I used a large series of casts found in the offices of orthodontists, for which series I am obliged to the good offices of Dr. Hellman. The series previously published by me suggested that the difference in period which characterizes the development of the two sexes, is not found in the development of the teeth. As I have pointed out before, the development of the head shows, that a sexual difference amounting to about $1^1/_2$ years in the rate of physiological development occurs about the fifth year, and we know that this difference increases to a little more than two years at the time of maturity. The time of the eruption of the permanent canines and of the premolars is between the tenth and eleventh years. During this time the normal difference in regard to physiological development between the two sexes is approximately a year and one-half, perhaps a little more. For the teeth, however, we obtain a difference of less than $^1/_2$ a year, which shows clearly that the dentition of males and of females must possess definite secondary sexual characteristics. An investigation of the size of the teeth does not bring out any very striking differences. E. Mühlreiter (*Anatomie des menschlichen Gebisses*, Leipzig 1891, pp. 128, 129) in his investigations of the teeth, has suggested that there are characteristic differences in form. Further investigations, however, will be necessary in order to show whether the physiological difference in development is accompanied by a morphological difference in form.

I expected that, if there should be physical retardation, we should find among the children of the well-to-do an early development of the permanent teeth and among the children of the poor, a later development. However, just the reverse was found. The eruption of permanent teeth among institutionalized children showed an acceleration of approximately eight months. I think this puzzling phenomenon may be explained by the fact that in the dental care of institutional children there is a strong tendency to remove the deciduous teeth as soon as they show decay, and that the removal of the teeth acts as a stimulus upon the development of the permanent teeth. Possibly the conditions in Porto Rico, where we also find an unusually early eruption of permanent teeth combined with a marked retardation in the development of stature, may be explained in a similar way; namely, by early decay of deciduous teeth which brings about an early loss of the teeth. I am still engaged in an investigation of the eruption of the deciduous teeth in relation to bulk of body and I hope to report on this subject at a later time.

The evidence that may be derived from a study of stature and weight alone indicates that the under-development of the children in charitable institutions is due to retardation.

On the whole the observations here reported confirm the conclusion that environmental conditions play an important part in the determination of the bodily form of the adult. It is obvious, therefore, that explanations that take into account only hereditary causes cannot satisfactorily account for the observed phenomena. This is particularly true of stature and weight, which appear extremely variable in the same lines of descent, according to the environmental conditions in which the individuals live.

THE POSSIBILITIES OF THE ROCKET IN WEATHER FORECASTING

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Communicated by C. G. Abbot. Read before the Academy, April 27, 1920

Most Desirable Conditions for Obtaining High Altitude Data for Weather Forecasting.—It is well understood that the pressure, temperature, wind velocity, and moisture content, which obtain at the top of the troposphere, i.e., at the 10 km. level, would be of much importance in weather forecasting; making possible the prediction of surface conditions many miles distant from the place of observation.

The data would obviously be of greatest value if obtained simultaneously at a number of separated stations. If this were done, an accurate weather map, representing conditions at a definite high elevation could be made, and compared with that representing surface conditions. Such a high altitude weather map would also be of obvious importance in aviation.

It is evident that the time of ascent should be as short as possible, not only in order that the data for the various stations should be obtained simultaneously, but also in order that drifting by the wind, and the consequent difficulty of recovery of the apparatus, be reduced to a minimum. The descent should also be as rapid as possible, for the same reasons, with proper arrangements to prevent damage on landing.

It would also be desirable, although not essential, that the instruments remain at, or near, the 10 km. level for from one to five minutes.

In short, then, the most desirable method of obtaining high altitude data for weather forecasting, would consist in the sending of instruments to the 10 km. level daily, from a number of stations, the ascent and descent being as rapid as practicable; provision further being made, if desirable, for maintaining the instruments at this level during an appreciable interval of time.

The Rocket Method as a Means of Realizing These Ideal Conditions.—Although the ordinary rocket has a vertical range of but a few hundred feet, the rocket should, in principle, be capable of reaching much greater altitudes.

A theory, together with estimates as to what should be attainable under realizable conditions, is given in *Smithsonian Miscellaneous Collections*, Vol. 71, No. 2. It is intuitively evident, however, that a great elevation must be obtainable provided a large part of the heat energy of the propellant is converted into kinetic energy of the ejected gases, and also provided the proportion of mass of propellant to total mass is high.

It should be understood that although theory indicates the possibility of reaching great altitudes, the application discussed in the present paper is solely the raising of recording instruments to a moderate height.

As regards the conditions to be satisfied by the most desirable method above outlined, other than the mere attainment of the altitude, the rocket method is ideally suited to the raising of apparatus rapidly and without jar, inasmuch as the propulsive force is sensibly constant, and, therefore, the apparatus, starting from rest, will rapidly attain a high velocity. After the propellant has been completely expelled, the apparatus will gradually be brought to rest by gravity. A parachute device, carried by the rocket, could permit of any desired speed of descent; the details of construction and operation being, of course, well understood.

Concerning the maintenance of high level for an appreciable interval of time, the rocket apparatus can contain, besides instruments and parachute, a rubber balloon of the usual size, compactly folded, together with a steel sphere containing compressed hydrogen, to be released into the balloon when the upper limit of flight has been reached, as suggested in principle by Dr. Abbot. The parachute would be brought into action, and the balloon released, a predetermined interval of time after the balloon had been inflated.

Extent to Which the Conditions Necessary for a Satisfactory Rocket Method have been Realized.—The first condition for increasing the range of the rocket apparatus is the ejection of the powder gases with as high a velocity as possible. This feature has already been developed to a satisfactory degree. The velocity of the ejected gases has been increased from 1000 ft./sec. to over 7,500 ft./sec.; or, expressed differently, the fraction of the heat energy of the powder that is transformed into energy of motion of the ejected gases has been increased from one-fiftieth to over a half; the propellant being a dense smokeless powder. It is important that the velocity of ejection be large, as it enters exponentially in the expression for the initial mass necessary to propel a rocket to any given height.

Regarding the second condition for a great range, namely, the employment of a large proportion of weight of propellant to total weight, it should be understood that this proportion need not be large for a range as low as 10 km. It is necessary to employ the principle of multiple charges, as explained in the above Smithsonian publication, if the proportion is to exceed the value for ordinary rockets, which is about $^1/_5$. This action consists in the loading and firing of a number of charges successively in the same combustion chamber.

The results of work upon this feature, to date, have been the development and experimental demonstration of a simple and light multiple charge apparatus, firing a few cartridges and travelling straight. In order to complete the development, it is necessary to adapt the apparatus to fire a large number of cartridges, and to make the parts, exclusive of propellant, sufficiently light. Work on increasing the number of cartridges is in progress.

In order to complete the development with a minimum of expense, the perfecting of details should be carried out only insofar as is necessary in order to produce an inexpensive apparatus. The only expense of maintenance will be a new magazine for each ascent.

In any case, the time required to reach the 10 km. level should be of the order of 20 seconds, if the retardation due to air resistance and gravity is minimized.

As an illustration of what should be possible with an apparatus developed in this way, it may be said that, using as a basis for the estimate a velocity of ejection of 5,500 ft./sec., which is easily obtained, a rocket weighing of the order of 11 lbs. initially and 6 lbs. at the highest point would be needed in order to send instruments weighing one pound to the 10 km. level.

NOTE ON A PNEUMATIC METHOD OF MEASURING VARIATIONS OF THE ACCELERATION OF GRAVITY[1]

By CARL BARUS

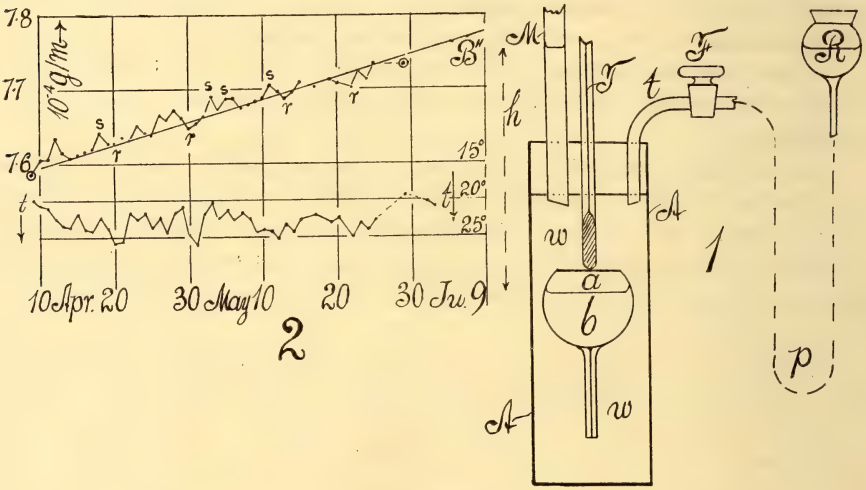
DEPARTMENT OF PHYSICS, BROWN UNIVERSITY

Communicated June 22, 1920

1. *Introductory.*—Some years ago I made an extended series of experiments[2] on the diffusion of gases through water; the gas in this work was imprisoned in a cartesian diver, and the very sensitive conditions under which the diver floats at a given level were made the criterion of measurement.

Inasmuch as such experiments consist virtually of a comparison of weights with the forces derived from air pressures, it must, therefore, be possible to obtain the acceleration of gravity in terms of these pressures, just as in another place I shall describe interferometer experiments made to evaluate the changes of g in terms of torsion. It will not, of course, be possible to determine g absolutely in this way because so little air is used; but it was hoped that the changes of g, in a proper environment would be determinable with some precision. Quite apart from this specific purpose, however, the long range experimental data are of varied interest.

2. *Apparatus.*—The chief difficulty with the experiment at the outset seems to be the elimination of loss of gas by diffusion. This, in the case of a cylindrical diver, open below, amounted in the experiments cited, to 0.41% per day. It would not, therefore, be long before the whole of the gas would be lost. To avoid this, two resources suggest themselves: (1) to place the diver about midway between two layers of air, one below and the other above it; and (2) to provide the diver with a long slender neck below, precisely what was to be avoided in cases of diffusion. I shall not describe the forms of apparatus in which the first precautions were taken, as they proved to be unavailable; nor any of the correlative forms with the diver sheathed within a completely submerged cylindrical tube, nearly closed. The results so obtained were no better than those found with the extremely simple apparatus, figure 1. The swimmer being here lighter than water, *AA* is the standglass, wholly filled and *a*, *b*, the swimmer held down at a definite level by the thermometer *T*. *M* is a water manometer for registering the pressure excess at which flotation just occurs at the given level. Pressure is applied by aid of the small water reservoir, *R*, communicating with *AA* by the tube *t* with the stopcock *F*, and the flexible pipe *p*, the whole system *t F p R* being full of water. To make an observation, *R* is raised sufficiently and clamped. The cock *F*, is now slightly opened, until the swimmer just begins to sink. The head of water, *h*, is then read off at *M*. This adjustment may be made to a fraction of a millimeter of water. The apparatus admits of observation in an agitated environment.



FIGS. 1 AND 2

The diver, which to be sensitive must be as light as possible (thin stem preferable), consisted of a bulb 4.5 cm. in diameter and a stem 4 cm. long and about 2 mm. in diameter. The rate of loss would be decreased by diminishing this diameter further; but it is by no means proportional to the sectional area.

3. *Equations.*—If M is the mass of the diver, m that of the air contained at the absolute temperature τ and pressure $h + (H - \pi)\rho_m/\rho_w$ in centimeters of water (h being the incidental water head, figure 1, H the residual mercury head, and π the vapor pressure in centimeters of mercury, ρ_m, ρ_w, ρ_g, the densities of mercury, water and glass, all read off at τ), we may write for the gravitational acceleration, g:

$$g = \frac{Rm\tau}{[h + (H - \pi)\rho_m/\rho_w][(1 - \rho_w/\rho_g)M + m]}. \qquad (1)$$

The first term in the denominator is to be multiplied by ρ_w at τ, to get the water head at 4° C.; the second term to be divided by ρ_w to get the volume of the air bubble. These reductions, therefore, cancel each other. Mercury, water, and air are necessarily at the same temperature, at least in a proper installation of apparatus.

Thus it follows that variations of g are determinable with the same accuracy as τ and H. The former being of the order of 290°, 0.03° C., must be guaranteed if g is to be correct to 10^{-4}. Furthermore, H, which is over 1000 cm. of water, must be read to 1 millimeter for the same accuracy. Both of these requirements are moderate. By tapping the tables from below, the swimmer is slightly separated from the thermometer above, and the degree of flotation may thus be estimated with great precision, by the speed with which the swimmer rises again into contact. An accuracy of 0.01% in g may thus be expected, provided m is adequately constant. This paper indicates to what degree this may be insured; for in a heated room, in winter, with very variable temperatures, the test is extremely severe.

The absolute determination of g is dependent on the value of m. To measure this large swimmers would be needed such as would make the apparatus too cumbersome. With regard to small quantities or small changes of the quantities ρ_m, ρ_w, etc., equation (1) may be stripped of its less important terms and written in an approximate form. The corrections are then found by logarithmic differentiation.

4. *Observations.*—Figure 2, curve B, contains the most complete series of observations made with this apparatus, g/m in ten thousands, being shown in its variations within a lapse of somewhat less than two months. Readings were made about at noon daily and the corresponding temperatures are given (increasing downwards) in the graph marked t. Corrections for the temperatures, etc., have been applied. As the air content was too small for persistent flotation, the diver was liable to sink at low temperatures, or high barometers, or both (s, in the figure). It is under these circumstances that the irregularities of the curve are usually marked.

In the figure, a line has been drawn through a succession of points which seem to suggest it. For this g/m increases per day (owing to the decrease of m) at a mean rate of $r = 0.00039$, which is equivalent to and air loss of 5×10^{-6} grams per day. Though it was formerly my opinion that the phenomenon could be completely explained in terms of the diffusion of air through water, the cusps (s) and subsequent intervals of relapse (r) when temperature increases, again do not bear this out. The whole is more complicated. The mechanism by which the air loss is brought about is largely solutional. Air is dissolved when temperature falls and is released from solution when temperature rises again. But for this the rate of increase of g/m throughout many months is in all the results remarkably constant and could be allowed for. Only at the beginning of the experiments with a new diver, and owing probably to gas adhering to the glass walls, are these mean rates irregular.

But the solutional effect in question under rapidly varying temperatures is fatal to the purpose for which the experiments were made. Though much of the discrepancy could be removed by placing the whole apparatus in an adequately constant thermostat, the outlook is not encouraging.

[1] Advance note from a Report to the Carnegie Institution of Washington, D. C. The work was done in deference to a request of President R. S. Woodward, that a variety of methods of the kind in question be looked into.

[2] *Carnegie Publications*, No. 185, Washington, 1913.

NOTE ON TORSIONAL MEASUREMENT OF VARIATIONS OF THE ACCELERATION OF GRAVITY BY INTERFERENCE METHODS[1]

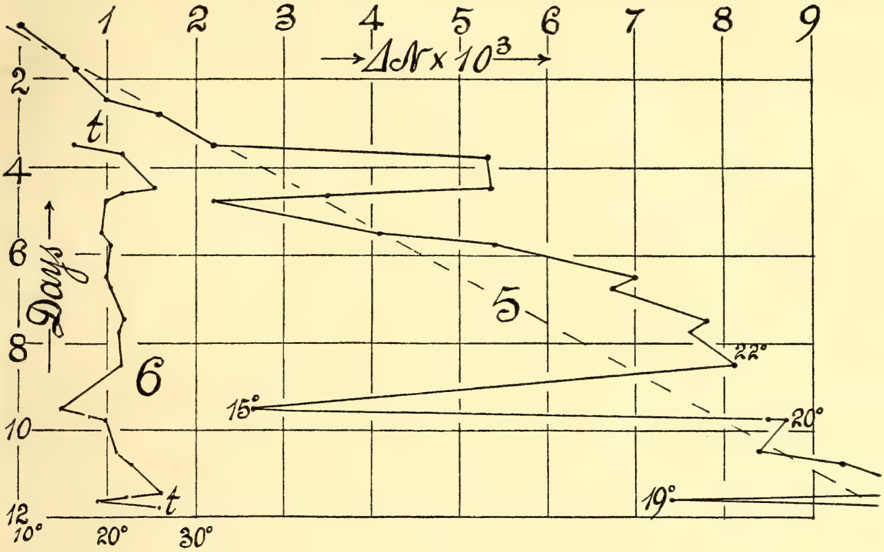
By Carl Barus

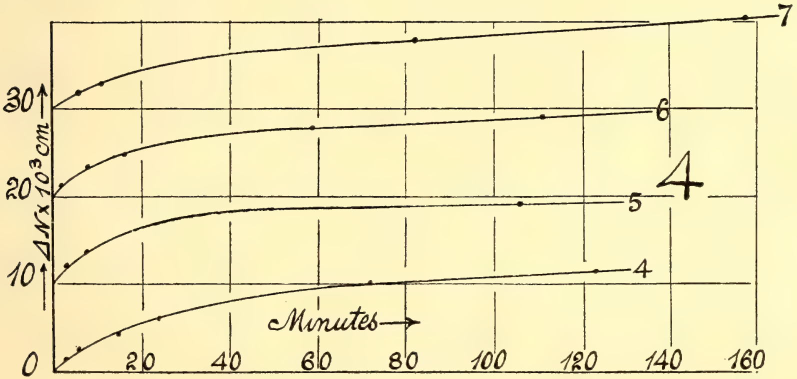
Department of Physics, Brown University

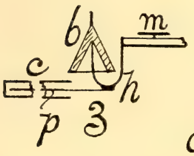
Communicated July 1, 1920

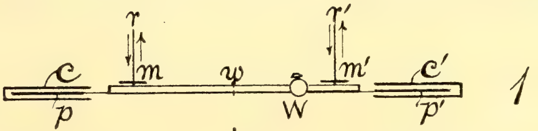
1. *Apparatus.*—These experiments were undertaken to find in how far interferometer methods might contribute to the measurement of changes of g, under conditions in which the pendulum is inapplicable. The apparatus as a whole is a horizontal torsion balance with the deflection readable in terms of the displacement of the achromatic interference fringes. The method developed would be applicable in case of any ordinary chemical balance. The interferometer described admits of moderately rough handling; but it requires a level base, at least during observation. The mirrors are adjusted normally to a vertical plane, as the rays are in this direction.

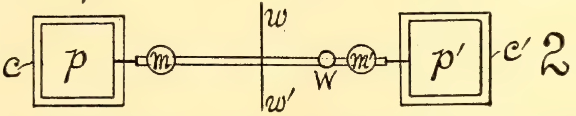
Torsional weighing, by passing the counterpoise from pan to pan, is in itself a delicate operation when fringes are used. Moreover, the variation of viscosity is particularly large at the beginning. Nevertheless my first observations were made with such an apparatus; and adequate air damping was secured by hanging the light scale pans in dash pots. Readings were made on a large graduated circle (torsion head), the stops of which were micrometer tangent screws. Changes of twist registered by the interference fringes were evaluated either by the latter, or by the micrometer screw of the rectangular interferometer.











If the weight remains on one side, viscous deformation not only grows indefinitely smaller and is subject to simpler equational conditions, but the apparatus itself may be made less cumbersome and steadier. Thus in figures 1 (elevation) and 2 (plan), r and r' are the two light rays of the interferometer. If w is the wire stretched between torsion heads (counterpoise W) and running normally to the balance beam, m, m', the auxiliary mirrors of the interferometer attached to the beam, the latter may be prolonged and terminate in the thin metallic plates, p, p'. These are surrounded by the narrow glass cases c, c', to secure air damping. This device is very efficient; and if c and c' are each placed on three leveling screws acting upward from below, while a stout vertical spring presses the cases downward from above (not shown), it is easy to free the plates p, p', particularly as they may now also slide, with slight friction, on the leveling screws. Hence, as the beam is practically aperiodic, on putting a wide brass plate underneath the system $p\ m\ m'\ p'$ and close to it, no further casing is needed. The beam is thus easily accessible without opening the doors of a case. The fringes occasionally move, but they at once return to their equilibrium position, which is thus easily recognized. I tested this by removing the fixed counterpoise W, and proceeding with the method of weighing by exchanges. For this reason stiff hooks, h, figure 3, were fastened rigidly at each end of the beam, m. The plates p, p', in c, c', were soldered to the bottom of h as shown. The counterpoising weight, b, was bell shaped and reposed on the sharp point of the hook, h. A small projection on top allowed its easy removal with forceps. Adjusting the stops of the torsion head of the wire w, the bell weight b could be passed (with the requisite torsion) from one end of the beam to the other, without any inconvenience, the fringes in either case of adjustment appearing at once, in their proper position relative to the cross wire at the slit of the collimator. Of course, h must be stout enough to be free from flexure, relative to b.

Observations. Twists Alternately Opposite.—If the wire is too thin, the fringes are liable to wander. This ceases entirely with a wire 0.05 cm. thick, and about 35 cm. long, one-half of which is effective. It was of steel, hard drawn, but annealed to a blue temper to diminish the molecular instability. A weight of the order of 1 gram was selected and the double twist produced by this was $\Delta\theta = 50°$. The weight was passed from end to end of the beam, counterpoised by torsion, without difficulty and the fringes set to coincide with the shadow of the cross wire on the slit. Great care had to be taken to avoid twisting the frame carrying the wire and torsion heads.

The constants of the interferometer being, angle of rays to mirrors, $i = 45°$, breadth of the ray parallelogram, $b = 10$ cm., micrometer displacement ΔN, we may write $\delta g/g = \delta\theta/\theta = \Delta N \cos i/b(\Delta\theta/2) = 0.16\,\Delta N$ and as ΔN may be read to within 10^{-4} cm., the sensitiveness is about 10^{-5}, the gram counterpoise in question presupposed. The error to be apprehended from viscosity may be estimated from the following observations, figure 4, of the yield of the wire (ΔN, vertical) with the gram excess at the end of the balance beam. The graphs shown are for the 4th to the 7th twists, alternately in opposite directions. The others, eleven in all, were similar. The curves show that if measurement is made within 3 minutes after twisting, the error from viscosity, if ignored, would not exceed $\delta g/g = 3 \times 10^{-4}$. If, however, the coefficient of viscosity is known and allowed for, the error of 3×10^{-5} is improbable.

If the graphs be used to compute the absolute viscosity of the metal for the given twist and diameter, it is found to increase in 100 minutes from about 4×10^{19} to 3×10^{20} and thereafter is nearly constant. The twist was about 1.4° per linear centimeter; but in alternating, the effect of this is about doubled.

Observations on the Permanently Twisted Wire.—The readings were made two or three times daily, as a rule, for over a month. They were adequately recorded in graphs like figure 5, for example, figure 6 being the corresponding temperature graph. As the twist produced by the excess of weight of about 1 gram was here also about 25°, we have $\delta g/g = 1.6 \times 10^{-4}$ for each of the divisions (10^{-3} cm.) of the scale. The laboratory temperature was very variable from day to day; and the effect of this on the graph is at first sight disconcerting, though it contributes essentially to the interpretation. In figure 5 the initial mean rate of yield referable to viscosity, may be estimated as $\Delta N = 0.0009$ cm. per day. This makes $\delta g/g$ vary 0.00014 per day, a quantity in itself too large to be used as a correction, with confidence; after a month, however, this nearly vanishes, but a much more serious consideration, the thermal variation of rigidity, remains. It is this feature which makes the graph so exceedingly jagged. If we take the large drop between the 8th and the 10th day to estimate this effect, the data would be: $t = 22°, 15°, 20°$; $10^3 \Delta N = 8.1, 2.6, 8.7$ cm., which is equivalent to $\Delta N/\Delta t = 10^{-3}$ cm. per degree; or $\delta g/g = 2 \times 10^{-4}$, nearly, for each degree of fall of temperature promoting increased rigidity. Excellent observations on the torsional rigidity of iron and steel are given by Pisati (Landolt and Boernstein's tables). The coefficients are 2.1×10^{-4} for iron, and 2.3×10^{-4} for steel, therefore, even larger than the graph indicates. In fact, the present interferometer method could easily be modified to measure this constant accurately.

Summary.—In the above experiments as a whole, there is thus very little promise of arriving at the variations of g, with precision, by using torsional apparatus, so long as there is any marked change in the temperature of the environment. The discrepancy of the method is to be referred less to the viscosity of the metal and its thermal coefficient, than to the decrease of rigidity with rising temperature.

There is, however, one possible way out of the difficulty and this points to the use of the alloys which Director C. E. Guillaume, of the Bureau International at Sévres, has spent so much time in perfecting. I shall hope to report observation with these in the future.

[1] Advance note from a Report to the Carnegie Institution of Washington, D. C.

THE GENUS BOTRYCHIUM AND ITS RELATIONSHIPS

BY D. H. CAMPBELL

DEPARTMENT OF BOTANY, LELAND STANFORD JR. UNIVERSITY

Read before the Academy, April 27, 1920

The genus *Botrychium* comprises about thirty-five species of almost world wide distribution. Up to the present our knowledge of the life history of these ferns has been confined almost exclusively to two species, *B. lunaria* (L.) Sw., and *B. Virginianum* (L.) Sw.

In 1910, Dr. H. L. Lyon[1] published a brief account of the embryo of *B. obliquum* Mühl, which differed so much from the two species mentioned, that he proposed to separate *B. obliquum* as the type of a new genus, *Sceptridium*.

Dr. Lyon made quite complete collections of material of this species, and also prepared series of microtome sections of the gametophyte and young sporophyte. This material, as well as similar material of some other species, was collected in the vicinity of Minneapolis.

Dr. Lyon very generously turned over this valuable material to me, so that I have been able to make a fairly complete study of the early development of this interesting species.

The gametophyte of *B. obliquum*, like that of the other species, is subterranean. It closely resembles in its general structure that of *B. lunaria*, and *B. Virginianum*, and in size is intermediate between them.

The mature gametophyte is a somewhat flattened tuber, 3–6 mm. in length and about half as wide. Upon the upper side is a more or less conspicuous median ridge upon which the reproductive organs, archegonia and antheridia are borne. These closely resemble those of the other species, but the spermatozoids are considerably larger.

The embryo differs remarkably from that of the other species. A very conspicuous suspensor is developed, which is wanting in the other species, and the relations of the first leaf and root are different.

The fertilized egg becomes much elongated, and forces its way downward into the tissues of the gametophyte before any cell-division occurs. The first division is transverse, and cuts off a large basal cell, which becomes the suspensor. The smaller terminal cell gives rise to the organs of the embryo-sporophyte.

The further development of the embryo is more like that of the Marattiales, especially Danaea, than it is like the other species of Botrychium.

Danaea also develops a suspensor. The relative positions of the first leaf and primary root, also recall the embryo of some species of Ophioglossum.

As in Danaea and Ophioglossum, the young sporophyte of *B. obliquum* is bipolar in structure, the primary leaf or cotyledon and the primary root having a common axis traversed by a single vascular bundle which is continued from one into the other. The root is endogenous in origin, and pushes its way vertically downward, penetrating the foot which is practically destroyed. The stem apex is very inconspicuous, and is situated between the cotyledon and the primary root. No stelar tissue is developed from the stem-apex, and the vascular system of the young sporophyte is made up exclusively of a union of vascular strands belonging to the leaves and roots.

The primary leaf is ternate in outline, and closely resembles the first foliage leaf of Helminthostachys.

The embryo of *B. obliquum* differs from that of *B. lunaria* in several important particulars. The embryo of the latter is in its early stages composed almost entirely of the foot and root between which is the very inconspicuous stem-apex. The early leaves are reduced to mere scales, and it is several years before the first green leaf is formed.

B. Virginianum, on the other hand, has the primary leaf even better developed than it is in *B. obliquum*, but the root is of external origin, the very large foot is persistent, and the cotyledon makes a sharp angle with the root.

B. obliquum agrees with Helminthostachys in the development of a suspensor, but the position of the organs of the embryo in the latter is more like *B. Virginianum*. In Helminthostachys, also, the first leaf is rudimentary. The second leaf is functional and closely resembles that of *B. obliquum*.

It looks as if *B. obliquum* were more generalized than the other species, suggesting a relationship on one hand with Ophioglossum, on the other approaching Helminthostachys, as well as the Marattiaceae, especially Danaea.

The differences in the embryos of the three investigated species of Botrychium, are accompanied by significant differences in the adult sporophytes, probably sufficient, together, to warrant a separation of the genus into three genera.

The "Ternatum" division of the genus Botrychium to which *B. obliquum* belongs, would probably constitute the genus Sceptridium, Lyon; Blunaria, the type species, and its allies, would retain the generic name Botrychium Sw., while *B. Virginianum* would be the type of Osmundopteris, Milde.

[1] A New Genus of Ophiolglossaceae, *Bot. Gaz., Chicago*, **40**, 1905 (455).

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CONTENTS

Page

PETROLOGY.—ON CHONDRULES AND CHONDRITIC STRUCTURE IN METEORITES.... *By George P. Merrill* 449

PHYSICS.—THE VERTICAL INTERFEROMETER........*By A. A. Michelson*...... 473

PHYSICS.—ON THE APPLICATION OF INTERFERENCE METHODS TO ASTRONOMICAL MEASUREMENTS..............................*By A. A. Michelson*.... 474

PHYSICS.—A MODIFICATION OF THE REVOLVING MIRROR METHOD FOR MEASURING THE VELOCITY OF LIGHT............................*By A. A. Michelson* 476

PHYSICS.—ON THE K SERIES OF X-RAYS.................................... *By William Duane and Wilhelm Stenström*.. 477

ASTRONOMY.—STUDIES OF MAGNITUDE IN STAR CLUSTERS, XII. SUMMARY OF A PHOTOMETRIC INVESTIGATION OF THE GLOBULAR SYSTEM MESSIER 3........ *By Harold Shapley and Helen N. Davis*.. 486

ANTHROPOLOGY.—THE INFLUENCE OF ENVIRONMENT UPON DEVELOPMENT........ *By Franz Boas* 489

PHYSICS.—THE POSSIBILITIES OF THE ROCKET IN WEATHER FORECASTING........ *By Robert H. Goddard* 493

PHYSICS.—NOTE ON A PNEUMATIC METHOD OF MEASURING VARIATIONS OF THE ACCELERATION OF GRAVITY..................................*By Carl Barus* 495

PHYSICS.—NOTE ON TORSIONAL MEASUREMENT OF VARIATIONS OF THE ACCELERATION OF GRAVITY BY INTERFERENCE METHODS............*By Carl Barus* 498

BOTANY.—THE GENUS BOTRYCHIUM AND ITS RELATIONSHIPS..*By D. H. Campbell* 502

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FURTHER MEASUREMENTS OF THE EFFECT OF PRESSURE ON RESISTANCE

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In a previous paper[1] data for the effect of pressures up to 12000 kg/cm^2 on the resistance of 22 metals were given. It has now been possible to extend the results to 18 more elements, and 6 alloys. The extension has been made possible by two changes in the technique. In the first place, by a change in the method of leading electrical connections into the pressure chamber it has been possible to replace the previous method of measuring resistance with a Carey Foster bridge by a potentiometer method. This makes it possible to measure accurately specimens whose total resistance is very low, and removes the restriction that the specimen must be in the form of a fine wire. In the second place, by a modification in the design of the apparatus, it has been possible to considerably extend the temperature range. The results now cover a range from atmospheric to 12000 kg. pressure, and an extreme temperature range from 0° to 275° C.

In selecting the substances to be measured over this increased range I have paid particular attention to the matter of liquid metals. Previous to this, the effect of pressure on both the liquid and solid state was not known for a single metal. The resistance of metals in the liquid state would seem to be particularly worthy of study, because here the crystalline structure introduces no complications. Six elements have now been investigated in the liquid and the solid states. Furthermore, I have endeavored particularly to investigate some of the more unusual elements, in the expectation that elements from unusual parts of the periodic table might show new types of behavior. This attempt has been rewarded by the discovery of three more elements whose pressure coefficient of resistance is positive; bismuth and antimony were the only ones known previously.

A number of commercial alloys have been included as being of some interest because of their wide use for scientific purposes. The measurements on them had been already made for another purpose. The composition of the alloys is as follows:

Hoskins Mfg. Co.	Chromel A	Ni 80%, Cr 20%.
	Chromel B	Ni 85%, Cr 15%.
	Chromel C	Ni 64%, Cr 11%, Fe 25%.
Electrical Alloy Co.	Comet	Ni x%, Cr y%, Fe z%.
Driver Harris Co.	Therlo	Al 2%, Mn 13%, Cu 85 (this is like Manganin).
	No. 192 Alloy	Ni 30%, Cr 2%, Fe 68%.

Only a very rough summary of the principle results of the measurements can be attempted here. The results will be published in full detail elsewhere. In the table are shown the average pressure coefficients to 12000 kg., and also the initial coefficients at atmospheric pressure. The numerical values of the table may be supplemented by the following remarks on various significant aspects of behavior.

TABLE

EFFECT OF PRESSURE ON RESISTANCE

Substance	Mean Pressure Coefficient, 0–12000 kg.	Instantaneous Pressure Coefficient at 0 kg.	Substance	Mean Pressure Coefficient, 0–12000 kg.	Instantaneous Pressure Coefficient at 0 kg.
Li, solid, 0°	$+0.0_{5}772$	$+0.0_{5}68$	Bi, liquid, 275°	$-0.0_{4}101$[c]	$-0.0_{4}123$
Li, liquid, 240°	$+0.0_{5}93$	$+0.0_{5}93$	As, 0°	$-0.0_{5}33$	....
Na, solid, 0°	$-0.0_{4}345$	$-0.0_{4}663$	W, 0°	$-0.0_{5}135$	$-0.0_{5}143$
Na, liquid, 200°	$-0.0_{4}436$	$-0.0_{4}922$	La, 0°	$-0.0_{5}331$	$-0.0_{5}39$
K, solid, 25°	$-0.0_{4}604$	$-0.0_{3}186$	Nd, 0°	$-0.0_{5}213$	$-0.0_{5}238$
K, liquid, 165°	$-0.0_{4}809$[a]	$-0.0_{3}168$	Carbon, amorphous, 0°	$-0.0_{4}100$	$-0.0_{4}118$
Mg, 0°	$-0.0_{5}408$	$-0.0_{5}447$	Carbon, graphite, 0°	$+0.0_{5}47$	$+0.0_{5}77$
Ca, 0°	$+0.0_{4}106$	$+0.0_{4}129$	Si, 0°	$-0.0_{4}117$	....
Sr, 0°	$+0.0_{4}680$	$+0.0_{4}502$	Black phosphorus, 0°	$-0.0_{4}81$	$-0.0_{3}200$
Hg, solid, 0°	$-0.0_{4}236$[b]	....	Chromel A, 0°	$-0.0_{6}134$	....
Hg, liquid, 25°	$-0.0_{4}219$	$0.0_{4}334$	Chromel B, 0°	$-0.0_{6}169$	....
Ga, liquid, 30°	$-0.0_{5}531$	$-0.0_{5}640$	Chromel C, 0°	$-0.0_{6}427$	....
Ga, solid, 0°	$-0.0_{5}247$	....	Comet, 0°	$-0.0_{5}241$	$-0.0_{5}263$
Ti, 0°	$\pm 0.0_{6}1$??	....	Therlo, 0°	$-0.0_{5}228$	$-0.0_{5}236$
Zr, 0°	$-0.0_{6}40$	$-0.0_{6}40$	No. 193 alloy, 94°	$-0.0_{5}179$	....

[a] Average 0–9000 kg.

[b] Average 7640–12000 kg.

[c] Average 0–7000 kg.

With regard to purity, Ga was prepared under the direction of Professor T. W. Richards for atomic weight work, and had less than 0.01% impurity. The Na, K, Mg, Hg, Bi, W, and black phosphorus were also of high purity. The Ca contained about 0.1% impurity, and the impurity in the Li and Sr was of the order of 1%. The Ti and Zr were known to be impure with 1.8% and 0.6% of W respectively; the impurity may have been higher. Judging by the temperature coefficients of resistance, As, La, and Nd were considerably impure. The graphite was the best Acheson graphite, but the results were not reproducible. Similarly results with Si were not reproducible. All that can be expected of C and Si is the order of magnitude of the effect.

Normal Solids.—The substances with negative pressure coefficients of resistance are Na, K, Mg, Hg, Ga, Ti, Zr, As, W, La, Nd, Si, and black phosphorus. Mg and W were previously measured. It is now possible to give better values because of increased purity of the specimens available. La and Nd are the first metals in the rare earth group whose pressure coefficients have been measured; they show no novel features. Ti and Zr are also from a new region of the periodic table. The interesting feature of their behavior is the extreme smallness of the coefficient. Hg has not been previously measured in the solid state; its coefficient is somewhat greater than that of the liquid. Abnormal results were expected for gallium, because it expands when it freezes, but it was found instead to be quite normal. Arsenic might be anticipated to be abnormal because of its position in the periodic table relative to bismuth and antimony, but it turns out to be normal. Silicon, a non-metallic element, decreases in resistance, as is normal for metals, but the pressure coefficient becomes larger with increasing pressure and the temperature coefficient may reverse in sign at high pressures, both of which are abnormal features. Black phosphorus, also non-metallic, is remarkable for the very large size of the effect, the resistance decreasing under 12000 kg. to about 3% of its initial value. The relative coefficient, however, does not change so much as it does for some metals. Na and K are the first alkali metals whose pressure coefficients have been measured. They are remarkable for the largeness of the effect, which is larger than for any other metals as yet measured. Na decreases 40% and K 70% in resistance under 12000 kg. The pressure coefficient of these metals decreases greatly with increasing pressure and increases with increasing temperature, and the temperature coefficient decreases with increasing pressure. The metals previously measured have shown relatively little change in these coefficients.

Abnormal Solids.—Three new elements have been found whose resistance increases under pressure; these are Li, Ca, and Sr. This was a great surprise because all of these metals are highly compressible. With the single exception of the variation with pressure of the pressure coefficient of strontium, the behavior of these three metals is like that of Bi and Sb in that the instantaneous pressure coefficient increases with increasing pressure and decreases with increasing temperature, and the temperature coefficient falls with rising pressure.

Relative behavior of Resistance of Solid and Liquid.—The pressure coefficient of liquid bismuth is found to be negative and normal, although that of the solid is positive and abnormal. This points to the importance of crystalline structure in determining the variations of resistance. On the other hand the coefficient of lithium is abnormal in being positive in both the liquid and solid states. (The melting curve of Li was measured under pressure and found to be normal in that the solid expands on melting.) The magnitude of the coefficient of the solid is less than that of the liquid. Gallium is normal in the solid as well as in the liquid, and the coefficient of the liquid is much larger than that of the solid. The coefficient of liquid mercury is slightly less than that of solid mercury. The coefficients of liquid sodium and potassium are less than those of the solids, but by only small amounts. The relative decrease in the coefficients of these solids with pressure may be greater than the decrease of the liquid. Liquid potassium has an abnornal change in the sign of the variation of temperature coefficient with pressure and the pressure coefficient with temperature.

All of the six metals measured in the solid and liquid state agreed in showing a relatively small change in the ratio of the resistance of liquid to solid along the melting curve from low to high pressure. Thus the change in the ratio of the resistance of liquid to solid potassium was from 1.56 to 1.55 under a pressure increase of 9700 kg. This pressure is sufficient to decrease the difference of volume between liquid and solid to 0.31 of its initial value. It seems to be universally true that the temperature coefficient of the liquid is less than that of the solid, and the change of resistance on melting is in the direction of the change of volume.

Theoretical Implications.—The following remarks are only two of many that might be suggested by the data.

It is probable, because of the peculiar nature of the atomic structure of lithium and the fact that its pressure coefficient of resistance is positive, that the picture of the mechanism of electrical conduction given by Wien and Lindemann as a space lattice of electrons sliding in the channels of a space lattice of atoms may have much of truth for this particular element.

The new evidence now at hand does not make it necessary to modify the former point of view[2] that for most elements the most important single factor in determining the variations of electrical resistance is the amplitude of atomic vibration.

This investigation was assisted in large part by a generous grant from the Rumford Fund of the American Academy of Arts and Sciences.

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[1] Bridgman, P. W., *Proc. Amer. Acad., Boston,* **52,** 1917, (573–646).

[2] Bridgman, P. W., *Physic. Rev., Ithaca, N. Y.,* **9,** 1917 (269–289).

CHARACTERISTIC ABSORPTION OF X-RAYS: L SERIES

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Communicated August 2, 1902

Introduction.—Critical absorption wave-lengths associated with the L series of X-rays have been measured by E. Wagner[1] and M. de Broglie.[2] These scientists have investigated the spectra of ten of the chemical elements from tungsten to uranium, both inclusive. Each employed a photographic X-ray spectrometer with a rocksalt crystal. M. de Broglie found two critical absorption wave-lengths characteristic of each chemical element except, in the case of mercury, for which he gives only one. For bismuth, thorium and uranium he found a third, weak band.

In the investigation reported in this note we employed an ionization spectrometer with a calcite crystal. We found three critical absorption wave-lengths characteristic of each of the nine elements examined.

A brief discussion of the bearing of this new data on certain empirical laws and on recent theories of the mechanism of X-radiation may be found in the concluding paragraphs.

Apparatus and Method of Measurement.—A detailed description of the ionization spectrometer and the method of using it so as to obviate certain errors of measurement has been given in previous articles.[3] These articles also describe the plant for generating and controlling the X-radiation. Two X-ray tubes have been employed, each equipped with a Coolidge cathode and with a thin glass window blown in a side arm. One of these tubes contained a tungsten and the other a molybdenum target. The current exciting the tubes came from a high tension storage battery. The ionization chamber contained ethyl-bromide or methyl-iodide, and sometimes a mixture of both.

In measuring a critical absorption wave-length we place a thin sheet of the chemical element to be investigated (or of a salt containing it) in the path of the beam of rays between the X-ray tube and the first slit of the spectrometer. We then measure the ionization currents for a series of positions of the crystal corresponding to wave-lengths in the neighborhood of the critical values. From these measurements we platt curves representing the ionization current as a function of the crystal table angle (see figs. 1 and 2). At a critical absorption point a sharp drop appears on the curve, indicating that X-rays of shorter wave-length than the critical value are absorbed by the chemical element to a greater extent than X-rays of longer wave-length. For the two strongest absorption bands characteristic of each element we have obtained the curves on both sides of the crystal table zero. The angular distance between the mid-points on two corresponding drops gives us twice the glancing angle, θ,

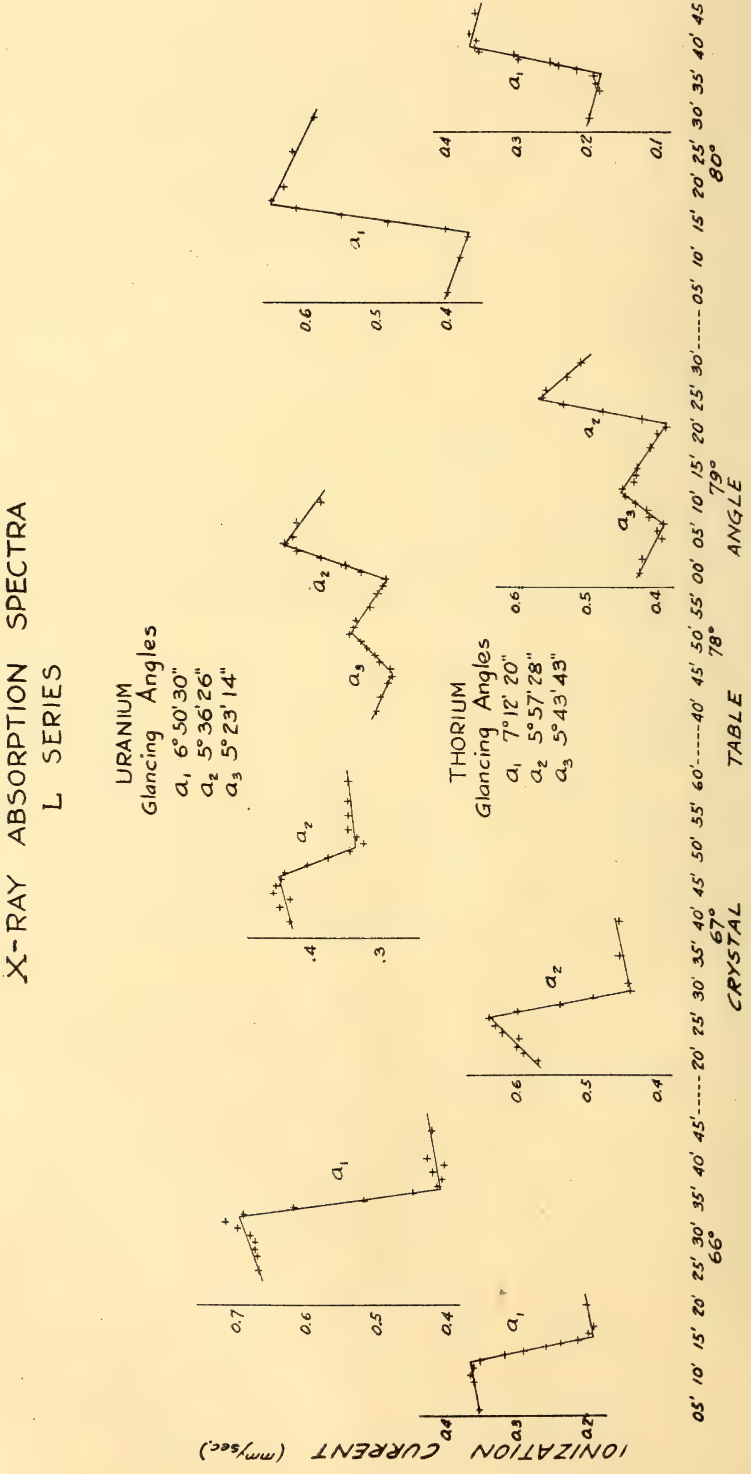
X-RAY ABSORPTION SPECTRA
L SERIES
URANIUM
Glancing Angles
a_1 6°50'30"
a_2 5°36'26"
a_3 5°23'14"
THORIUM
Glancing Angles
a_1 7°12'20"
a_2 5°57'28"
a_3 5°43'43"
a_1
a_2
a_3
IONIZATION CURRENT (mm/sec.)
0.7
0.6
0.5
0.4
0.3
0.2
0.1
.4
.3
05' 10' 15' 20' 25' 30' 35' 40' 45'----20' 25' 30' 35' 40' 45' 50' 55' 60'----40' 45' 50' 55' 00' 05' 10' 15' 20' 25' 30'----05' 10' 15' 20' 25' 30' 35' 40' 45'
66°
67°
78°
79°
80°
CRYSTAL TABLE ANGLE

FIG. 1

from which we compute the wave-length, λ, by means of the usual formula

$$\lambda = 2d \times \sin \theta,$$

where d is the distance between the reflecting planes of the crystal. The glancing angle for the third and weakest absorption band has been estimated by careful comparison with the neighboring stronger absorption drop obtained in the same series of readings.

Data Obtained.—Table 1 contains the results of our measurements. For purposes of comparison we include also the wave-lengths given by Wagner[1] and de Broglie.[2] The angular breadths of the drops vary from three to ten minutes of arc according to circumstances, and we believe that we can estimate the positions of their centres to within ten to thirty seconds, depending upon the magnitudes of the drops and the regularity of the curves. Considerations of this kind have given us the estimates of the accuracy of each measurement recorded in the table.

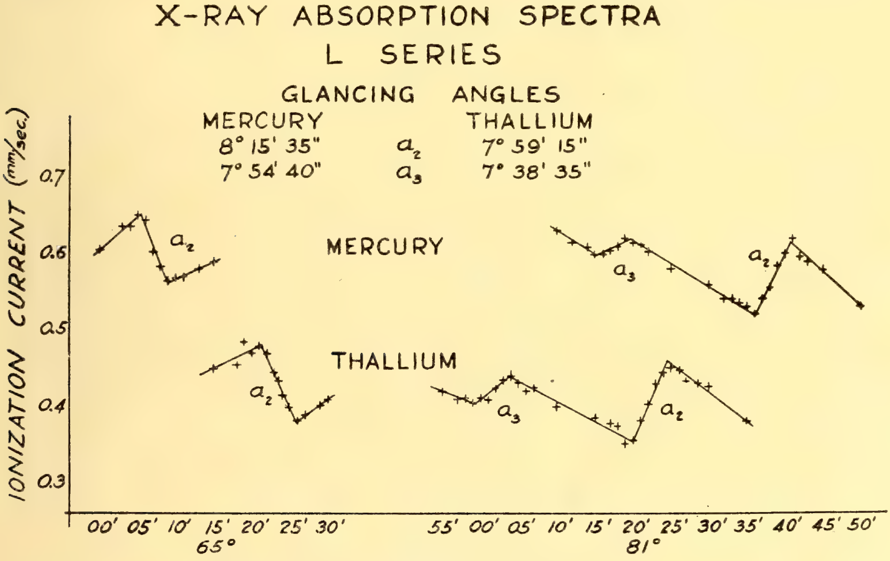

FIG. 2

Relative Positions of Emission and Absorption Wave-lengths.—In the article referred to above[3] the authors have called attention to the fact that in the case of tungsten: (*a*) the absorption wave-length, a_3, is slightly shorter than that of the highest frequency emission line of the L series, namely, γ_4; (*b*) the absorption wave-length, a_2, is a large fraction of one per cent longer than that of the emission line, γ_2; and (*c*) the absorption wave-length, a_1, is a large fraction of one per cent longer than the emission line, β_5. The measurements on which these conclusions were based were made under the same experimental conditions for both the emission and the absorption lines. We have not measured the L series of emission lines for chemical elements of higher atomic numbers than that of tungsten with our spectrometer. Hence we cannot make a direct comparison between emission and absorption wave-lengths obtained under the same experimental conditions for these elements. If we take the wave-lenghts recorded by Siegbahn and Friman,[4] we find that (*a*) our value for the absorption wave-length, $L\alpha_3$, is shorter than that of the emission line, γ_4, for each element and that the difference between the two increases slightly with the atomic number; (*b*) our value for the absorption wave-length, $L\alpha_2$, is longer than that of the emission line, γ_2, for tungsten and gold, but shorter for the other elements; (*c*) our value for the absorption wave-length, $L\alpha_1$, is longer than that of the emission line, β_5, for tungsten, gold, and thallium, but shorter for platinum and bismuth. The relative positions of these emission and absorption lines appear to be matters of considerable theoretical importance,[5] and this subject will be discussed at greater length in a future note.

TABLE 1

Critical Absorption Wave-Lengths. L Serirs of X-Rays

Grating Space for Calcite, $2d = (6.056 \pm 0.004) \times 10^{-8}$ cm.

CHEMICAL ELEMENT	ATOMIC NUMBER	ABSORBING SCREEN	MEASURE BY	WAVE-LENGTHS, $\lambda \times 10^8$ CM.		
				$L\alpha_1$	$L\alpha_2$	$L\alpha_3$
Tungsten	74		de Broglie	1.217	1.083	
Tungsten	74	Na_2WO_4	Author's	1.2136±1	1.0726±5	1.024±3
Platinum	78		Wagner	1.072	0.934	
Platinum	78		de Broglie	1.069	0.930	
Platinum	78	Metal	Author's	1.0705±3	0.9321±3	0.8885±9
Gold	79		Wagner	1.042	0.914	
Gold	79		de Broglie	1.038	0.898	0.858
Gold	79	Metal	Author's	1.0383±3	0.8993±3	0.8606±8
Mercury	80		de Broglie	1.006		
Mercury	80	$HgSO_4$	Author's	1.0067±5	0.8700±3	0.8335±9
Thalium	81		de Broglie	0.974	0.840	
Thalium	81	$Tl_2(SO_4)_3$	Author's	0.9776±3	0.8415±3	0.8055±14
Lead	82		de Broglie	0.945	0.811	
Lead	82	$Pb(NO_3)_2$	Author's	0.9497±3	0.8133±3	0.7803±9
Bismuth	83		de Broglie	0.921	0.786	0.753
Bismuth	83	Metal $Bi(NO_3)_3$	Author's	0.9216±3	0.7872±3	0.7532±9
Radium	88		de Broglie	0.802	0.670	
Thorium	90		de Broglie	0.757	0.624	0.604
Thorium	90	$Th(SO_4)_2$	Author's	0.7596±3	0.6286±3	0.6044±7
Uranium	92		de Broglie	0.718	0.588	0.564
Uranium	92	Acetate	Author's	0.7214±3	0.5918±3	0.5685±7

The Wave-lengths as Functions of the Atomic Numbers.—Many of the laws connecting X-ray wave-lenghts with each other, or with the atomic numbers of the chemical elements may be expressed most simply in terms of the wave-numbers, $1/\lambda$, or of the ratios of the frequencies of vibration to the Rydberg fundamental frequency for heavy atoms. In his classical memoirs on the wave-lengths of characteristic X-rays Moseley called attention to the fact that the square root of the wave-number (or frequency) of a line in the X-ray spectrum of an element is very nearly a linear function of its atomic number. To test this relation in the case of critical absorption data we have calculated the wave-numbers and the square roots of the wave-numbers for all of our values. Column 3, in Table 2, contains the wave-numbers and Column 4 their square roots. In Column 5 we have tabulated the increase in the value of $\sqrt{1/\lambda}$ per unit increase in the atomic number. It appears that these differences do not vary from

TABLE 2

CRITICAL ABSORPTION WAVE-NUMBERS. L SERIES OF X-RAYS

Values of $1/\lambda$ and of $\sqrt{1/\lambda}$

CHEMICAL ELEMENT	ATOMIC NUMBER	$1/\lambda$	$\sqrt{1/\lambda}$ $L\alpha_1$	DIFFERENCES
Tungsten	74	0.8240±1	0.9077	
Platinum	78	0.9341±3	0.9665	0.0147±1
Gold	79	0.9631±3	0.9814	0.0149±3
Mercury	80	0.9933±4	0.9966	0.0152±4
Thalium	81	1.0229±3	1.0114	0.0148±4
Lead	82	1.0530±3	1.0262	0.0148±3
Bismuth	83	1.0851±3	1.0417	0.0155±3
Thorium	90	1.3164±5	1.1473	0.0151±1
Uranium	92	1.3862±5	1.1774	0.0151±2
			$L\alpha_2$	
Tungsten	74	0.9323±5	0.9656	
Platinum	78	1.0728±4	1.0358	0.0176±1
Gold	79	1.1120±4	1.0545	0.0187±4
Mercury	90	1.1494±4	1.0721	0.0176±4
Thalium	81	1.1883±5	1.0901	0.0180±5
Lead	82	1.2295±5	1.1088	0.0187±5
Bismuth	83	1.2703±4	1.1271	0.0182±5
Thorium	90	1.5908±8	1.2613	0.0192±1
Uranium	92	1.6898±8	1.2999	0.0193±3
			$L\alpha_3$	
Tungsten	74	0.977±3	0.988	
Platinum	78	1.1255±12	1.0609	0.0182±5
Gold	79	1.162±1	1.0780	0.0171±11
Mercury	80	1.200±1	1.0953	0.0174±11
Thalium	81	1.242±2	1.1142	0.0189±15
Lead	82	1.282±2	1.1320	0.0178±16
Bismuth	83	1.328±2	1.1523	0.0203±13
Thorium	90	1.655±2	1.2863	0.0192±2
Uranium	92	1.759±2	1.3263	0.0200±7

the average values of the differences in their neighborhoods by more than the estimated errors, except in one or two cases. In other words, the $\sqrt{1/\lambda}$ may be represented as a function of the atomic number by a smooth curve, from which the individual values do not differ by more than the experimental errors. In the case of the absorption band, La_1, this curve differs a small amount from a straight line. In the cases of the bands La_2 and La_3, however, the departures from the straight line law appear to be much greater. A very much more pronounced variation from the linear law has been noted in the case of the critical absorption frequencies associated with the *K* series of X-rays.[6] These departures from the straight line law have been ascribed to the relativity change of mass with velocity of the electrons connected with the production of the X-rays.

The fact that the values of $\sqrt{1/\lambda}$ do not differ from a smooth curve law by more than the indicated errors of measurement shows that our estimates of accuracy are substantially correct.

The Relation between the Emission and Absorption Frequencies.—It has been found by experiment that the difference between the critical absorption wave-number of the *K* series and one of the critical absorption wave-numbers of the *L* series equals the wave-number of one of the α lines in the *K* series to within the limits of error.[7] The experiments were performed with the X-ray spectrum of tungsten. As the emission wave-lengths in the *K* series have not been measured for chemical elements of higher atomic numbers than that of tungsten, we cannot test this law between the *K* and *L* series for the heavier elements. We can test the law, however, in the case of the *L and M* series of thorium and uranium, for here all the ncessary data is available. The tables of Siegbahn and Friman[4] contain the emission wave-lengths in the *L* series, and Stenström[8] has measured three absorption wave-lengths in the M series for each of these two elements. Using our values of the *L* absorption wave-lengths we have compiled the following table of wave-numbers (Table 3):

TABLE 3
RELATION BETWEEN ABSORPTION AND EMISSION WAVE-NUMBERS
L and M Series

		U	TH		U	TH		U	TH
Absorption	La_1-Ma_1	1.099	1.047	La_1-Ma_2	1.085	1.034	La_1-Ma_3	1.038	0.988
Emission	$L\alpha_1$	1.098	1.045	$L\alpha_2$	1.085	1.032			
Absorption	La_2-Ma_1	1.403	1.322	La_2-Ma_2	1.389	1.309	La_2-Ma_3	1.342	1.263
Emission	$L\beta_3$	1.408	1.319	$L\beta_1$	1.389	1.305			
Absorption	La_3-Ma_1	1.472	1.385	La_3-Ma_2	1.458	1.372	La_3-Ma_3	1.411	1.326
Emission							$L\beta_3$	1.408	1.319

It appears from this table that the wave-numbers of the emission lines $L\alpha_1$ $L\alpha_2$ and $L\beta_1$ agree very well with the differences between the absorption wave-numbers La_1-Ma_1, La_1-Ma_2 and La_2-Ma_2, respectively.

Judging from this data alone, there is some question as to whether the emission line $L\beta_3$ belongs to the absorption difference La_2–Ma_1 or to La_3–Ma_3.

No emission lines have been observed in the spectrum of either thorium or uranium that correspond to the other absorption differences contained in the table.

Four emission lines have been found by Siegbahn and Friman in the spectra of both thorium and uranium in addition to those contained in our Table 3, namely, $1, \beta_2, \gamma_1$, and $\gamma_{2,3}$. The way in which the frequencies of the lines for different chemical elements vary with their atomic numbers[4] and the researches of Webster and Clark[9,5] on the voltages required to produce the lines in the spectrum of platinum indicate that 1 and β_2 belong to the same X-ray group as α_1 and α_2, and therefore also to the absorption La_1. Similarly, γ_1 belongs to the absorption La_2. If these lines obey the frequency difference law, there must be critical absorption wave-lengths at $\lambda = 2.37 \times 10^{-8}$ cm. and at $\lambda = 2.23 \times 10^{-8}$ cm. for thorium and uranium, respectively, to correspond with the emission line 1. These wave-lengths lie within the range of possible measurements, but the spectrum in their neighborhood has not been examined for critical absorption. Similarly, there should be critical absorption wave-lengths at $\lambda = 16.2 \times 10^{-8}$ cm. for thorium and at $\lambda = 15.8 \times 10^{-8}$ cm. for uranium to correspond with the emission lines β_2 and γ_1. These wave-lengths, however, lie beyond the range of wave-lengths that can be measured at present by means of crystal spectrometers.

It is well known that Bohr's theory of radiation explains very simply the above relation between emission and absorption wave-numbers. The relation, however, should not be absolutely exact, if, as Sommerfeld supposes, some of the atoms contain *M* orbits that are elliptic, and other atoms of the same chemical element contain *M* orbits that are circular. In this case the *L* absorption drops should have a complex structure. We have not been able to observe such a structure in the drops on our curves, and the effect must be exceedingly small, if it exits at all.

Theoreies have been developed by Rubinowicz[10] and Bohr,[11] according to which transfers of electrons between certain pairs of orbits cannot take place.[12] If we apply these theories to the data contained in Table 3, we find that according to both theories the transfers represented by Ma_1–La_1 and Ma_2–La_2 should occur. As a matter of fact, the corresponding lines, α_1 and β_1, are the strongest lines in the *L* series. According to Rubinowicz's theory the transfer Ma_2–La_1 should occur, but according to Bohr's theory it should not, unless the atom is in a field of force. The corresponding line, α_2, has been observed, but is relatively very faint. According to both theories the transfer Ma_1–La_1 should not occur, and, as noted above, the experiments do not indicate conclusively whether the line β_3 belongs to this combination or not. Both theories allow the transfer

Ma_3–La_1, but no line corresponding to it has been observed. The transfer Ma_3–La_2 can occur according to Rubinowicz's theory, but not according to Bohr's, and there is no line to correspond with it. The other transfers, involving La_3, are not covered by the theory, for it does not in its present form include a third L orbit.

Sommerfeld's L-Doublet Theory.—Sommerfeld has worked out an interesting theory of the splitting up of the L series into two distinct parts—the L series doublet. An account of his reasoning may be found in his book, Atombau und Spektrallinien, Chapter 5. According to this theory the difference in frequency $\Delta\nu$ between the two parts of the L series is due to the difference between the energies of electrons travelling in circular orbits and of electrons travelling in elliptic orbits. Some atoms are supposed to have circular L orbits, and other atoms of the same chemical element, elliptic L orbits. The general equation (28), in the chapter referred to, for the difference, $\Delta\nu$, in wave-number, reduces to

$$\frac{\Delta\nu}{\nu_\infty} = \frac{1}{\alpha^2}\left(\sqrt{2}\sqrt{1+\sqrt{1-b^2}} - \sqrt{4-b^2}\right), \qquad (1)$$

for the L doublet, where $\nu_\infty = 109737$ is Rydberg's fundamental wave-number, $\alpha = \frac{2\pi e^2}{hc} = .007286$, $b = \alpha(N - n)$, N is the atomic number, and n, a correction term due to the influence of neighboring electrons in the atom. Sommerfeld has tested his theory by means of the wave-number differences between certain emission lines in the spectra of a great many chemical elements, and has deduced from this data the value $n = 3.63$. Although there is some difficulty in interpreting this value of the constant, n, we have used it to test the formula by means of our values of the critical absorption wave-numbers.[13] According to the theory the wave-number difference in the doublet should equal the difference between the wave-numbers of the absorption bands La_1 and La_2.

TABLE 4

THE L-DOUBLET WAVE-NUMBER DIFFERENCES, $\Delta v \times 10^{-8}$

CHEMICAL ELEMENT	ATOMIC NUMBER	OBSERVED	CALCULATED		
			$n = 3.63$	$n = 3.45$	MODIFIED
Tungsten	74	0.1083 ± 6	0.1071	0.1087	0.1037
Platinum	78	0.1387 ± 7	0.1369	0.1384	0.1327
Gold	79	0.1489 ± 7	0.1453	0.1468	0.1411
Mercury	80	0.1561 ± 8	0.1540	0.1558	0.1496
Thalium	81	0.1654 ± 8	0.1635	0.1652	0.1587
Lead	80	0.1765 ± 8	0.1733	0.1751	0.1682
Bismuth	83	0.1852 ± 7	0.1836	0.1856	0.1782
Thorium	90	0.2744 ± 13	0.2713	0.2739	0.2639
Uranium	92	0.3036 ± 13	0.3023	0.3044	0.2941

Table 4 contains the observed wave-number differences of the L doublet, with estimated errors of experiment, and also three sets of computed differences. The values given in Columns 4 and 5 have been calculated directly by the above formula (1) without expanding it into a series of ascending powers of b. It would be necessary to use six or more terms of such a series in order to attain the indicated accuracy.

The differences between the observed values and those computed, using Sommerfeld's value of $n = 3.63$, all have the same sign, and are distinctly greater than the errors of measurement. The values obtained with $n = 3.45$ compare more favorably with the experimental results, and indicate that with $n = 3.43$ the formula would give the observed differences, $\Delta\nu$, within the limits of error for nearly all the elements contained in the table. Even in this case there appears to be a small systematic variation between the theoretical and experimental frequency differences, as though the formula were nearly but not quite correct.

The constant n represents the repulsive force on an electron in the L orbit due to the electrons in the K orbit plus the force due to the other electrons in the L orbit. If n_1 and n_2 are the numbers of electrons in the K and the L orbits, respectively, $n = n_1 + s_{n_2}$ for circular orbits, where

$$s_n = \frac{1}{4} \sum_{s=1}^{s=n-1} \operatorname{cosec} \frac{s\pi}{n}.$$

The values of n for elliptic orbits is assumed equal to that for circular orbits. There are, however, no values of n_1 and n_2 that agree exactly with the empirical values $n = 3.63$, 3.45 or 3.43. For $n_1 = 2$ and $n_2 = 5$ we have $n = 3.38$. Other evidence of the distribution of electrons in atoms indicates that this arrangement is highly improbable.

As a matter of fact, the theory in its present form does not take account entirely of the influence of all the electrons in the L orbit itself. When one electron is removed from this orbit, the other electrons change their positions relative to each other and to the nucleus. In calculating the change in the atom's energy envolved we must take account of the change in their energy also. A modification of Sommerfeld's formula which includes these energy changes may be obtained by taking the product of n_2 into the value of the right-hand member of equation (1), with $n = n_1 + s_{n_2}$, and subtracting from it the product of $n_2 - 1$ into the value of the same expression, with $n = n_1 + s_{n_2 - 1}$. Before computing wave-number differences from this modified formula we must assign definite values to n_1 and n_2. This gives a sort of theoretical value for n.

Column 6, in Table 4, contains wave-number differences calculated in this way with $n_1 = 2$ and $n_2 = 4$. They differ uniformly from the experimental results by about 3%.

The above suggested alterations in the theory, however, do not obviate certain difficulties (discussed by Sommerfeld in his book) that we encounter, if we suppose that the orbits all lie in the same plane. Possibly these difficulties will disappear, when we study more thoroughly atomic models in which the orbits do not lie in the same plane.

[1] E. Wagner, *Ann. Physik, Leipzig,* March, 1915 (868).

[2] M. de Broglie, *J. Physique, Paris,* May-June, 1916 (161); and *Paris C. R. Acad. Sci.,* Nov. 24, 1919 (962).

[3] Blake and Duane, *Physic. Rev., Ithaca,* Dec., 1917 (624); and Duane and Patterson, *Ibid.,* 1920.

[4] Siegbahn and Friman, *Phil. Mag., London,* April, 1916 (423); and Nov., 1916 (497).

[5] D. L. Webster, *Proc. Nat. Acad. Sci., Washington, D. C.,* Jan., 1920 (26).

[6] Duane and Hu, *Physic. Rev.,* June, 1918 (488); and Dec., 1919 (516); and Duane and Shimizu, *Ibid.,* Feb., 1919 (159) and Dec., 1919 (522).

[7] Duane and Shimizu, *Ibid.,* July, 1919 (67); Duane and Patterson, *Ibid.,* 1920; Duane and Stenström, *Ibid.,* April, 1920 (302); and *Proc. Nat. Acad. Sci.,* Aug. 1902.

[8] Stenström, *Doctor's Dissertation,* Lund, 1919.

[9] Webster and Clark, *Proc. Nat. Acad.,* March, 1917 (181).

[10] Rubinowicz, *Physik. Zs., Leipzig,* **19,** 1918 (441–465).

[11] Bohr, *Copenhagen Academy,* 1918.

[12] Compare Sommerfeld, *Atombau und Spektrallinien,* Chapter 6.

[13] Compare de Broglie *Paris, C. R. Acad. Sci.,* Nov. 24, 1919 (962).

ON THE RELATIVE POSITIONS AND INTENSITIES OF LINES IN X-RAY SPECTRA

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Communicated August 17, 1920

Grouping of L Series Lines.—The wave-lengths of the lines in the L series of X-rays have been measured for a great many of the chemical elements by Siegbahn and Friman.[1] They have published graphs which represent the square roots of the frequencies of corresponding lines in the spectra of different elements as functions of the atomic number. The graphs for the lines 1, α_2, α_1, β_2 and β_5 are very nearly straight, indicating that the square roots of the corresponding frequencies are almost linear functions of the atomic numbers, whereas the graphs for the other lines are curved, indicating a marked departure from the linear law. This would seem to mean that we can divide the lines in the L series into at least two groups.

In his work on characteristic absorption de Broglie[2] found three critical absorption wave-lengths associated with the L series of gold, bismuth, thorium and uranium. The authors[3] have extended this, and have measured the third critical absorption wave-length as well as the other two for the elements they examined from tungsten to uranium both inclusive. The fact that three critical absorption wave-lengths appear in the L series of each chemical element would seem to indicate the existence of at least three distinct groups of lines.

The researches of D. L. Webster and Harry Clark[4] on the voltages required to produce the various lines in the L series of platinum and on the way in which their intensities increase with increasing voltage confirm this, and prove further that the lines η, β_1, γ_1 and probably γ_2 belong to the second group.

The modern theory of line spectra, also, seems to indicate the splitting up of the L series into groups. The differences in frequency between certain lines in the first group and corresponding lines in the second group are very nearly equal to each other. These pairs of lines with nearly the same frequency interval are η–l, β_1–α_2, γ_1–β_2 and γ_2–β_5. Sommerfeld[5] explains this division of the L series into groups with a constant frequency difference on the assumption that one group of lines is due to electrons falling into an L orbit that is circular, and that the other group is produced by electrons falling into an L orbit that is elliptic. The equation he derives from his theory for the magnitude of the frequency difference fits the facts with considerable precision.

The following facts indicate that the critical absoprtion La_1 belongs to the first group of lines, and that the critical absorption La_2 belongs to the second group of lines. Firstly, the wave-length of the critical absorption La_1 lies not far from that of the line of shortest wave-length in the first group, and the wave-length of La_2 lies close to that of the line of shortest wave-length in the second group. Secondly, as the authors have shown,[3] the square root of the frequency of La_1 for the different elements is almost a linear function of the atomic number, while that of La_2 departs from the linear law in the same way as do those of the second group of lines in Siegbahn and Friman's graphs. Thirdly, the frequency difference between La_2 and La_1 equals to a high degree of precision that between the pairs of lines η–l, β_1–α_2 and γ_2–β_5 which were all measured by the authors[6] under the same experimental conditions for the tungsten spectrum. Fourthly, the frequency differences between La_2 and La_1 for the various chemical elements from tungsten to uranium[3] agree very well with Sommerfeld's formula. Fifthly, the values of the critical voltages of two of the groups of lines in the L series of platinum measured by Webster and Clark[4] (namely, 13.20 kilovolts and 11.45 kilovolts) agree very well with the values of the same quantities calculated by the quantum equation from our critical absorption wave-lengths (namely 13.26 kilovolts and 11.54 kilovolts).

Relative Position of Lines.—It has been shown by Duane and Hu[7] that the critical absorption wave-length associated with the K series of Rhodium is about one-third of a per cent shorter than that of the shortest line (the γ line) in that K series. Further, Duane and Stenström[8] have found that the difference between the wave-length of the critical absorption in the K series of tungsten and that of its γ line amounts to about one-half of one per cent, the γ line having the longer wave-length. In the case of each element the wave-lengths were measured under exactly the same experimental conditions, so that there can be no doubt but that the critical absorption wave-length in the K series is shorter than the wave-length of the γ line by an amount that considerably exceeds the errors of experiment.

Turning to the L series we find that the third critical absorption wave-length, La_3, appears to be shorter than that of the line of shortest wave-length in the L series, namely, γ_4.[3] The authors[6] have found the following values for these wave-lengths, $La_3 = 1.024 \pm 3$ and $\gamma_4 = 1.0261 \pm 6$, in the spectrum of tungsten. In the other two groups, however, the critical absorption wave-lengths are longer than those of the shortest wave-length lines respectively. In the tungsten spectrum the authors[3,6] found for the wave-lengths in the first group $La_1 = 1.2136 \pm 1$ and $\beta_5 = 1.2040 \pm 7$, and in the second $La_2 = 1.0726 \pm 5$ and $\gamma_2 = 1.0655 \pm 4$. In each case the difference between the two wave-lengths considerably exceeds the estimated errors of experiment.

As the relative position of these emission and absorption lines appears to be a matter of considerable theoretical importance the authors have repeated their experiments in a slightly different form. To make doubly sure of the facts they have taken the readings so as to show the position of the critical absorption and those of the emission lines on each side of it on one and the same curve. The experimental conditions were exactly the same as those in the experiments described in the article referred to,[6] except that the X-ray tube was turned so that the rays left the target making a slightly smaller angle with its surface than in the earlier experiments. This increased the absorption of the rays by the target itself, and made the absorption drop in the curves more prominent.

The curve in figure 1 represents the intensity of the radiation (measured by the currents in the X-ray spectrometer's ionization chamber) as a function of the crystal table angles. The drop in the curve marked a_1 corresponds to the critical absorption belonging to the first group of L series emission lines. The absorption of its own rays by the tungsten target alone produced this drop, as no absorbing screen was introduced in the path of the rays in this experiment. The tall peak on the curve represents the relatively strong emission line β_2. Its separation from the absorption drop corresponds very well with the wave-length interval between the two, calculated from the data obtained in the earlier experiments, namely 0.0283 ± 2. The small hump in the curve on the other side of the absorption drop represents the emission line, β_5.

This experiment proves conclusively that the critical absorption lies between the emission lines β_2 and β_5, in other words it has a longer wave-length than that of β_5.

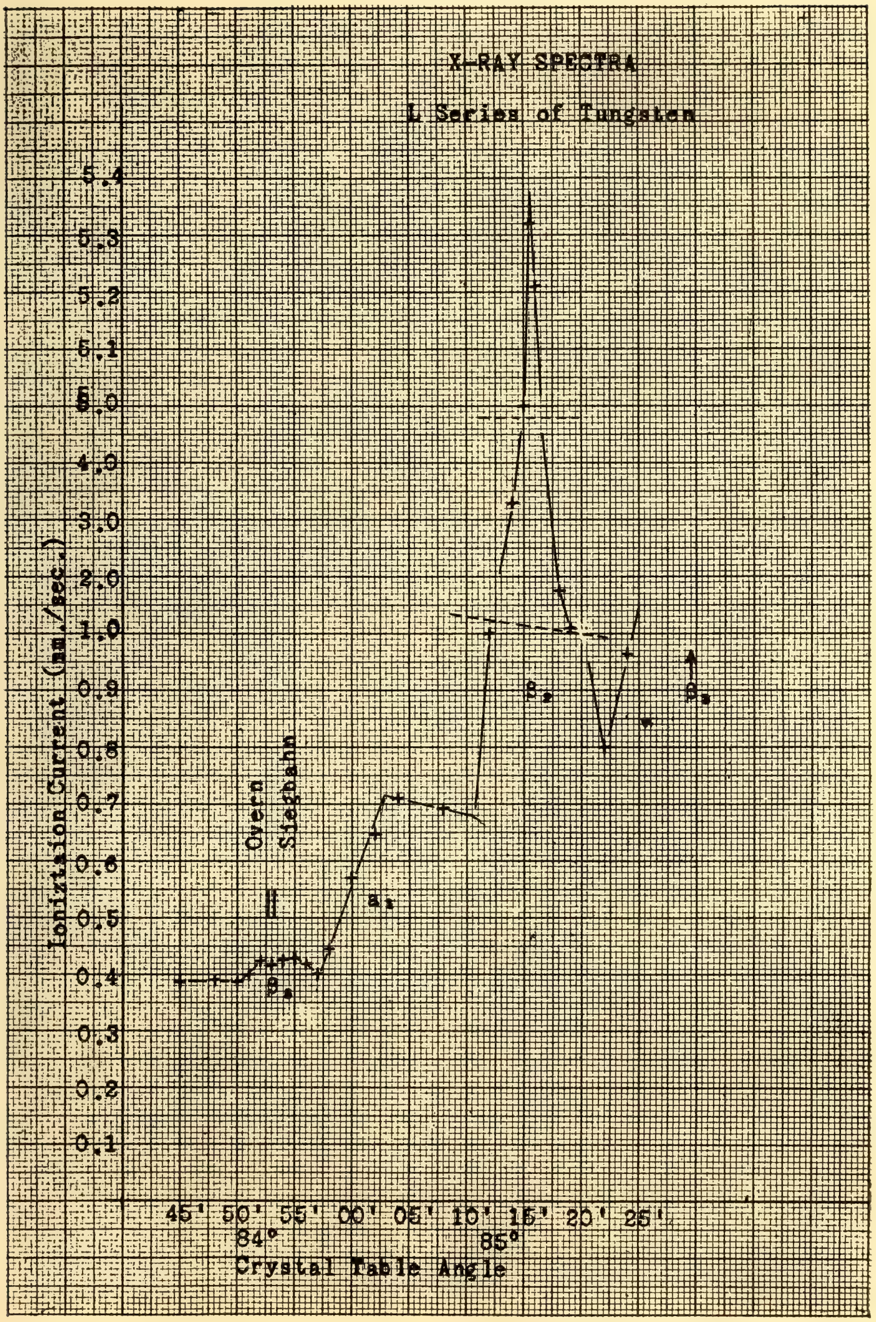
X-RAY SPECTRA
L Series of Tungsten
Ionization Current (mm./sec.)
5.4
5.3
5.2
5.1
5.0
4.0
3.0
2.0
1.0
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
Overn
Siegbahn
β4
a1
β2
β3
45' 50' 55' 00' 05' 10' 15' 20' 25'
84°
85°
Crystal Table Angle

FIG. 1

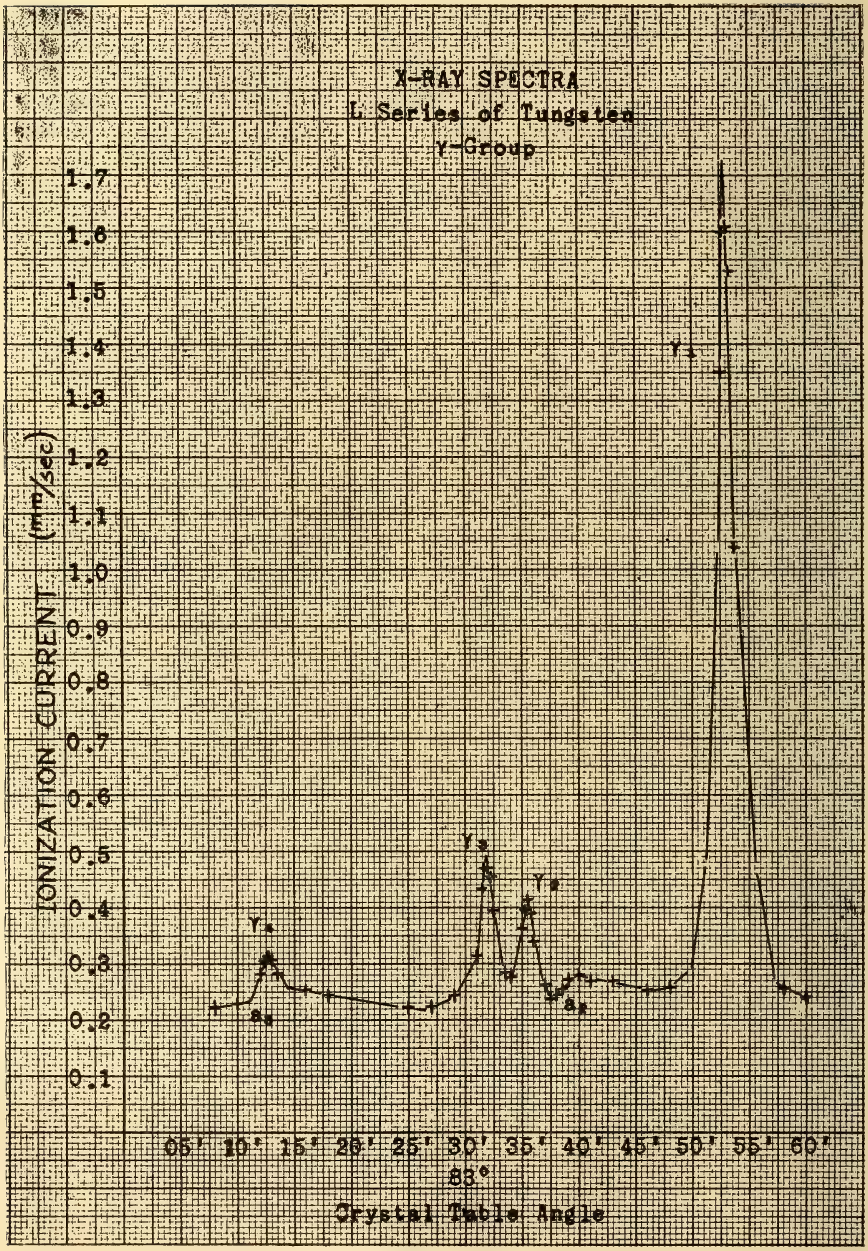
X-RAY SPECTRA
L Series of Tungsten
γ-Group
IONIZATION CURRENT (mm/sec)
1.7
1.6
1.5
1.4
1.3
1.2
1.1
1.0
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
γ_1
γ_3
γ_2
γ_4
a_4
a_2
05'
10'
15'
20'
25'
30'
35'
40'
45'
50'
55'
60'
83°
Crystal Table Angle

FIG. 2

The positions of β_5 as determined from the measurements of Overn[9] and Siegbahn[10] are marked above the curve in figure 1. According to their measurements this faint line has a wave-length a small fraction of one per cent shorter than our value, which would bring it still further from the critical absorption in the direction of shorter wave-lengths.

The curve in figure 2 represents the spectrum of tungsten in the neighborhood of the critical absorption La_2. The drop corresponding to the absorption and the peaks corresponding to the γ emission lines appear on the curve. The critical absorption lies between the emission lines γ_1 and γ_2, and therefore has a wave-length longer than that of γ_2. Overn's and Siegbahn's values for γ_2 are in exact agreement with our values.

According to the conception of radiation held by many scientists the critical absorption wave-length corresponds to the short wave-length limit of the group or series of emission lines with which it is associated. If this be true, or, to speak more accurately, if the term "group of lines" is defined in such a way that this is true, then β_5 cannot belong to the first group in the L series of X-rays, and γ_2 cannot belong to the second group.

The strongest evidence in favor of believing that β_5 belongs to the same *atomic mechanism* that produces the lines in the first group appears to be that the square root of its frequency increases from atom to atom nearly as a linear function of the atomic number, corresponding in this respect to all the lines in the first group but not to those in the second and third groups. In terms of the theory of atomic orbits this means that electrons falling into the L_1 orbit produce the line β_5, as they do all the lines in group 1. Webster and Clark[4] found that β_5 in the platinum spectrum appeared at a lower voltage than that required to produce the lines in the second group. This proves conclusively that β_5 cannot belong to the second group. The experiments on critical potentials, however, are not sufficiently accurate to decide whether β_5 appears at *exactly* the same voltage as the other lines in group 1, for β_5 is a weak line, and the difference between its wave-length and that of the critical absorption L_1 amounts to only 0.7% for tungsten. We are not, therefore, compelled to assume that β_5 can be produced by electrons in the X-ray tube having quantities of energy less than that given by the quantum equation, $Ve = hv$.

The argument in favor of supposing that the line γ_2 belongs to the mechanism that produces the lines in the second group rests largely upon the fact that for the various chemical elements the difference in frequency between γ_2 and β_5 equals the frequency interval between the lines in the other pairs belonging respectively to the two groups. It also equals the difference in frequency between the two critical absorptions La_2 and La_1, and is given quite accurately by Sommerfeld's L doublet formula. Hence, according to the theory of electron orbits, γ_2, in common with the other lines belonging to the second group, is produced by electrons falling into the second, L_2, orbit.

On the experimental and theoretical evidence we have reached the conclusion that the emission line β_5 belongs to the atomic mechanism that produces the lines in the L_1 group, and that γ_2 belongs to that producing the lines in the L_2 group. On the other hand our experiments prove conclusively that the wave-length of β_5 is shorter than that of the La_1 critical absorption, and that the wave-length of γ_2 is shorter than that of La_2. A somewhat similar situation occurs in the spectra of ordinary light. Here we have resonance and ionization potentials. There is a difference, however, between the character of the absorption of X-rays and that of ordinary light.

The best way of explaining these phenomena may be somewhat as follows: Let us suppose that the critical absorption La_1 corresponds to the transfer of an electron from the L_1 orbit to the periphery of the atom—to an orbit there where it can find a place to stick. This does not mean a transfer to an indefinitely great distance from the centre of the atom. There may be many orbits outside that at the periphery, even in addition to those that belong to visible spectra. With the electron in the orbit at the periphery the atom is in a state such that by the transfer of an electron back to fill the vacancy in the L_1 orbit it can radiate any one of the emission lines 1, α_2, α_1, β_2, etc., but not β_5. If, now, the frequency of the incident X-ray beam is higher than that of the critical absorption, if the energy hv is greater than that required to lift the electron to the periphery of the atom, it may carry the electron to an orbit further out or even outside of the atom altogether. In this case the atom would be in a state such that the return of an electron from one of these outside orbits would emit X-radiation of higher frequency and shorter wave-length than that of the critical absorption. This would correspond to the emission line β_5. A similar explanation applies also to the emission line γ_2, and the L_2 orbit.

From this point of view we would expect the critical potential for β_5 to be slightly higher than that for the other lines belonging to the L_1 orbit, and the critical potential for γ_2 to be slightly higher than that for the lines associated with the L_2 orbit.

Further, on this theory, the critical absorption does not correspond to the limit of the group, or series. It becomes incorrect to speak of the "absorption limit," as these words are usually employed.

An accurate, quantitative test for our point of view cannot be obtained without making special assumptions. According to our measurements, however, the difference in frequency between the emission line and the critical absorption for tungsten, each divided by the Rydberg constant, is 6.0 for β_5–La_1, and 5.6 for γ_2–La_2. These are of the order of magnitude of frequencies associated with the peripheries of atoms.

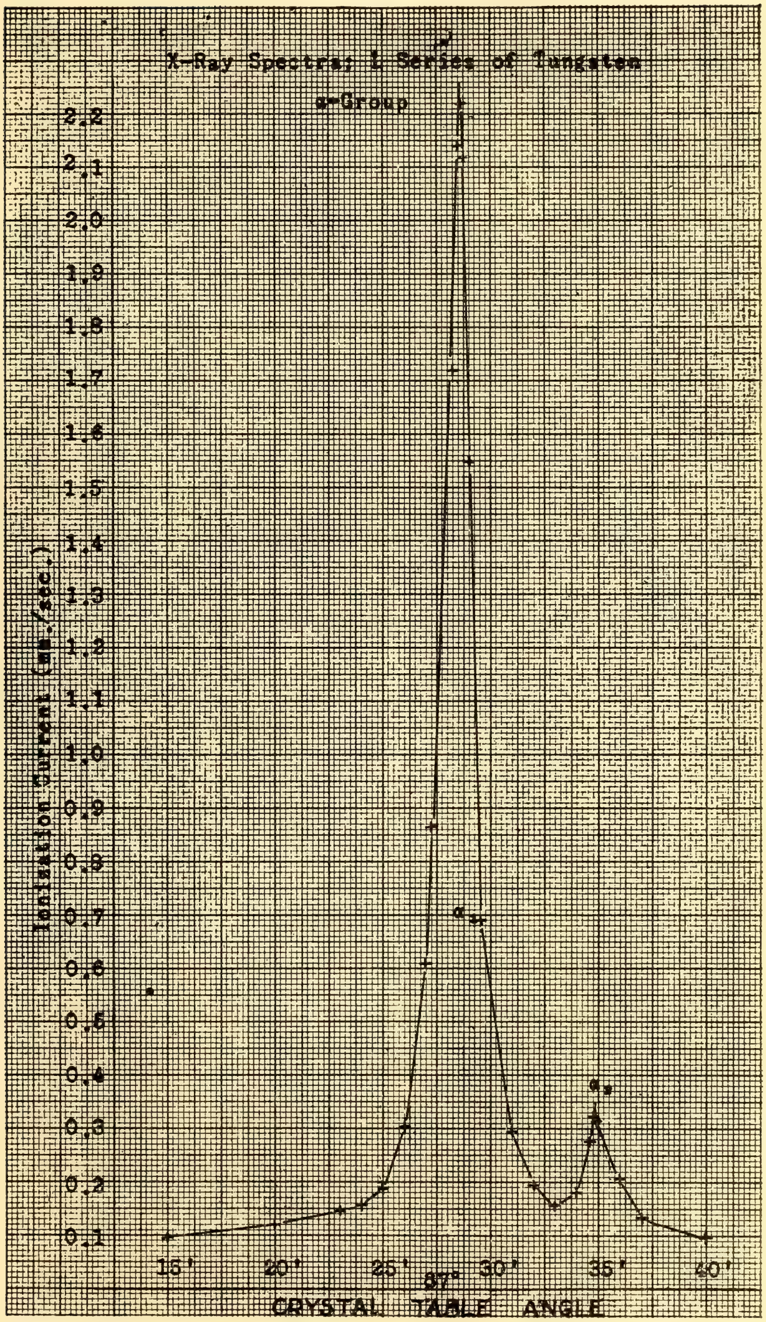
X-Ray Spectra; L Series of Tungsten
α-Group
Ionization Current (mm./sec.)
2.2
2.1
2.0
1.9
1.8
1.7
1.6
1.5
1.4
1.3
1.2
1.1
1.0
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
α
α₂
15'
20'
25'
87°
30'
35'
40'
CRYSTAL TABLE ANGLE

FIG. 3

The question now arises as to whether or not an L critical ionization frequency equals an L critical absorption frequency. A critical ionization frequency represents the transfer of an electron completely *outside* of an atom. In the case of the K series of iodine the two frequencies have been found to be equal to each other within the limits of error of the measurements.[7] Experiments are in progress to test this point for the L series.

Relative Intensities of Lines.—The ionization spectrometer provides us with an excellent method of estimating the relative intensities of lines in X-ray spectra. Accurate estimates of the relative intensities, however, cannot be obtained unless the lines lie fairly close together. If a considerable interval separates them, corrections must be applied for the changes with varying wave-length in the amounts of energy absorbed by the substances through which the rays pass, etc. These corrections cannot be accurately calculated at present. Special precautions must be taken to make sure that no critical absorption nor critical ionization wave-length lies between the wave-length of the lines to be compared with each other. These limits to the accuracy of relative intensity measurements present themselves in photographic spectrometry in addition to those due to the difficulty of estimating the blackening of the photographic plates.

In the case of a line that is not perceptibly broadened we take the height of the peak corresponding to it on the ionization current crystal table angle graph to represent its intensity. This height must be measured from the level of the curve corresponding to the general radiation on the two sides of the peak, and not from the axis of zero ionization current.

Two peaks appear on the curve in figure 1, corresponding to the spectral lines β_2 and β_5. The ratio of the heights of these two peaks is 116. This number, however, does not represent accurately the relative intensity of β_2 to β_5, for the critical absorption of La_1 lies between the two emission lines. The target absorbs a greater fraction of the X-ray energy in the neighborhood of β_5 than it does in the neighborhood of β_2. In this case, therefore, the ratio of the heights of the peaks gives us only an upper limit for the ratio of the intensities. β_2 cannot be more than 116 times as intense as β_5.

The curve in figure 2 contains four peaks corresponding to the emission lines γ_1, γ_2, γ_3 and γ_4. The relative heights of these peaks are represented by the numbers 100, 14, 18 and 6. These numbers have not been corrected for the errors due to absorption, etc., so that they give only an approximate estimate of the relative intensities of the emission lines. The lines γ_2 and γ_3, however, lie very close together, and the correction must be very small. Since the critical voltages for all the γ lines are not quite the same, their relative intensities depend somewhat on the voltage applied to the X-ray tube. This was 22,750 volts. The applied voltage lies so far above the critical voltages, however, that the effect due to variations in the latter do not amount to as much as three per cent.

Figure 2 in the paper referred to above[6] contains curves representing the four strongest β lines. The heights of the peaks corresponding to β_1, β_2, β_3 and β_4 are proportional respectively to the numbers 100, 55, 16 and 9; the voltage applied to the tube in this experiment being 24,800 volts.

The ratio of intensities of the two α emission lines possesses special theoretical interest. Sommerfeld[5] has developed a theory which predicts the relative intensity of certain lines in the spectrum. By applying the quantum theory to electron orbits that do not lie in the same plane he finds that the number of postitions which the plane of an orbit can occupy is greater by unity than the quantum number associated with the electron's angular coördinate. According to a line of reasoning due to Bohr[11] one of these positions is in a certain sense dynamically impossible, which reduces the number of possible positions the plane of the orbit can occupy by unity. According to Sommerfeld's theory all the possible positions of the orbit's plane are equally probably, and hence the intensity of the line is proportional to the number of possible positions.

In the case of the α lines in the K series this theory agrees very well with the facts.[8] The α lines in the L series, however, do not have the relative intensity predicted by the theory. The elctrons producing these α lines fall from M orbits into the same L_1 orbit. Those producing the α_1 line fall from the first, M_1, orbit, which has three quanta associated with the angular coördinates, while those producing the α_2 line come from the orbit M_2, which has only two quanta associated with the angular coördinates. According to Sommerfeld, therefore, the relative intensities should be in the ratio of 4 to 3. As a matter of fact, the ratio is very much larger than this. The curve in figure 3 represents the two α lines in the L series of tungsten. The ratio of the heights of the two peaks is about 10 to 1. According to Bohr's "analogue principle" the transfer from M_2 to L_1 could not take place, unless the atom were in a field of force. This may account for the weakness of the α_2 line.

[1] Siegbahn and Friman, *Phil. Mag., London,* April, 1916 (403); and Nov. 1916 (497).

[2] de Broglie, *J. Physique, Paris,* May–June, 1916 (161).

[3] Duane and Patterson, *Proc. Nat. Acad Sci., Washington,* Sept. 1920.

[4] Webster and Clark, *Ibid.,* March, 1916; and Webster, *Ibid.,* Jan., 1920.

[5] Sommerfeld, *Atombau und Spektrallinien.*

[6] Duane and Patterson, *Physic. Rev., Ithaca,* 1920.

[7] Duane and Hu, *Ibid.,* Oct., 1919 (369).

[8] Duane and Stenström, *Ibid.,* April, 1920 (329); and *Proc. Nat. Acad. Sci. Washington,* Aug., 1920.

[9] Overn, *Physic. Rev.,* Aug., 1919 (137).

[10] Siegbahn, *Phil. Mag.,* Nov., 1919 (639).

[11] Bohr, *Copenhagen Academy,* 1918.

ON A DIFFERENTIAL EQUATION OCCURRING IN PAGE'S THEORY OF ELECTROMAGNETISM

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Communicated by R. A. Millikan, June 7, 1920

In a recent article Mr. Leigh Page[1] has generalized the electromagnetic equations by introducing the idea of a rotation of the field round a moving electric pole. A line of electric force is supposed to be the locus of points traveling with constant velocity, c, in directions which vary according to a law giving rise to three differential equations of type

$$k\frac{dl}{d\tau} = c(f + vr - wq) - m(c^2r - gu + fv) + n(c^2q - fw + hu)$$
$$- cl[l(f + vr - wq) + m(g + wp - ur) + n(h + uq - vp)], \quad (1)$$

where (cl, cm, cn) are the component velocities of the point which leaves the electric pole at time τ; (u, v, w), (f, g, h) and (p, q, r) are the components of the velocity, acceleration and angular velocity of the pole at this instant. The quantity, k, is defined by the equation $k = c^2 - u^2 - v^2 - w^2$. These equations are of a type considered in my book on *Differential Equations* (Ex. 7, p. 71) and can be reduced to two Riccatian equations by means of the substitutions

$$s = \frac{l + im}{1 + n}, \quad \sigma = \frac{l - im}{1 + n}, \quad i = \sqrt{-1}. \quad (2)$$

The lines of force can consequently be found by solving a Riccatian equation just as in the usual electromagnetic theory.[2]

It should be noticed that Page's expressions for the electric and magnetic vectors may be written in the forms

$$\mathbf{E}_x = \frac{1}{c}\frac{\partial(\tau, \sigma)}{\partial(x, t)} - \frac{\partial(\tau, \theta)}{\partial(y, z)},$$

$$\text{— — — — — — — —} \quad (3)$$

$$\mathbf{H}_x = \frac{\partial(\tau, \sigma)}{\partial(y, z)} + \frac{1}{c}\frac{\partial(\tau, \theta)}{\partial(x, t)},$$

where

$$\theta = \frac{ec^3}{4\pi k}\,\frac{(x - \xi)p + (y - \eta)q + (z - \zeta)r}{c^2(t - \tau) - u(x - \xi) - v(y - \eta) - w(z - \zeta)},$$

$$\sigma = \frac{e}{4\pi}\,\frac{(x - \xi)f + (y - \eta)g + (z - \zeta)h + k}{c^2(t - \tau) - u(x - \xi) - v(y - \eta) - w(z - \zeta)}, \quad (4)$$

e is the electric charge associated with the moving electric pole, (ξ, η, ζ) are the coördinates of the pole at time τ and τ is defined in terms of (x, y, z, t) by the equation

$$[x - \xi(\tau)]^2 + [y - \eta(\tau)]^2 + [z - \zeta(\tau)]^2 = c^2(t - \tau)^2. \quad \tau < t.$$

The electromagnetic field is thus obtained by superposing on the usual electromagnetic field of a moving electric pole a field derived from a type of field, which I have considered elsewhere,[3] by writing **H** for **E** and $-$**E** for **H.** The properties of this additional field may be derived at once from the properties of the field described previously by simply interchanging the words magnetism and electricity.

If (f, g, h) are no longer restricted to be the components of the acceleration and k is no longer restricted to be $c^2 - u^2 - v^2 - w^2$, the formulae (3), (4) still specify an electromagnetic field in which a line of force is the locus of points moving with velocity c in directions specified by a set of equations of type (1) and these equations can still be reduced to Riccatian equations by means of the substitution (2).

In a field of this more general type both electricity and magnetism travel away from the electric pole with the velocity of light while in the case of a field of Page's type there is an emission of magnetism but no emission of electricity. There is, however, no magnetic charge associate with the moving electric pole.

In a field of this general type it is possible when $f = g = h = p = q = r = 0$ for the points which trace out a line of force to move in one constant direction which is the same for all and for there to be no radiation of energy even through the electric pole has an acceleration.[3] It is possible, then, for an electric pole of this more general type to describe a circular orbit without radiating energy, as in Bohr's theory of the hydrogen atom. In the transition from one circular orbit to another the angular velocity (p, q, r) may be different from zero and there may be radiation of the type described by Page.

The weak point of the present theory is that it requires the presence of electric and magnetic currents in the space surrounding the electric pole. This space in fact contains a continuous distribution of electric and magnetic particles which have been emitted from the moving pole and are travelling along straight lines with the velocity of light.

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[1] *Proc. Nat. Acad. Sci.*, **6**, March, 1920 (115).

[2] *Amer. J. Math.*, **37**, 1915 (192); see also Murnaghan, F. D., *Ibid.*, **39**, 1917; and *Johns Hopkins Circular*, July, 1915.

[3] *Proc. London Math. Soc.*, (Ser. 2) **18**, 1919 (95), paragraph 10.

A NEW PROOF OF A THEOREM DUE TO SCHOENFLIES

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In a paper recently published in the Transactions of the American Mathematical Society, Professor Robert L. Moore[1] proved the following theorem: If $ABCDA$ is a closed curve, then there exist two sets of arcs α_1 and α_2 such that (1) each arc of α_1 lies wholly within $ABCDA$ except that its end-points are in AB and CD, (2) each arc of α_2 lies wholly within $ABCDA$ except that its end-points are on BC and DA, (3) each point of $ABCDA$, except A, B, C, and D, is an end-point of just one arc of α_1 or of just one arc of α_2, (4) through each point within $ABCDA$ there is just one arc of α_1 and just one arc of α_2, (5) each arc of α_1 has just one point in common with each arc of α_2. It is the purpose of the present note to show how this theorem may be used to establish the following theorem due to Schoenflies[2]—*Suppose the closed curves* J_1 *and* J_2 *are in continuous one-to-one correspondence under a transformation* π. *Let* R_i ($i = 1, 2$) *denote the point set* $J_i + I_i$, *the interior of* J_i. *Then there exists a continuous one-to-one correspondence* π' *between the points of* R_1 *and* R_2, *such that points of* J_1 *and* J_2 *correspond as under the transformation* π.

Proof.—Let A_1, B_1, C_1 and D_1 be four points on the closed curve J_1 such that A_1 and C_1 separate B_1 and D_1 on J_1. Let $\pi(A_1) = A_2$, $\pi(B_1) = B_2$, $\pi(C_1) = C_2$ and $\pi(D_1) = D_2$. It may easily be proved, with the use of the definition of a simple closed curve and the properties of continuous one-to-one correspondences, that A_2 and C_2 separate B_2 and D_2 on J_2. Cover R_i ($i = 1, 2$) with sets of arcs $(\alpha_{i,1})$ and $(\alpha_{i,2})$ having the properties described in Professor Moore's theorem. Consider the square whose vertices are A (0, 0), B (0, 1), C (1, 1) and D (1, 0). There exists a continuous one-to-one correspondence Σ between the points of that arc of J_1 from A_1 to B_1 which does not contain C_1 and the points of the interval AB of the Y axis, such that $\Sigma(A_1) = A$ and $\Sigma(B_1) = B$. Likewise there exists a continuous one-to-one correspondence Σ' between the points of that arc of J_1 from A_1 to D_1, which does not contain B_1, and the interval AD of the X-axis such that $\Sigma'(A_1) = A$ and $\Sigma'(D_1) = D$. Let us assign to every point P_1 of R_1 coördinates (x'_{P_1}, y'_{P_1}) in the following manner: Suppose P_1 is a point of R_1 lying neither on the interval A_1B_1 nor on the interval C_1D_1 of J_1. Let x_1 denote the end-point on A_1D_1 of that arc of $(\alpha_{1,2})$ which contains P_1. Then x'_{P_1} shall be the abscissa of $\Sigma'(X_1)$. If H_1 is a point of R_1 on the interval A_1B_1 of J_1, then x'_{H_1} is zero, while, if K_1 is a point of R_1 on the interval C_1D_1 of J_1, x'_{K_1} is one. Suppose Q_1 is a point of R_1 lying neither on the interval A_1D_1 nor on the interval B_1C_1 of J_1. Let Y_1 denote the end-point on A_1B_1 of that arc of $(\alpha_{1,1})$ which contains Q_1. Then y'_{Q_1} shall be the ordinate of $\Sigma(Y_1)$. If M_1 is a point of R_1 lying on the interval A_1D_1 of J_1, then y'_{M_1} is zero; while, if M_1 is on the interval B_1C_1 of J_1, then y'_{M_1} is one. We shall say that the point P_1 of R_1 corresponds to the point P of R, if and only if, $x'_{P_1} = x_P$ and $y'_{P1} = y_P$. In this manner, a continuous one-to-one correspondence Γ is defined transforming the points of R into the points of R_1.

If H_2 is a point of the interval A_2B_2 of J_2, then we shall say that H_2 corresponds to the same point of AB as $\pi^{-1}(H_2)$ corresponds to under Σ^{-1}, while M_2, a point of A_2D_2, corresponds to the same point of AD as $\pi^{-1}(M_2)$ corresponds to under $(\Sigma')^{-1}$. In this manner, we define continuous one-to-one correspondences, T and T', between the points of A_2B_2 and AB and between the points of A_2D_2 and AD, respectively. We shall assign to every point P_2 of R_2 coördinates (x''_{P_2}, y''_{P_2}) in the following manner. Suppose P_2 is a point of R_2, lying neither on the interval A_2B_2 nor on the interval C_2D_2 of J_2. Let X_2 be the end-point on A_2D_2 of that arc of $(\alpha_{2,2})$ which contains P_2. Then x''_{P_2} shall be the abscissa of $(T')^{-1}X_2$. If H_2 is a point of R_2 on the interval A_2B_2 of J_2, then x''_{H_2} is zero, while, if H_2 is a point of R_2 on the interval C_2D_2 of J_2, then x''_{H_2} is one. Suppose Q_2 is a point of R_2, lying neither on the interval A_2D_2 nor on the interval B_2C_2 of J_2. Let Y_2 denote the end-point on A_2B_2 of that arc of $(\alpha_{2,1})$ which contains Q_2. Then y''_{Q_2} shall be the ordinate of $(T)^{-1}Y_2$. If M_2 is a point of R_2 on the interval A_2D_2 of J_2, then y''_{M_2} is zero; while, if M$_2$ is a point of R_2 on the interval B_2C$_2$ of I_2, then y''_{M_2} is one. We shall say that a point of P_2 of R_2 corresponds to a point P of R if and only if, $x''_{P_2} = x_P$ and $y''_{P_2} = y_P$. In this manner a continuous one-to-one correspondence Γ' is defined transforming the points of R into the points of R_2. It should be noted that the correspondence $\Gamma'\Gamma^{-1}$ is a continuous one-to-one correspondence transforming the points of R_1 into the point of R_2 in such a manner that the points of $B_1A_1D_1$ and $B_2A_2D_2$ correspond as under π. This is, however, not necessarily true of the points of $B_1C_1D_1$ and $B_2C_2D_2$.

Let P_2 be any point of A_2B_2, while $\pi^{-1}(P_2) = P_1$. Let Q_1 denote the end-point on C_1D_1 of that arc of (α_{11}) which contains P_1. Let $\pi(Q_1) = Q_2$. Let $(\Gamma')^{-1}(Q_2) = Q$. Let PQ denote the interval from P to Q of the straight line joining P and Q, where $P = (\Gamma')^{-1}(P_2)$. Let $\Gamma'(PQ)$ be the arc P_2Q_2. Do this for every point of A_2B_2. Then let P'_2 denote any point of A_2D_2 and let $\pi^{-1}(P'_2) = P'_1$. Let Q'_1 denote the end-point on B_1C_1 of that arc of $(\alpha_{1,2})$ which contains P'_1. Let $\pi(Q'_1) = Q'_2$, while $(\Gamma')^{-1}(P'_2) = P'$ and $(\Gamma')^{-1}(Q'_2) = Q'$. Let $P'Q'$ denote the interval between P' and Q' of the straight line joining these two points. Let $\Gamma'(P'Q')$ be the arc $P'_2Q'_2$. Do this for every point P'_2 of A_2D_2. In this manner two new sets of arcs (α'_{21}) and (α'_{22}) are obtained having all the properties of the original sets of arcs (α_{21}) and (α_{22}) and having the additional property (6) that if t is any arc of either set with end-points H_2 and K_2 and if $\pi^{-1}(H_2) = H_1$ and $\pi^{-1}(K_2) = K_1$, then one of the arcs of the set $(\alpha_{11}) + (\alpha_{12})$ joins H_1 and K_1.

Using these new sets of arcs, $(\alpha'_{2,1})$ and (α'_{22}), we may define in a manner similar to that used in defining Γ', a new continuous one-to-one correspondence Γ'' transforming the points of R into the points of R_2. It is clear that $\Gamma''(\Gamma)^{-1}$ is a continuous one-to-one correspondence transforming the points of R_1 into the points of R_2 in such a manner that the points of J_1 and J_2 correspond as fixed by π.

[1] Cf. Moore, R. L., *Trans. Amer. Math. Soc.*, **20,** 1919 (176).

[2] Cf. Schoenflies, A., *Math. Ann. Leipzig,* **62,** 1906 (324).

ARE THE SUPERIOR CERVICAL GANLIA INDISPENSABLE TO THE MAINTENANCE OF LIFE?

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The sympathetic nerve fibres which originate in the thoracic part of the spinal cord terminate in the superior cervical ganglia, from which separate bundles of nerve fibres go to the various parts of the head. Experiments with stimulation of the central end of the cervical sympathetic nerves revealed that these nerve fibres control various functions localized in the head. For instance, to mention the more important ones, the contraction and dilatation of the bloodvessels of various parts of the head, the constriction and dilatation of the iris and the secretion of various glands. Section of the sympathetic nerve showed that some of these innervations are in a state of tonus, for instance, the pupils become constricted and the bloodvessels of the ear (to be seen best in rabbits) become dilated. By means of local application of nicotine to the ganglia which paralyzes postganglionic but does not affect preganglionic nerve fibres, Langley has shown that some of the postganglionic nerve fibres of the superior cervical ganglia are simply a continuation of the cervical sympathetic, preganglionic nerve fibres, while other postganglionic nerve fibres have their origin in nerve cells within the superior cervical ganglia.

About sixteen years ago we[1] showed that epinephrin (adrenalin) which, if administered by subcutaneous injection, or by instillation in the conjunctival sac, in normal animals, shows no influence upon the size of the pupil, produces promptly a dilatation of the pupil and a considerable reduction of its responsiveness to light, if the subcutaneous injection or the instillation were made in animals in which the corresponding ganglion was removed. To obtain this result the subcutaneous injection or instillation must be made about twenty-four hours in rabbits and forty-eight hours in cats and dogs after the removal of the ganglion. Cutting of the cervical sympathetic nerves has no such effect. These facts were best illustrated in an experiment in which the superior cervical ganglion was removed on one side and the sympathetic nerve was cut on the other side. A simultaneous instillation of adrenalin in both conjunctival sacs caused a dilatation of the pupil on the side in which the ganglion was removed, but not on the side in which merely the sympathetic nerve was cut. For that reason we designated this pupilary phenomenon as "paradoxical." These experiments revealed that in the normal state, and for twenty-four hours in rabbits, or for forty-eight hours in cats and dogs, after the removal of the superior cervical ganglion, the normal condition of the pupil is maintained by influences which take their origin in nerve cells within the superior cervical ganglion and not in the spinal cord.

In studying at various times certain phases of this experiment, I was sometimes annoyed by failures which were caused by early deaths of the animals. A survey of these failures revealed the fact that they took place in animals in which both ganglia were removed. A survey of the several changes in the functions which stimulation of the sympathetic nerves or section of these nerves bring about, shows that these changes, interesting as they are, affect only peripheral conditions, and do not endanger life. But some of the post ganglionic fibres of the superior cervical ganglion enter the "brain" before they reach the periphery. May not some of these fibres also terminate in some vital point within the brain or the medulla? It is true, in our previous experimental studies, death of the animal was rather an exception. But these studies were essentially confined to the removal of a ganglion on one side only. From experiments upon the vagus nerves we know that cutting of one vagus does not affect the life of the otherwise normal animal, while, when both vagi are cut, with few exceptions, the animal dies within a day or two after the operation. May we not meet with a similar result by the removal of both ganglia? I therefore started a series of experiments in which both superior cervical ganglia were removed.

This series of experiments was made chiefly on rabbits. Although in these animals the superior cervical ganglion is small, it offers the advantage that it is not intimately connected with the vagus nerve, as it is the case in dogs and cats; the ganglion in rabbits may therefore be readily removed without causing injury to the neighboring vagus nerve. (It is not superfluous to state that the present series of experiments were carried out last winter, beginning November 3rd.) To this date more than eighty rabbits were used for the study of various phases of our problem. I shall state the results briefly.

For the chief issue of our problem we used twenty-eight animals in which both superior cervical ganglia were simultaneously removed under ether anesthesia. Of these, twenty-five died as follows: Six died in less than twenty-four hours, that is, the animal which was operated before noon or in the afternoon was found dead next morning at about nine o'clock. Nine animals died in less than two days, five in less than three days, one in four, one in five, one in six, one in eight and one in ten days. The autopsies revealed the presence of pulmonary lesions in all these animals. Three animals survived the removal of the ganglia and were killed after many weeks or months and no pulmonary lesions were found.

Both ganglia were also removed in two cats which died in less than two days and showed pulmonary lesions.

At an early stage of the investigation we met with one failure in a cat; it survived for many days the removal of both ganglia. Suspecting that the operation might not have been successful, it occurred to me, that this might be tested by the action of adrenalin upon the pupil. In the experiments with adrenalin,[2] it was observed that when a ganglion was incompletely removed, instillation or subcutaneous injection of adrenalin did not cause the characteristic upon the pupil. I therefore applied this method to the surviving cat and found indeed that an instillation in a conjunctival sac as well as a subcutaneous injection of adrenalin neither caused a dilatation of the pupil nor a retraction of the nictitating membrane, the latter being one of the striking effects of adrenalin in ganglionectomized cats. The animal was then killed on the tenth day after the operation, and it was established that neither of the ganglia were successfully removed, and that the lungs showed no lesions. Also in one surviving rabbit it was found that adrenalin caused no dilatation of the pupil of the right eye. The autopsy showed normal lungs, proved that the ganglion on the right side was only partly removed. Henceforth an adrenalin test was made on the pupils of practically all animals in which the ganglia were removed. This was also done in the above mentioned three rabbits in which the removal of the ganglia did not lead to the death of the animals. In one of these animals the effect of adrenalin was indeed insignificant; in the other two rabbits, however, the instillation of adrenalin brought a definite dilatation of both pupils. In some rabbits the removal was purposely restricted to one half, or a little more than one half, of one ganglion or of both. As a rule the part to be removed was first crushed and then torn away. In these cases the adrenalin test was made a day or two later; the result was negative, and the animal survived even when both ganglia were mutilated.

However, in testing the success of the operation by the use of adrenalin, it has to be borne in mind that while a negative result prove that the corresponding ganglion was incompletely removed, a positive adrenalin test does not necessarily prove that the ganglia were so completely removed as to bring about the death of the animal. It may be that the number of nerve cells the removal of which is sufficient to bring about a positive adrenalin reaction upon the pupil, is smaller than the number of cells which have to be removed in order to bring about the death of an animal. In other words, the presence and integrity of a small number of nerve cells may be sufficient to maintain life, while the balance of the cells serve only as *factors of safety*—a condition which is especially met with in glands with internal secretion. It follows that in the above mentioned surviving rabbits in which the adrenalin test was positive, we had no assurance that the ganglia were so completely removed as to bring about the death of these animals. (It has to be added that at the autopsy of the three animals neither a minute macroscopical nor a microscopical search was made for possible remnants of the ganglia.)

However this may be, *this series of experiments showed that in about ninety per cent of the animals, the operative removal of both ganglia proved to be fatal to the animals—an experiment which, as far as I know, was never* *made before.* The question now arose: Definite as the result is, is it important, that is, does it prove that the ganglia are really important to the maintenance of life, or was the fatal outcome perhaps due only to the operative procedure and not to the indispensability of some principle provided by the ganglia? Pulmonary lesions were found at the autopsy after the removal of both ganglia; pulmonary lesions are found after cutting both vagi. The ganglia are in the very proximity of the vagus nerves. May not the operative procedure injure both vagi sufficiently to bring about a "vagus pneumonia?"

The following considerations and experimental facts speak against such an interpretation. The possibility of injuring the vagus nerves was taken into consideration at the very beginning of this study. As it was stated before, it was just on account of this possibility that rabbits were given the preference. In these animals the superior cervical ganglion is separate from the vagus nerves, and it may be removed without touching the nerves or pulling at them. With this requirement in mind, I tried to carry out the experiment with care. It is not quite probable that I should have failed on both sides in nearly all my attempts, and that the injury which I might inadvertently have afflicted to both vagus nerves should have been of such an extent as to be equal in effect to a complete section of the nerves. Furthermore, in several experiments one of the ganglia could not be found and only one ganglion was removed. In these cases in which a long search for the ganglion was made the tissues and the corresponding vagus nerve had to be quite severely manipulated. The animals nevertheless survived this procedure. Finally in some experiments, the lower half of both ganglia were removed, as mentioned before. Here the lower half was first crushed and then removed, which required more manipulations than the tearing out of the entire ganglion. These animals survived the operation without manifesting any untoward symptoms.

More direct and decisive evidence was obtained by stimulations of the central end of the vagus nerves after removal of the corresponding ganglion, and after removal of both ganglia. The vagus nerves carry centripetal as well as centrifugal nerve fibres. The centripetal fibres affect and control the respiration and cause a change in the state of blood pressure. Further, the vagus trunk and the superior laryngeal nerve, a branch of the vagus, carry fibres the stimulation of which causes reflex deglutition. The vagus nerve seems to contain also sensory fibres. The stimulation was effected by Faradic currents of various strengths. The procedure was as follows: One vagus nerve was cut and the normal effect of stimulation established; then the corresponding ganglion was removed and the nerve again stimulated, and finally, the other ganglion was removed and the vagus stimulated. I shall state the results very briefly.

Respiration.—Stimulation of the vagus nerve after removal of the corresponding ganglion, or of both ganglia, gave results similar to those obtained before the removal, that is, active respiration, passive respiration (inhibition of the diaphragmatic movements), acceleration of the respiration, or a tetanic standstill of the diaphragm in an inspiratory state, according to the strength of the stimulus used and the individuality of the animal. *Bloodpressure.*—Stimulation of the vagus nerve gave a definite rise, and stimulation of the depressor nerve gave a fall. Rise and fall after removal of the ganglia were—at least— as good as before the removal. *Deglutition.*—When the animal was not too deeply narcotized, stimulation of the vagus trunk produced irregularly one deglutition or more, and stimulation of the central end of the superior laryngeal nerve produced regularly a series of swallows. *Sensation.*—Finally, it may be added that when the animal was nearly out of ether, stimulation with a strong current brought out signs of sensation, which gave the signal for an increase of the anesthesia. *These experiments show unmistakably that the afferent nerve fibres within the vagus were in no visible way affected by the act of removing the ganglia.*

The Centrifugal Fibres.—The efferent nerve fibres of the vagus, which concern us here, are those which innervate the muscles of the larynx, the pharyngeal muscles and the muscular fibres of the esophagus. The laryngeal innervation after removal of the ganglia has not yet been studied. However, clear evidence was obtained that the motor nerve fibres attending the pharynx and the esophagus were functioning in a normal way. *Under moderate anesthesia the inferior pharyngeal muscles were seen to contract with the rise of the larynx, which accompanies the initial stage of degution, and the rise was followed by a normal peristaltic wave in the esophagus.*

The condition of the esophagus at the autopsy is another evidence that the vagus was not affected by the procedure of the removing of both ganglia. In the autopsies made on the twenty-five rabbits which died after the removal of both ganglia, the esophagus was found empty and contracted in fourteen cases; in the other cases the condition of the esophagus was not noted. In other words, in all the cases in which the esophagus was examined, it was found that after removal of the ganglia this tube was empty and contracted. On the other hand, in a series of eighteen rabbits in which both vagi were cut and in which nearly all animals died in less than twenty-four hours, at the autopsy the esophagus was found distended and filled with food in its entire length in thirteen animals; in the remaining five the esophagus was either only partly filled or entirely empty but was not exactly contracted. I wish to point out here that the upper part of the esophagus is partly innervated by pharyngeal nerves which remain intact when both vagi are cut in the neck. Moreover, food may be driven out by the rigor of the esophagus which, after the rigor passes away, may be relaxed again. At any rate, *it is a noteworthy fact that after the removal of the ganglia the esophagus was found* *empty and contracted in all cases in which the state of that tube was looked for, while in thirteen out of eighteen animals in which both vagi were cut, the entire esophagus was distended and full of food, a fact which speaks for the contention that the vagus nerves have not been affected by the removal of the ganglia.*

From the foregoing evidences, I am inclined to conclude *that the death of the animals in which both ganglia were removed was due to the removal of these organs—if I may call them so—and not due to the procedure of the operation. In other words, the superior cervical ganglia contain a principle which is essential for the maintenance of life.*

As to the nature of the pulmonary lesions which were found after death following the removal of both ganglia, I shall only say that nearly in all instances there was a bronchopneumonia which was mostly located in the upper and middle lobes, and a pulmonary edema located mostly in the lower lobes of the lungs. In a few cases there were large and small abscesses in some parts of the lungs, and in some lungs there were plugs in the small bronchi which were apparently connected with the abscesses and which consisted of fibrin, pus and foreign material. In a few instances there were also serous or purulent pleurisies. I shall not attempt to enter into further details. Furthermore, I shall not discuss the question whether there is any difference between the pulmonary lesions observed after the removal of both ganglia and the lesions found in the lungs after section of both vagi. This I can the more afford to do, since the centuries' old question as to the nature of the pulmonary lesions after section of both vagi was recently opened up again by the important researches of Schafer[3]), and the entire question will surely have to be a subject of further investigations.

As to the nature of the mechanism by which the ganglia may control life, I shall, for the present, offer only a hypothesis. At the beginning of this communication, I called attention to the fibres from the superior cervical ganglia which enter the "brain," and put forward the question, whether some of these fibres may not terminate in a vital part of the medulla oblongata. The hypothesis answers this question in the affirmative, namely, that the fibres reach the respiratory centre in which they exert a controlling influence upon the coördinate antagonistic activities of the laryngeal muscles in the function of respiration. As we know, the glottis becomes wider during inspiration and narrower during expiration and is otherwise changing its configuration in the different respiratory phases. The larynx is provided with extrinsic and intrinsic muscles which act alternately as adductors and abductors and are innervated by different fibers running in the trunks of the laryngeal nerves. According to the law which I termed *Antagonistic Innervation* or the law of *Reciprocal Innervation* (Sherrington), the adductor muscles of the larynx become inhibited during the contraction of the abductors, and the latter become inhibited during the contraction of the adductors. Such complicated continuous muscular activities suggest the control of a centre. This central control is maintained in a state of tonus. The hypothesis assumes that the tonus of the central laryngeal control is maintained by impulses coming continuously from the superior cervical ganglia. Some time after the removal of these ganglia the respiratory laryngeal centre loses its tonus and its coördinating activity upon the laryngeal muscles during respiration. The loss of coördination gives rise to results similar in some way to those consequent upon cutting of the vagus nerves. The difference between the two conditions is, that in cutting the vagus nerves, the *peripheral* (*motor*) *innervation of all the muscles* are simultaneously abolished, while after removal of the ganglia *only the central control* is eliminated. In this case impulses may still reach the laryngeal muscles from the respiratory centre, but they will now contract in an incoördinate and harmful manner. Future experiments may reveal that the behavior and appearance of the glottis after removal of the ganglia differ from its behavior and appearance after cutting of the vagi.

This supposed reflex action of the ganglia upon the control of the antagonistic contraction of the laryngeal muscles is somewhat parallel to the action of the same ganglia upon the peripheral organs, for instance, upon the iris. In this instance it controls the width of the pupil by causing either a contraction of the dilator muscle simultaneously with a relaxation of the sphincter pupilae, or, it causes a constriction of the sphincter with a simultaneous relaxation of the dilator.

As to the nature of the origin of the impulses, I compared above the ganglia with the glands of internal secretion. I append here a very brief report of a few experiments, the making of which was stimulated by the mentioned comparison. In four rabbits both superior cervical ganglia were torn away from their upper connections, but were left connected with the sympathetic nerves. In addition, two ganglia from another rabbit were placed deep in the wound before closing it. *All four animals survived many weeks* and were killed later by chloroform; the lungs were found normal. A fifth rabbit was killed six days after the operation in an experiment in which by accident only one foreign ganglion was added. At the autopsy a small abscess was found in the right middle lobe. Probably, the animal would have recovered. These few experiments do not permit any definite conclusion; but the findings are suggestive. If further experimentation should give similar results, the question would be obvious: *whether other sympathetic ganglia also possess some sort of an internal secretion.*

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[1] Meltzer, S. J., and Auer, Clara Meltzer, "Studies on the Paradoxical Pupil-Dilatation Caused by Adrenalin. I.—The Effect of Subcutaneous Injections and Instillations of Adrenalin upon the Pupils of Rabbits," *Amer. J. Physiol.*, **11**, 1904 (28–36); Meltzer, S. J., "Studies on the Paradoxical Pupil-Dilatation Caused by Adrenalin.

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[2] Meltzer, S. J. and Auer, Clara Meltzer, L. c. (30).

[3] Schafer, E. Sharpey, *Quart. J. Exper. Physiol.*, **12,** 1919 (231–301).

OBSERVATIONS ON THE BODY TEMPERATURE OF DRY COWS

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While a knowledge of the normal fluctuations in the body temperature of cattle is of importance to the veterinarian insofar as it may help to detect diseased conditions, it assumes special significance when metabolism experiments on cattle are made with the respiration calorimeter. By means of the latter, the heat eliminated by the animal during a certain period of time, as well as the gaseous exchange between the animal and the atmosphere that surrounds it, is measured. But in order to determine accurately the heat production as distinguished from the heat elimination a correction must be applied for any storage or loss of heat by the animal body and failure to do so might introduce an appreciable error. For example, if at the end of an experimental period the body temperature of an animal weighing 400 kilograms differs by 1° C. from that at the beginning, it means a storage or loss of heat by the body amounting to 332 calories (assuming the specific heat of the body to be 0.83). The data on record regarding the body temperature of farm animals are unsatisfactory for the reason that the observations have not been repeated often enough under different conditions which are known to affect the temperature of man. In this investigation an attempt has been made to secure data regarding the extent and the course of the body temperature variations in cattle and to study some of the factors that might influence them. The investigation covers a period of some sixteen weeks. The animals were two dry cows used at the same time for metabolism experiments with the respiration calorimeter, in which strict control of feeding, environment, etc., is maintained. Several series of observations on the body temperature of the cows have been made with special reference to the study of the following:

(1) Variations in body temperature from about 7.30 a. m. to about 5.30 p. m., including the effect of water drunk.

(2) Thermal gradient in the body.

(3) Variations in body temperature from about 5 p. m. to about 7 p. m., including the effect of the feed.

(4) Effect of the act of defecation and of change in position.

(5) Daily variations in body temperature measured at exactly the same hour of the day.

The observations were made by means of carefully standardized clinical thermometers. During the greater part of the experiment one and the same thermometer was used, while for simultaneous readings of the rectal and vaginal temperatures a second one was used. To avoid any possible error that might be due to the use of two different thermometers, the latter were for a few days interchanged, i.e., the thermometer used one day for measuring the rectal temperature was used the following day for the vaginal and vice versa. This was further checked by using one and the same thermometer for both the rectal and vaginal temperature readings being taken one immediately after the other.

The observations on the thermal gradient were made at a depth of 4, 5, 6, and 7 inches. All other observations were made at a depth of 7 inches. The thermometers, which were originally five inches long, were accordingly lengthened by means of a piece of rubber tubing slipped a half inch or so over the upper end of the thermometer and over the string attached to the loop and stiffened by inserting inside the rubber tubing a strip of copper wire. This added portion was finally covered with adhesive plaster and a rubber ring tightly fitted at the end. This arrangement proved to be very satisfactory. The thermometer could be inserted to the desired depth and there was little danger of breaking it during the insertion. The thermometer was ordinarily allowed to stay in the rectum or vagina for about three minutes and the time and temperature recorded immediately upon its removal.

The results of the several series of observations may be summarized as follows:

(1) The rectal temperature was higher than the vaginal when measured at the same depth of 7 inches, showing an average excess of about 0.3° F. The relative values, however, varied under different conditions, but showed a trend toward parallelism.

(2) A fall in body temperature invariably followed the drinking of water. This fall varied directly with the quantity of water drunk. After the effect of the water drunk in the morning had been overcome, the temperature remained fairly constant till about 2.30 p. m. When no water was drunk the temperature was practically constant in the morning and in the afternoon till about 2.30 p. m.

(3) There was a gradual rise in temperature in the afternoon from about 2.30 p. m. to about 5.00 p. m.

(4) Eating of feed raised the body temperature slightly for about a half hour when the cows received a maintenance ration.

(5) The temperature of the rectum or vagina was decidedly higher when measured at a depth of 6 or 7 inches, than at a depth of 4 or 5 inches.

(6) There was no material difference in temperature between a depth of 6 inches and one of 7 inches, while there was a distinct difference of temperature between a depth of 4 inches and one of 6 inches, thus showing the unreliability of measurements of temperature at a depth of less than 6 inches.

(7) The standing as compared with the lying position of the animal had hardly any effect on the body temperature, but there was some indication that the temperature was slightly affected when measured shortly after the change in position had been made.

(8) There was no difference in body temperature when measured before or after defecation.

(9) Daily fluctuations in body temperature depend to a great extent on the individuality of the cow.

(10) A variation of 0.8° F. in the rectal temperature of the same animal was observed, when measured at the same hour of the day under identical conditions and outside the influence of water or feed, while under the influence of water a difference of 1.3° F. was observed at the same hour on two consecutive days.

A detailed report of the results will appear in the *Journal of Agricultural Research.*

ON THE STRUCTURE OF FINITE CONTINUOUS GROUPS WITH EXCEPTIONAL TRANSFORMATIONS

By S. D. Zeldin

Department of Mathematics, Massachusetts Institute of Technology

Communicated by Edwin B. Wilson, August 2, 1920

Let $X_k \equiv \sum_{1}^{r+q} {}_i\, f_{ik}(x) \frac{\partial}{\partial X_i}$, were $k = 1, 2, \ldots, r, r+1, \ldots r+q$, be the differential symbols of a finite continuous group G of order $r+q$, and let $E_k \equiv \sum_{1}^{r+q} {}_j \sum_{1}^{r+q} {}_i\, \alpha_j C_{jki} \frac{\partial}{\partial \alpha_i}$ be the symbols of the operators of the adjoint of G. We then have

$$(X_i, X_j) = \sum_{1}^{r+q} {}_k\, c_{ijk} X_k$$

$$(i, j = 1, 2, \ldots r+q)$$

and

$$(E_i, E_j) = \sum_{1}^{r+q} {}_k\, c_{ijk} E_k,$$

where the c's are the so-called structural constants. We assume that the group G has q ($\leqq r$) *exceptional* infinitesimal transformations (Lie calls them "*ausgezeichnete*"), which for simplicity are taken to be

$$X_{r+1}, X_{r+2}, \dots X_{r+q}.$$

Then

$$(X_i, X_j) = 0 \quad (i = 1, 2, \dots r+q; j = r+1, \dots r+q).$$

The purpose of this paper is to find the conditions to be imposed on the group G which would make the structural constants $c_{ijr+1}, \dots c_{ijr+q}$ all zero for $i, j = 1, 2, \dots r+q$.

Since the adjoint of G has the same structure as G itself, it follows that

$$(E_i, E_j) = \sum_1^r {}_k\, c_{ijk}E_k \quad (i, j = 1, 2, \dots r).$$

Therefore we may say that if G contains q exceptional infinitesimal transformations, there exists another group, say G', with r essential parameters having the same structure as the group G.

We shall denote the operators of the adjoint of G' by the symbols

$$M_k = \sum_1^r {}_j \sum_1^r {}_i\, \alpha_j c_{jki} \frac{\partial}{\partial \alpha_i} \quad (k = 1, 2, \dots, r)$$

The condition we impose on the adjoint of G' is that it shall have one invariant spread. From that follows that the nullity of the matrix formed by the coefficients of the r differential operators is equal to one, i. e., at least one of the minors of the determinant of the coefficients of order $r-1$ is not zero. But every minor of that determinant is also a minor of the determinant formed by the coefficients of the differential operators of the adjoint of G, therefore in this larger determinant there will be at least one non-vanishing minor of order $r-1$. Furthermore, for $\alpha_1 \dots \alpha_r \dots \alpha_{r+q}$ assigned, the symbolic equations

$$\left(\sum_1^{r+q} {}_i\, \alpha_i \overline{X}_i, \sum_1^{r+q} {}_i\, \beta_i \overline{X}_i \right) = 0$$

have the following $q+1$ independent solutions:

$$\beta_1 = \alpha_1, \dots \beta_r = \alpha_r,\ \beta_{r+1} = \dots = \beta_{r+q} = 0 \quad (1)$$

$$\beta_1 = \dots = \beta_r = 0,\ \beta_{r+1} = 1,\ \beta_{r+2} = \dots = \beta_{r+q} = 0 \quad (2)$$

$$\beta_1 = \dots\dots = \beta_{r+1} = 0,\ \beta_{r+2} = 1,\ \beta_{r+3} = \dots = \beta_{r+q} = 0 \quad (3)$$

. . . .

. . . .

$$\beta_1 = \dots\dots\dots\dots = p_{r+q-1} = 0,\ \beta_{r+q} = 1 \quad (q+1)$$

That shows that there will be in the large determinant at least one non-vanishing minor of order $r-q-1$. Therefore, the nullity of the matrix formed by the coefficients of the operators of the adjoint of G is equal to $q+1$, which means that the adjoint of G leaves invariant $q+1$

independent functions in $r + q$ variables. It should be observed, however, that one of those functions will also be invariant to the adjoint of G' and will consist of the first r variables alone. The following theorem may therefore be stated:

If the adjoint of G' has one invariant, the adjoint of G has $q + 1$ independent invariants, one of which is the invariant of the adjoint of G'.

We assume first that q of those invariants, not involving the one common to both, G and G', are all flats of order $r + q - 2$ in the $r + q - 1$ space and that their common intersection is an $r - 1$ flat. It is then proven that if that flat does not pass through any of the points of the space of the adjoint of G, corresponding to the exceptional transformations of G, we can form new operators, such linear functions of the old ones, of which r, not involving the exceptional operators, form an invariant subgroup of G of order r. This means that the structural constants, whose last subscripts are greater than r, will all become zero. If, however, those q invariants are not flats but algebraic spreads, not necessarily of the same order, none of which passes through the points of the space of the adjoint of G described above, then by considering the polars of the q invariant spreads we prove that the structural constants which have for their last subscripts numbers greater than r can also be made all zero. If, finally, each of the q invariant spreads passes through only one of those points, not necessarily the same for all spreads, then the structural constants described above can also be made all zero.

Detailed proofs and references are given in a paper accepted for publication by the *Annals of Mathematics*.

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CONTENTS.

Page

PHYSICS.—FURTHER MEASUREMENTS OF THE EFFECTS OF PRESSURE ON RESISTANCE..*By P. W. Bridgman* 505

PHYSICS.—CHARACTERISTIC ABSORPTION OF X-RAYS: L SERIES................ *By William Duane and R. A. Patterson* 509

PHYSICS.—ON THE RELATIVE POSITIONS AND INTENSITIES OF LINES IN X-RAY SPECTRA..........................*By William Duane and R. A. Patterson* 518

PHYSICS.—ON A DIFFERENTIAL EQUATION OCCURRING IN PAGE'S THEORY OF ELECTROMAGNETISM....................................*By H. Bateman* 528

MATHEMATICS.—A NEW PROOF OF A THEOREM DUE TO SCHOENFLIES........... *By J. R. Kline* 529

PATHOLOGY.—ARE THE SUPERIOR CERVICAL GANLIA INDISPENSABLE TO THE MAINTENANCE OF LIFE?..................................*By S. J. Meltzer* 532

PHYSIOLOGY.—OBSERVATIONS ON THE BODY TEMPERATURE OF DRY COWS........ *By Max Kriss* 539

MATHEMATICS.—ON THE STRUCTURE OF FINITE CONTINUOUS GROUPS WITH EXCEPTIONAL TRANSFORMATIONS..........................*By S. D. Zeldin* 541

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A SURVEY OF RESEARCH PROBLEMS IN GEOPHYSICS

Prepared by Chairmen of Sections of the American Geophysical Union

CONTENTS

Present status of geodesy and some of the problems of this branch of geophysics. By William Bowie, Chairman, Section (a), Geodesy........................ 545
The problems of seismology. By Harry Fielding Reid, Chairman, Section (b), Seismology........................ 555
The status and problems of meteorology. By C. F. Marvin, Chairman, Section (c), Meteorology........................ 561
Some of the chief problems in terrestrial magnetism and electricity. By Louis A. Bauer, Chairman, Section (d), Terrestrial Magnetism and Electricity..... 572
The problems and functions of the Section of Physical Oceanography of the American Geophysical Union. By G. W. Littlehales, Chairman, Section (e), Physical Oceanography........................ 580
The problems of volcanology. By Henry S. Washington, Chairman, Section (f), Volcanology........................ 583
An outline of geophysical-chemical problems. By Robert B. Sosman, Chairman, Section (g), Geophysical-chemistry........................ 592

These papers were presented at the first annual meeting of the American Geophysical Union, by the chairmen of the several sections of the Union. The meeting was held at the National Research Council in Washington, D. C., on April 23, 1920. The American Geophysical Union is also the Committee on Geophysics of the National Research Council and is the National Committee for the United States of the International Geodetic and Geophysical Union.

PRESENT STATUS OF GEODESY AND SOME OF THE PROBLEMS OF THIS BRANCH OF GEOPHYSICS

By William Bowie

Geodesy is one of the oldest of the geophysical sciences. Originally it embraced only a limited field, consisting of the determination of the shape of the earth and its size and the means by which this was done. But now the subject of geodesy is very closely allied with other matters and there are few geophysical questions that do not, either directly or indirectly, concern the geodesist in the solution of his problems.

Almost every country of the world that pretends to any standing has organized a service for the purpose of carrying on what are called geodetic surveys. As a matter of fact, any survey of the land in which the shape and size of the earth are taken into consideration can be called geodetic; thus, the hydrographic surveys along the coast made for a sailing chart are really geodetic surveys and, similarly, a topographic survey of a large area may be considered to be a geodetic survey; but what I have in mind in speaking of geodetic surveys, are triangulation, base measurements, precise leveling, etc.

By means of base measurements and triangulation, the geographic positions of places are determined which are of immense value in the permanent establishment of boundary lines between nations and political sub-divisions of a nation, for the control of various classes of surveys and maps, and for other engineering purposes.

Practically the whole of Europe has been covered by a system of points whose latitudes and longitudes have been determined by triangulation. This is also the case in Japan and in India. Australia, portions of Africa, and some of the countries of Central and South America have made good starts towards extending triangulation over their areas. In the United States excellent progress has been made in this work. There is triangulation around the entire border and there are many arcs in the interior of the country from which triangulation of precise or lower grades may be extended into every area in which detailed surveying and mapping operations are to be carried on.

It will be of interest to outline briefly the methods for carrying on triangulation or trigonometric surveys.

In the first place a spheroid of reference must be adopted which will approximate the actual shape and size of the earth. Then there must be an initial point whose astronomic latitude and longitude have been observed. Next is needed the azimuth or true bearing of a line of which the station occupied forms one end. After these data are available it is necessary to measure with extreme accuracy the distance between two intervisible points on the earth's surface to serve as a base. After the length of the base is known, additional stations are selected with a view to forming triangles by lines which are clear of obstructions and thus intervisible between their ends. It is, of course, a well known mathematical principle that when a side and the angles of a triangle are known, the other sides can be computed. When these sides have been computed from the base, either one of them can be used as the base for a new triangle. In a similar manner we may extend the computations through a long chain of triangles extending in some cases hundreds of miles across a country.

When the base has been measured and the angles observed and the computations of the triangles have given the lengths of the sides of the triangles, then computations can be made which will give the latitude and longitude of each point and the bearing of each line based on the data for the initial point, at which the astronomic latitude and longitude were observed and the true bearing determined of one line of the triangulation.

This is the simplest case of a geodetic survey where the whole net work depends on some initial astronomic station.

In practice, we have a much more complicated situation. Geodetic surveys have been started from many widely separated astronomic stations and the triangulation based on each has been finally extended until it meets the triangulation nets based on other astronomic stations. When this has been done, it is always found that the geographic positions computed from one astronomic station will differ from the geographic positions of the same points computed from other astronomic stations. We have a very striking example of this on the island of Porto Rico. The astronomic latitude was determined at Ponce, on the southern coast, and at San Juan on the northern coast of the island. The distance across the island, in a north and south direction, between these stations, is approximately thirty miles. The distance between the two stations, as computed from their observed astronomic positions, differs by about one mile from the distance between the two as determined by triangulation. The triangulation distance is correct within ten feet. The cause of this difference in distance as determined by the two methods is the attraction of the mountain mass, forming the Island of Porto Rico, on the plumb line to which the astronomic observations are referred. To the northward of Porto Rico are the deepest parts of the Atlantic Ocean and just to the southward of the island is a vast depth in the Caribbean sea. It can be readily seen that the effect of this distribution of mass in the oceans and in the island and its base is to draw the plumb line towards the centre of the island. This would throw the zenith on the south shore too far to the south and the zenith at the north shore station too far north. The plumb lines projected against the celestial sphere would therefore include a greater arc than they would under normal conditions. This angle is actually about 56 seconds of arc greater than normal.

In carrying on a geodetic survey, as described above, it is necessary to know accurately the elevations of the base lines and triangulation stations above the so-called plane of reference which is mean sea level. Each line that is determined by the triangulation is referred to sea level, that is, the computed distance between any two stations is the distance, along the geoid or sea level surface, between the points where the plumb lines at the two stations pierce this surface. Accurate elevations are also needed in many phases of surveying, in mapping, and in other engineering operations. They are also essential in many branches of science, notably in meteorology, geology, etc.

In order that elevations may have the maximum value to engineers it is necessary that they all be referred to the same, so-called, plane of reference. The one which has been almost universally adopted is mean sea level. It may be defined as the surface which would coincide with the surface of the oceans and their tide water branches if the tide producing forces should cease to act and there were no movements of the air and the barometric pressure were uniform. This surface can be determined at any point on the exposed coast by continuous tidal observations for approximately three years. The mean of the hourly readings of the tide record for such a period is practically free from error. If observations were made for a long period of years, it would be found that the average position of the surface of the ocean, for one three-year period, would agree very closely with the position of the surface for any other three-year period. The difference, if any, would be very minute.

It has been assumed by geodesists that, insofar as engineering work is concerned, mean sea level at each place along an open coast is in the same equipotential surface. This, of course, cannot be absolutely true because of different densities in the waters of the oceans, different barometric pressures, trade winds, and possibly other causes, but the deviation of mean sea level at one station from the equipotential surface containing mean sea level as determined at another station is so small as to be negligible in surveying, mapping, and other engineering work, and for practically all scientific purposes.

After the mean sea level surface has been determined, lines of very accurate spirit leveling are extended inland from the fundamental tidal stations. Each country of the world has done more or less of this work and in the United States we have tens of thousands of miles of this grade of leveling.

When a network of triangulation has been extended over a large area and astronomical latitudes, longitudes and azimuths have been observed at many of the stations, differences between the astronomic and geodetic positions of the stations are found. These differences are due to the deflection of the vertical, as was explained in the case of Porto Rico, and to the fact that the spheroid of reference used in computing the geographic positions of the triangulation stations differs from the actual figure of the earth. The former differences, namely, those due to deflections of the vertical, tend to be more or less accidental in character, while those due to an erroneous spheroid of reference are systematic.

After the triangulation and astronomic work have been done over an extensive area, the shape and size of the mean figure of the earth may be computed with a higher degree of accuracy, possibly, than the spheroid of reference on which the triangulation was originally based. For one hundred years or more we have made closer and closer approximations to the true figure of the earth, as the triangulations of the various nations have been extended. In the earlier of these determinations there was considerable uncertainty, owing to the deflection of the vertical caused by mountain masses and other topographic features. Some of the determinations of the figure of the earth were based on all of the observed geodetic data, that is, triangulation and connected astronomic stations, and other determinations were made to depend only on those data from which stations with large deflections of the vertical had been eliminated.

In the late fifties of the past century, Archdeacon John H. Pratt announced the theory that the geodetic data seemed to indicate that under mountain masses there were deficiencies of matter approximately equal in quantity to the masses that were above sea level. This theory was frequently commented on and studies were made to intepret certain geological features by means of it.

In 1889, Major C. E. Dutton, in an address before the Philosophical Society of Washington, entitled "On some of the greater problems of physical geology,"[1] applied the term "isostasy" to this theory of equal pressure, that the mountains are accompanied by deficiencies of mass under them and that there is an excess of matter under the oceans.

About fifteen years after Dutton gave prominence to this theory, Prof. John F. Hayford, at that time Chief of the Division of Geodesy of the United States Coast and Geodetic Survey, began investigations which showed rather conclusively that the material above sea level is counterbalanced to a marked degree by a deficiency in density of material in the column directly under the exposed material. This column, which is affected by this decreased density, was found by Hayford to extend to a depth of approximately 122 kilometers, if the compensation is assumed to be uniformly distributed. Hayford also found that the column extending approximately 122 kilometers below the bottom of the ocean has a density in excess of normal that will balance approximately the deficiency of matter in the mass of water directly above the column.

This condition of approximate equilibrium in the earth's material at a depth of approximately 122 kilometers was applied by Hayford in determining the figure of the earth from geodetic data in the United States. The results appear in his report published in 1909,[2] which gives dimensions for the earth that are believed to be of a higher order of accuracy than any previously arrived at. This is because the theory of isostasy was considered in this work. It is hoped that in the future the theory of isostasy can be considered in deriving a figure of the earth from all available triangulation data in the world.

In order that this figure of the earth may be computed it will be necessary to have some central office to guide the investigators of the several countries who may assist in the work. Undoubtedly this central office should be under the direction of the Division of Geodesy of the International Geodetic and Geophysical Union, although the central office itself might be located in a geodetic organization of any one of the countries adhering to the union.

In North America great progress has been made in coördinating the geodetic work of the several countries. Some years ago, there was adopted for the triangulation net of the United States what was called the United States Standard Datum. This datum had its designation changed to North American Datum when, in 1913, it was adopted by Canada and Mexico. It is hoped that within a few years a continuous triangulation will be extended into Alaska from the United States and then that territory can also have its triangulation placed on the same datum as the rest of the continent.

A datum may be defined as the adopted latitude and longitude of some station of the triangulation and the azimuth of a line of the triangulation radiating from that station. It also includes the dimensions of the reference spheroid, on which the triangulation is computed.

The advantages of a single datum are, first, that it makes it possible to compute the figure of the earth with greater accuracy and, second, that it avoids the confusion existing in the maps at the frontier of any country when the adjoining countries base their surveys and maps on different datums. Whenever the datums are different, boundary monuments have different geographic positions on the maps of the two countries and there are overlaps, gaps, and offsets in the maps, which frequently result in controversies over the boundary line.

One of the greatest needs in Europe to-day, from a geodetic standpoint, is the adoption of one datum for the whole of its area. This would involve a tremendous amount of revision of the computation and adjustment of the triangulation nets of the several countries, but the resulting benefits would far offset the cost of the work.

Trigonometric leveling is a branch of geodetic work which, though important, is not as much so as triangulation and precise leveling. It is employed in determining the elevations of the triangulation stations in order that the lines observed may be corrected to what they would be if the observations were made at sea level. Theoretically, trigonometric leveling should furnish data from which to compute the shape of the geoid, but this is impracticable owing to our inability to know the exact value of the atmospheric refraction at the instant of observation. Trigonometric leveling is of value in determining the elevations of mountain peaks used as stations or those outside of the main scheme of the triangulation. It is by this method that the elevations of Pikes Peak, Mt. Everest, Mt. McKinley and many others, have been determined.

A branch of geodetic work which has been receiving great attention during the past decade or more is the determination of the intensity of gravity. For a number of years observations were made using reversible pendulums in order to determine the absolute value of gravity. Following this, observations were made with what were called invariable pendulums approximately one meter in length, to get the relative value of gravity or the difference in gravity between two places, one of which was the base station. These results were not particularly satisfactory because the pendulums were swung in the air with rather poor temperature control and the effect of the flexure of the support due to the swinging pendulum was not considered, or, if it was, was not accurately determined.

About 1885, Von Sterneck devised an apparatus which consisted of a quarter-meter pendulum of the invariable type, swung in a case from which the air could be almost entirely exhausted. With this apparatus, the difference in gravity between two stations could be determined with a high degree of precision. There have been modifications of the Von Sterneck pendulums, but only in minor features, the general principle of the apparatus remaining unchanged until this time.

With the short pendulums of the Von Sterneck type it was possible to establish a station in a very short time and at a very low cost. The result has been a great stimulation of effort in this branch of geodetic work. Most of the countries of the world carrying on geodetic operations have adopted this type of pendulum and many stations have been established.

In 1908 there were 47 modern gravity stations in the United States, while to-day there are 276 such stations.

Gravity determinations have a number of uses; one is to furnish a formula by which the value of gravity can be computed for any latitude for use in the physical and chemical sciences; another, to furnish data by which the shape of the earth may be determined; and another, to furnish data for making studies of the theory of isostasy or the deviation from normal densities of matter in the outer portion of the earth.

As a result of the application of the theory of isostasy to the gravity determinations in the United States, the shape of the earth, or rather the amount of its flattening, has been determined by investigations made by the Coast and Geodetic Survey and published in 1917 as Special Publication No. 40 of that Bureau. The reciprocal of the flattening was found to be 297.4. This value is probably the most reliable one in existence to-day. There were also derived from this investigation new constants for the gravity formula for computing the value of gravity at any latitude. This formula was adopted by the U. S. Weather Bureau and by the Smithsonian Institution in the preparation of the latest edition of the Meteorological Tables. In that investigation was also determined the depth of compensation; that is the depth at and below which the materials of the earth are supposed to be in a state of equilibrium. This depth was found to be 96 kilometers, which agrees very closely with the value derived by Hayford in his determination of the figure of the earth at stations in mountainous regions only. It is necessarily true that a depth of compensation can be derived from data in elevated regions with greater certainty than where the land is low. The higher land is liable to be freer from constant or systematic deviation from normal conditions in the immediate vicinity of a station.

The gravity observations not only give a means of determining whether there are regional deviations from normal densities in the outer portion of the earth under the continents and oceans to cause equilibrium at the depth of compensation, approximately 96 kilometers, but they also afford an opportunity to detect abnormal densities in the materials of the earth which are close to the surface but below sea level. Recent investigations in the United States and in India show a decided relation between the value of gravity and the Cenozoic geologic formation. This formation is approximately 10% lighter than the material in the older geological formations. Pendulum observations may be a means of indicating the approximate depth to which the Cenozoic formation extends at any place.

Gravity observations also indicate a relation between the value of gravity and the Pre-Cambrian formation. In this formation the material is, in general, somewhat heavier than normal, and when a gravity station is established at or near Pre-Cambrian rocks the value of gravity is found in most cases to be greater than normal. It is not improbable that a knowledge of this relation between the Pre-Cambrain formation and the gravity anomaly may make it possible to discover crystalline rocks or others that are extra heavy which may be covered by materials of normal or lighter densities. If this proves to be the case, it may be possible to locate areas underlaid by dense rocks close to the surface of the ground, which may have an economic importance in connection with boring for oil. Much money has been lost by oil prospectors in drilling in regions which have been found by them to be underlaid by crystalline rocks.

It is seen that gravity determinations have much more than a purely geodetic value. The results obtained from them will undoubtedly have a very great effect on the science of geology.

There are geophysical problems that would be considerably cleared up if the value of the intensity of gravity over the oceans could be determined accurately. Such observations would make it possible to determine whether the outer portion of the earth under the ocean is in the same isostatic condition as has been found under the continents. It would seem probable that isostasy obtains to the same extent under the oceans as under the land, but it has not been possible to prove this in the absence of accurate gravimetric determinations on the ocean.

It is true that gravity observations have been made over the ocean, but the apparatus, in each case, lacked the necessary precision to give the value of gravity with a sufficiently high degree of accuracy. It is probable that improvements will be made in the apparatus for determining gravity at sea and when one has been perfected to such an extent that it will give results of adequate accuracy, a long step forward will have been made in geodesy.

The theory of isostasy has some corroborative evidence in the form of a relation between the elevation of the ground and the density of the igneous rocks. Recently Dr. H. S. Washington delivered a paper at a meeting of the Geological Society of Washington in which he showed very conclusively that the higher the elevation at which igneous rocks are found, the smaller the density of such rocks. This tends to confirm the theory of isostasy which postulates the higher the land the less the density in the column under it, down to some indefinite depth called the depth of compensation, approximately 96 kilometers.

It is hoped that these studies by Dr. Washington may be continued by him and by others, for their results will undoubtedly be of great value to geophysicists in general and especially to geologists working in the subject of isostasy.

Another matter that is assuming importance in geodesy is the determination of earth tides. These were studied by the International Geodetic Association, a station having been established for this purpose in Europe some years ago. The apparatus used was of sufficient delicacy to show that there is an earth tide caused by the tide producing forces of the moon and sun, but it could not be considered that the results obtained were entirely satisfactory. This condition has been changed, as a result of the construction of an earth tide apparatus by Prof. A. A. Michelson, of the University of Chicago. This apparatus consists essentially of two tubes placed horizontally in the ground, one in a north and south and the other in an east and west direction. These tubes are partially filled with liquid and the tide producing forces cause an inclination of the liquid surfaces, producing the phenomena of tides. The actual amount of tilting of the surface of the liquid is determined by the interferometer method.

The differences between the tides, as observed in the horizontal tubes, and the theoretical tides that would be produced on the earth, if it were rigid, give the measure of the actual distortion of the earth itself. This apparatus of Prof. Michelson is considered to be entirely satisfactory and it is hoped that it may be used nationally and internationally in determining the value of the earth tides at different parts of the earth and under different geological conditions.

From the result of the earth tides, values can be deduced for some of the physical properties of the material of the earth, such as elasticity and viscosity, at different distances from the surface. A knowledge of these will be of great geophysical value.

Seismology is a geodetic subject in the sense that it throws light on the constitution of the interior of the earth and thus assists in investigations of the shape of the earth. As was said in the first part of this paper, geodesy originally had as its function the determination of the figure of the earth, but in order to ascertain why the figure of the earth is such as we find it, a knowledge of the physical characteristics of the material composing the earth is essential. Seismology in connection with the investigation of the earth tides should furnish much information regarding the physical properties of the material of the earth at various depths.

A subject that is of interest to many geophysicists as well as to astronomers is the variation of latitude which has been studied for a number of years by the International Geodetic Association. While certain parts of the variation have been found to follow certain laws of change there are still apparent irregularities not yet explained.

From investigations dating back to Euler's time it was at first supposed that the latitude variation, if found, would have a period of about ten months, this being on the supposition of a rigid earth. It is found that the principal part of the polar motion has a period of 14 months. From this fact it is inferred that the earth is not rigid, and from the numerical amount of the lengthening of the period we may draw inferences as to the elastic properties of the earth as a whole; these inferences agree in a general way with what has been found by investigations along other lines. We naturally expect the shifting of the pole to be accompanied by a shifting of the ocean waters responding to the change in the centrifugal force of rotation. This latitude-variation tide has already been studied to some extent without in any way exhausting the subject.

This problem of determining the variation of latitude at observatories is really an astronomic one, although it has a geodetic bearing. The figure of the earth, determined from triangulation and astronomic data, is affected though very slightly by this variation. When it was found desirable to carry on systematic observations on an international scale, there was no astronomic organization with sufficient funds to do the work. The problem was presented to the International Geodetic Association, which had a source of funds not available to the International Astronomic Society. The result was that the International Geodetic Association undertook the work and has carried it on continuously, even during the war, for twenty years or more. The work must not be discontinued nor should there be any break in the observations at the international stations in Italy, Japan and the United States.

Geodesists are interested in the science of volcanology or at least in certain phases of the work in that science which may throw light on the constitution of the material, with densities and temperatures, in the outer portion of the earth, and the cause of volcanic activity. It is believed that these data will supplement the information obtained from the several other lines of investigation such as gravity determinations, earth tides, seismology and variation of latitude.

There is another subject which is of particular importance to geophysicists and, I might say, of very great interest to geodesists. This is what may be called geo-mechanics. It would embrace an investigation to show the relation of forces to movements of material, the origin of forces, and their causes. It would really combine the subjects of isostasy, earth tides, variation of latitude, seismology, and certain phases of volcanology. The data collected in those branches of geophysics should be considered in connection with geology in order to interpret in the light of present knowledge the various movements that have taken place in the earth. Up to this time, it is believed that these data have not been given due consideration and weight in the study of the earth, its structure, forces, etc.

Some of the subjects that have been discussed above are only incidentally related to geodesy, but the results of investigations in them are certainly of great value and interest to geodesists and it is believed that there should be committees of the Section of Geodesy of the American Geophysical Union to consider these questions and be able to inform the members of the Section at any time as to the progress made in these branches of geophysics. There are several of the subjects which have sections of their own, for instance, those of Seismology and of Volcanology.

The Geodetic Section of the American Geophysical Union will not attempt, nor does it desire, to pre-empt any field of geophysics nor will it encroach on the rights and privileges of any other organization. It will simply attempt to keep informed of what is going on in the various fields of geophysics which are of interest to geodesists and it hopes that it may from time to time give some assistance to other organizations in suggesting problems or lines of research. On the other hand, it stands ready to receive suggestions and help from any other organization dealing in any branch of geophysics.

[1] *Bulletin of the Phila. Soc. of Washington,* **II** (51–64).

[2] Supplemental Investigations of the Figure of the Earth and Isostasy from Measurements in the United States.

THE PROBLEMS OF SEISMOLOGY

By Harry Fielding Reid

Although earthquakes have received sporadic attention for some time in the past, their systematic study may almost be said to have begun with Mallet about the middle of the last century; and it was only towards the end of the century that instruments delicate enough to register distinct shocks were developed, making it possible to gain some knowledge of the characteristics of earthquake vibrations and the velocities with which they are propagated through the earth. But, notwithstanding the shortness of the time during which these studies have been in progress, the science has been well outlined and first approximations to the solutions of many of its problems have been obtained. The time has now arrived for more concentrated and still more systematic attacks, from which important results may be confidently expected.

We can summarize the general state of our knowledge of earthquakes as follows:

Nearly all earthquakes are due to the sudden fracture of the rock of the earth's crust, which has been strained by slow earth movements beyond its strength. Strong vibrations are set up at the fractured surface at the time of the fracture, just as vibrations are set up whenever any solid is broken. The earth consists of solid material, which is necessarily elastic, and therefore these vibrations are transmitted through it as elastic waves. There are two kinds of elastic waves: normal waves, where the movement is in the direction of propagation, and transverse waves, where the movement is at right angles to the direction of propagation. These two kinds of vibration advance with different velocities. Their velocities near the earth's surface are about 7 and 4 kms. per second, respectively; but the deeper they penetrate below the surface the faster they go. Therefore, from the characteristics of elastic waves their paths are curved and concave upwards. These important results have been obtained by a study of the time necessary for the two types of waves to pass from the place of their origin to stations at different distances where delicate instruments are installed, which record the time of their arrival. It may seem remarkable that the paths followed by the rays and the velocity in different parts of the paths could be determined merely from the time required to arrive at a number of places on the earth's surface; but, by the help of some rather abstruse mathematics, it can be done. One interesting conclusion which can be drawn from the passage of the transverse waves through the body of the earth is that the earth is a solid and not a liquid sphere; for transverse waves can be transmitted only by solid substances.

There are other waves which are transmitted along the surface of the earth; they must be started in some way when the body waves, mentioned above, arrive at the surface, but we do not know just how near the origin their starting place is.

In many instances submarine earthquakes give rise to great water waves, which have been known to travel from one side of the Pacific Ocean to the other. It has been the general belief that the first indication of these waves along a coast is marked by the withdrawal of the water; and the great elevated wave follows. Although this order is certainly frequent, it is not general, and in many cases the elevated wave is first to appear.

This is a very summary sketch of what has been learned about earthquakes in the comparatively short time that they have been systematically studied; but it will serve as a basis from which to point out the problems which are now pressing for solution.

For our present purposes we can classify seismological problems into:

A. World-wide.
B. Regional.
C. Theoretical and Auxiliary.

A. World-wide Problems

The noted seismologist, the late John Milne, has estimated that fifty or sixty earthquakes, strong enough to be recorded on seismographs in all parts of the world, occur every year. The study and comparison of the seismograms made at these scattered observatories requires international coöperation, and this will be provided for by the new International Geodetic and Geophysical Union.

The problems facing us are numerous, but perhaps the most important is the accurate determination of the time of the earthquake waves to pass from their origin to points on the earth's surface at various distances from it. We already have a fair knowledge of these times, but need more accurate determinations. This information serves two main purposes: it enables us to determine the origins of earthquakes when they occur in inaccessible regions or under the sea, and thus gives us information of the regions of seismic and therefore of geologic activity; and it enables us to trace out the paths and the velocity of the earthquake waves through the earth, thus throwing light on the physical condition of the earth's interior.

The records of earthquakes at distances of more than about 110° from the origin have proved very unsatisfactory. It is still uncertain whether this is due to the imperfections of our observations or, as Oldham thinks, to the existence of a central core in the earth having different physical properties from the material surrounding it, which materially changes the direction of propagation of the seismic vibrations passing through it, and may even damp some of them out entirely. This very interesting suggestion deserves to be tested.

Professor Wiechert and his collaborators have announced the existence of several shells in the earth where the velocity of propagation of the waves suddenly changes. These results are based on the form of the transmission curve; but, unfortunately, this form is not known with sufficient accuracy to justify the conclusions.

Do earthquake waves travel under the continents and the oceans at a different rate? We can say that the difference is certainly small, but we cannot say that there is no difference. These are some of the problems dependent for their solution on increased accuracy of the transmission curve.

The comprehensive work of the Count de Montessus de Ballore on the distribution of earthquake centers on the earth, has shown that 95% of all earthquakes on land occur in two broad zones, one surrounding the

Pacific Ocean and one passing through the West Indies, the Mediterranean, and the East Indies. These zones are the regions where sediments were accumulated in middle geologic times, known as the Mesozoic Age; after consolidation into rock they have been elevated and in places they now form the highest mountain ranges in the world. De Montessus de Ballore's great work need not be repeated; what is now needed is a detailed study of the distribution of earthquake origins in special regions, and this comes under our third class.

But the distribution of earthquake centers under the seas is still in an embryonic state. A good beginning in the study of this problem has been made by Rudolph, but far more data are needed and they can only be supplied by the records of many widely-spread observatories.

Much study has been given to periodic variations in earthquake activity and its possible relation to variations of latitude, to the positions of the sun and moon, to the seasons, etc. Although this line of investigation has not yielded satisfactory conclusions and, in the opinion of some competent seismologists, is not destined to be fruitful, others believe in its possibilities. If it is to be pursued successfully it is important that a good catalogue of submarine earthquakes be added to the catalogue of those occurring on land. There are a sufficient number of seismological observatories in the United States to supply the data required from this country towards the solution of these world-wide problems, though perhaps a new station somewhere in Texas and another in Montana or Idaho might be of advantage. The effort should be to improve the stations already functioning rather than to increase their number. The examination of many earthquake records has convinced me that not a few observatories are provided with poor clocks, or that insufficient care is given to them. Better time-keeping will greatly increase the accuracy of the records. Some of the best observatories should have a more complete equipment. They should have seismographs of high magnifying powers to catch the very feeble movements, and seismographs of low magnifying power, that the characteristics of the stronger movements may not be lost by too wide a range of the recording point. They should have instruments to record vertical motion, a component that has received far too little attention. I believe there are but two vertical component instruments in this country.

When we consider the important area covered by the Pacific Ocean, we find that there are stations in Japan, Formosa, the Philippines, Australia, New Zealand, Chile, Panama, Mexico, California, and Sitka; and on the islands of Oahu, H. I., Guam, and Samoa. The last station was a branch of the Physical Institute of Göttingen and should not be allowed to fall into desuetude. A station on one of the Aleutian islands would also gather valuable information.

Knott has shown that the greater part of the energy of an earthquake comes to the earth's surface within a short distance of the epicenter, and 95% within a distance of 90°. When we consider that the earthquake originates at a fault and that the mass movement there may be horizontal, inclined, or vertical, we realize that the energy sent out in different azimuths, and indeed at different vertical angles, may be dependent on the direction of the fault and of the mass movement, but no information has yet been obtained on this subject.

B. Regional Problems

The accurate surveys made by the United States Coast and Geodetic Survey of parts of California before and after the earthquake of 1906, made it very plain that slow movements of the earth's crust during some scores of years finally brought about a rupture of the rock along the San Andreas Fault, and that the two sides of the fault flung back in opposite directions under the elastic forces set up in the rock by those movements. But no information was gained regarding small ruptures taking place preliminary to the main break, nor regarding the location of the many sudden movements after the shock, causing what are known as aftershocks. A methodical study of a limited area where small shocks are frequent and strong shocks are occasional would throw much light on the sequence of events leading up to the rupture producing a strong shock, on the way in which the rupture is gradually enlarged, on the depth to which it extends, and on the order according to which quiet is gradually restored. It seems not impossible that the location of the preliminary shocks might serve to block out the fault along which a rupture was about to take place. Davison's study of the shocks preliminary to the severe Mino-Owari earthquake of Japan in 1891, seems to encourage this idea. But it is important that the actual origins of the shocks should be determined; it is insufficient merely to list the places where the shocks were felt; this last method has led to serious error in the past. By placing simple monuments in properly selected positions and fixing their relative positions and heights from time to time by careful surveys, the slow movements of the crust and the relation of these movements to earthquakes could be discovered. Information could also be gained regarding the relation of the geological structure and the physiography of a region to its seismic activity.

The velocity of transmission of earthquake vibrations in the immediate vicinity of the origin and near the earth's surface, can only be satisfactorily studied by placing a number of seismographs at comparatively short distances apart, and not far from the origin of the shock. A good determination of this velocity would add precision to many seismological problems.

There are many other problems that come in this class. Microseisms, small vibrations continuing for hours, frequently interfere with the satisfactory recording of earthquakes. The origin of these vibrations is not understood. They may be connected with meteorological conditions. The actual movements of the ground, velocity and direction of propagation of the microseisms should all be studied and compared with the direction of the wind and the direction of the barometric gradient. The concentration of a number of similar seismographs in a limited area would furnish the proper conditions for the solution of this problem.

The influence of the foundation, rock or alluvium, upon the intensity of the vibrations; the relative intensity of the movement in a mine and above ground, due to local or to distant earthquakes, etc., are examples of regional problems before us.

The study of the actual effects of strong earthquakes usually follows somewhat different methods. It requires the examination of the region where the shock has left visible marks by a competent seismologist. The experiences of persons present must be collected; the damage to buildings and other structures noted; and the relation of the earthquake to the geology of the region determined. The slow surface waves in alluvium, sometimes called "gravity" waves, are still a great enigma, and require special attention. A few portable seismographs should be available and should be placed in suitable positions to record and locate the aftershocks. If in addition to these field studies, instrumental records before, during, and after the strong shock should have been made the two kinds of information collected would not only supplement but would add greatly to the value of each other.

C. Theoretical and Auxiliary Problems

The results obtained by world-wide and by regional studies must be woven together into a unified whole and the fundamental laws of the sciences deduced. This kind of work is only done occasionally, and then by individuals who have developed an unusual insight into their science. There are many problems, however, which do not come either under the world-wide or the regional problems, which are important either in helping the solution of these problems, or in elucidating the meaning of the data collected. For instance, the theoretical and experimental study of seismographs is of great importance. It looks to a better interpretation of the seismograms, and to improvements of the instruments, or the development of new forms of instruments. This is work to be done in the study or in the laboratory, but should be in close touch with observations in the field. In particular, we need an instrument that will give a correct record in the shaken area of a strong earthquake.

Then there are many problems, such as the actual genesis of the vibrations, the transmission of body and surface waves, the changes in the character of the waves on reflection, the reasons for differences in the instrumental records of different earthquakes, the peculiarities of earthquake sea-waves, etc.

Although the various seismological problems have been, for convenience, classified into three groups, it must not be inferred that they are sharply defined and distinct; each group will contribute to the problems of the others. Instruments that are primarily intended for world-wide problems will yield information bearing on local problems in their neighborhood; and instruments meant for local problems will also record the world-shaking earthquakes.

Relations to Other Sections of Geophysical Science

For convenience we divide science into branches, influenced largely by differences in the methods used. But nature knows no such dividing lines; and we find that many problems can be solved only by the co-operation of several branches. This is especially true of the geophysical sciences. The special section of seismology looks to all the other sections for help; to geodesy to determine the slow earth movements, horizontal or vertical, that lead up to earthquakes, and the sudden movements that take place at the time of the shock; to meteorology to tell us the atmospheric conditions when microseisms are prevalent; to physical oceanography for information about the deeps of the oceans, along whose borders submarine shocks are common.

The independence of earthquakes and volcanoes has been strongly emphasized during the last thirty years, because nearly all the strong earthquakes were unaccompanied by volcanic phenomena of any kind; and, on the other hand, the great volcanic outbursts, such as Krakatoa in 1883 and Mt. Pelée in 1902, caused only insignificant earth tremors. These facts cannot be contraverted; and still we must not forget that the volcanic belts lie in or near the great earthquake zones; and there may be some common cause of both classes of phenomena.

The influence of earthquakes on suspended magnets may be assigned, with much confidence, merely to the mechanical vibrations. But may not the altered state of strain, in the neighborhood of the generating fault, change the magnetic condition of the rock there if the rock contains magnetic minerals, so that the general magnetic field might be modified? Or may not the passage of violent earthquake waves through magnetic rock alter its magnetization, especially if that magnetization was assumed at some time when the earth's field was different from what it now is?

Seismology does not ask without giving. It can contribute its conclusions to the other branches of science and help them in the solution of their problems.

THE STATUS AND PROBLEMS OF METEOROLOGY

BY C. F. MARVIN

Meteorology is one of those sciences which developed on the side of practical application for the welfare of mankind to a far greater extent than the development on the scientific or pure theory side of the problem. As a result, meteorologists responsible for the practical applications to human welfare are at a distinct disadvantage on account of the imperfect and incomplete theoretical development of the science.

The United States was distinctly the pioneer among all nations in officially and formally creating a meteorological service. This was done by a joint resolution of Congress in 1870, which provided that advices, warnings and forecasts concerning future weather conditions should be prepared and issued for the benefit of agriculture, commerce and navigation.

The greatest achievement of the Weather Bureau is doubtless that of creating the present organization of a service of applied meteorology from the nucleus resulting from this early legislation.

Taking the U. S. Weather Bureau as a prominent example of a well-developed, thoroughly organized agency for the application of the principles of meteorology to the welfare of man, it seems appropriate to outline below, in the briefest possible manner, the principal activities and subdivisions through which this public service is rendered.

THE WEATHER BUREAU

A Daily Service of the Science of Meteorology Applied to Human Welfare.—When Congress created the Weather Service in 1870, the primary object was to benefit and protect navigation on the Great Lakes and Atlantic coast by advance warnings of dangerous storms. Later the issue of flood warnings was added to its duties. To-day there is scarcely any important industry or activity of the nation which is not to a greater or less extent influenced by weather conditions, and therefore needs the advices, information and economic benefits which flow from the full and efficient administration of all its duties.

These benefits are of great variety and the information, advices and warnings of the Weather Bureau serve the immediate needs of practically every interest and industry of the nation. Great atmospheric disturbances visit with relative frequency one section of the country or another. Violent storms, cold waves, frosts, freezes, hurricanes, floods, heavy snows, and the like, repeatedly cause destruction of property, such as shipping on the Great Lakes and coastal waters of the nation, and, in recent times, the lives and property engaged in aerial navigation. Crops and farms in flooded districts are laid waste. Lambs, livestock, and meat animals in the stock ranges of the west are killed by blizzards and cold waves. Orchard crops, truck gardens, and vineyards everywhere are damaged by frosts and freezes. By forecasts and warnings, issued and disseminated well in advance, great economic benefits and saving accrue to the nation through the precautionary measures which can be taken on timely advices to minimize or ward off injuries which otherwise inevitably attend the great atmospheric phenomena mentioned.

The following comprise the principal items of service:

SUMMARY OF WEATHER BUREAU ACTIVITIES

1. Weather Forecasts and Warnings

Weather forecasts and warnings are based upon simultaneous observations of local weather conditions taken daily at 8 a.m. and 8 p.m., 75th Meridian time, at about 200 regular observing stations scattered throughout the United States, and upon similar reports received daily from points in the West Indies, Central America, Hawaiian Islands, Alaska, Canada, Bermuda, and a few other parts of the Northern Hemisphere. The reports are received by telegraph, charted, and the forecasts and warnings deduced therefrom are issued from district centres located at Washington, D. C., Chicago, Ill., New Orleans, La., Denver, Colo., and San Francisco, Calif. The forecasts and warnings are of varying classes as follows:

(*a*) *Daily Forecasts and Weather Information.*—These forecasts are issued twice daily in the a.m. and p.m. and are telegraphed to about 1,600 principal distributing points and thence they are further disseminated by telegraph, telephone, radio and mail to several thousand communities. They are furnished also to the press association and newspapers and are published by practically every daily newspaper in the United States, thus reaching all sections and every class of people. The weather observations on which the forecasts are based are printed in map and bulletin form and mailed or delivered to commercial, marine, agricultural, industrial organizations, educational institutions, etc.

These maps and bulletins are prepared, printed and distributed within three hours after the observations are taken.

The manifold uses to which the daily weather forecasts and weather information are applied are impracticable of enumeration as the life and activity of the whole population is more or less affected by the weather and receives more or less benefit therefrom.

(*b*) *Weekly Forecasts.*—Forecasts are made each Saturday for the six days beginning with the ensuing Monday. These forecasts are made separately for nine districts covering the entire United States, and necessarily are couched in general terms. They are immediately telegraphed to certain designated centres whence they are further disseminated by telegraph, telephone, and mail, and are published in Sunday and Monday newspapers following the day of issue.

These forecasts are intended more particularly for the agricultural interests, as a guide to their activities during the growing season; and the Weather Bureau is in receipt of many communications testifying to their great value.

(*c*) *Shippers' Forecasts.*—Forecasts of temperature expected within the following 24 to 36 hours that will be injurious to perishable products in shipment are made by officials in charge of the stations located in large commercial centres. They are issued only when temperatures of 32 degrees or lower, or 90 degrees or above, are expected.

Special distribution of these forecasts is made to railroads and shippers of perishable products. Shipments are to a large degree regulated by these forecasts. The saving affected thereby is enormous.

(*d*) *Storm and Hurricane Warnings.*—This service consists of the issuance to shipping interests on the Atlantic, Pacific and Gulf coasts and in the waters adjacent thereto and on the Great Lakes, of warnings of the approach of storms dangerous to shipping, and to the coasts of the South Atlantic and Gulf States and adjacent regions of the approach of hurricanes. These warnings are disseminated by means of flags by day and lanterns by night, displayed at about 425 ports. In addition, the warnings are telephoned and telegraphed to practically all maritime interests and broadcasted by wireless to ships in the threatened regions. So nearly perfect has this service become that scarcely a storm of marked danger has occurred for years of which ample warnings have not been issued from 12 to 24 hours in advance.

The sailings of the immense number of vessels engaged in our ocean and lake traffic are largely determined by them. Warnings displayed for a single hurricane are known to have detained in port on our Atlantic coast vessels valued, including their cargoes, at over $30,000,000. Fewer dangerous storms than usual occurred during 1919. However, one unusually severe one occurred on the Lakes in November, accompanied by wind velocities of over 80 miles an hour. Owing to the ample advance warnings not a single marine casualty was reported. Only one tropical hurricane occurred, namely, that of September 6–14, 1919, that passed over southern Florida, traversed the Gulf of Mexico and struck the coast of Texas near Corpus Christi. It was attended by large losses in life and property that were inevitable, but the saving to ships and other property as well as lives was incalculable because of the warnings that were distributed well in advance of the storm. This hurricane was of extraordinary intensity and the efficiency of the warnings was so complete that the portion of the Gulf of Mexico which it traversed was practically cleared of shipping; otherwise many ships valued at millions of dollars would have been caught in the storm. Few, if any, ships caught within the storm centre could have survived. This efficiency of service added to the difficulties of the forecaster, because he was unable to secure wireless reports from the region of the storm centre which were so necessary for an accurate determination of the hurricane's direction and progress.

(*e*) *Cold Wave Warnings.*—These are warnings of sudden and destructive temperature changes that frequently sweep across the country during the winter season. They are issued from 24 to 36 hours in advance and are disseminated throughout the threatened regions by means of flag displays, by telegraph, telephone and mail.

The uses made of these warnings are manifold. They are especially valuable to railroads in the making up of trains and the maintenance of schedules, to shippers of perishable goods, cattle growers, and innumerable business organizations. The warnings issued in a previous year for a single cold wave of exceptional severity and extent resulted, according to reliable reports, in a saving of over three and a half million dollars. No unusually severe cold waves occurred during 1919, although warnings were issued on about 30 different occasions for moderate cold waves.

(*f*) *Frost Warnings for Truckers, Tobacco, Cranberry and Sugar Growers.*—These warnings are issued whenever conditions indicate the occurrence of damaging frosts. The growers take advantage of these warnings to protect their crops by the various means in vogue. In many instances the growers are dependent upon the Weather Bureau for these warnings as without them their industries would be too hazardous to be profitable. This is especially true of early trucking in the South Atlantic and Gulf States. During the year 1919 rather fewer instances than usual of weather conditions necessitating these warnings occurred; but during the year previous it is of record that warnings of a damaging freeze saved nearly one-third of the entire sugar crop in Texas and Louisiana by enabling the growers to windrow the standing cane.

Special frost warnings are sent to all parts of the tobacco-growing section in Wisconsin and Connecticut. Information of current weather conditions during the growing season is also supplied to the other large producing areas.

Frost warnings are sent to the principal cranberry-growing sections of the country, which enable growers to protect their crops by flooding or other protective means. They are indispensable to this industry.

(*g*) *Fire Weather Warnings.*—These warnings are issued whenever conditions are favorable for hot, dry winds, which are favorable to the inception and spread of fires in the National and other forested regions. They are distributed principally to Forest Service officials and others charged with forest protection.

The warnings aid in the protection of the forests by enabling crews and apparatus to be ready for action. Numerous testimonials of their value have been received. several in the form of resolutions passed at the conventions of Forestry Associations, urging appropriations.

(*h*) *Forecasts and Warnings for Stock Growers.*—This service covers the western and northwestern portions of the country and consists of warnings of cold waves, high winds and heavy snows. These warnings enable stock men to make proper provision for the protection of stock exposed on the ranges. They are of especial value to sheep men during the shearing and lambing periods as heavy losses sometimes occur when proper protection has not been provided, sometimes as much as 50 per cent of individual flocks. In the States of Washington, Oregon, and Idaho alone there are two million sheep valued at $12,000,000, exposed to the weather and the sheep men depend largely on the Weather Bureau for information as to when protective measures should be taken.

(*i*) *Fruit Frost Warnings.*—This service is maintained in a number of important fruit growing sections of the country, particularly in California and Oregon. A killing frost without warnings or protective measures means disaster to the crops. So important has the work become that a trained meteorologist is assigned to important districts during the frost danger season to determine how low the temperature will fall and to advise as to the extent of the protective measures that must be taken.

(*j*) *Forecasts for Alfalfa Growers.*—Special warnings are issued for alfalfa seed growers in the principal seed producing sections of the West, and for alfalfa hay making in nearly all sections from the Mississippi Valley westward. Frosts are frequent at about the time alfalfa seed is maturing and are very damaging to unharvested crops. It is estimated that near harvest time, the crop increases in value at the rate of about five dollars an acre during each twenty-four hours; if it is cut too early, much loss is sustained but a heavier loss may result from frost if not cut early enough. On the receipt of a warning, the growers vigorously push harvesting operations to save as much seed as possible.

In addition to the seed growing service, fair weather warnings are sent to all the principal alfalfa growing districts of the West, during the hay-harvesting period. In Oklahoma alone two thousand alfalfa growers receive these special forecasts which are distributed through the County Agents of the Farm Extension Service.

(*k*) *Fruit Spraying Forecasts.*—A special fruit spraying service is maintained in New York and other important fruit growing districts, by which special forecasts are made for the benefit of fruit growers during the spraying season. The effectiveness of spraying depends largely upon the weather conditions at the time and immediately after spraying, and this service is of much value in supplying information as to the best time at which this work should be done. This service has been so successful that requests are on file for the establishment of similar work in a number of districts not now covered.

2. HIGHWAY WEATHER SERVICE

This service consists of the issue of bulletins, daily, semi-weekly, or weekly, depending on locality, giving the conditions of the main highways in the section in which the distribution centre is located, and their passability as affected by rains, heavy snows and other conditions. Detours and other pertinent information are included. Bulletins are issued at about 50 stations in 30 states. These bulletins are used by travelers in determining journeys and routes, in the dispatching of truck, determining loads, etc.

3. AEROLOGICAL INVESTIGATIONS; FORECASTS AND WARNINGS IN AID OF AVIATION

This work involves the obtaining of free air data in different parts of the United States for the purpose of making these data immediately available for the information and assistance of the forecasters of the bureau, army, navy, and post office fliers. The observations also are summarized, studied, and the data published for the purpose of furnishing reliable information as to free air conditions for the benefit of aviation in general and especially for the aviation and artillery services of the army and navy.

Advices to the Navy crews, for the trip from Newfoundland to the Azores, were based to a considerable extent upon free air observations over the ocean. The N. C. 4 reached her destination and the other two landed close enough to the Azores so that their crews were rescued. Without the wind assistance as predicted none of these seaplanes could have flown as far, and the crews would probably have perished.

The British dirigible R. 34 left this country for England just in time to prevent being swept away by a high wind. The forecaster based his warning partly upon upper air observations. Its officers have stated that had not the warning been received the air ship, valued at more than $1,000,000, would have been lost.

The N. C. 4's recruiting trip during the autumn was successful from beginning to end largely because her pilot followed the weather forecasts, which were based partly on upper air observations.

There is no authenticated instance in which an aviator has had an accident due to bad weather, when favorable weather had been predicted.

4. MARINE METEOROLOGY IN AID OF NAVIGATION; VESSEL REPORTING

Meteorological observations taken on ships plying all the seas of the world are collected, charged, and discussed, and the information furnished to the Hydrographic Office of the Navy and published in the pilot chart issued by that office. The information is also made available for vessel owners, ship builders, and others engaged in work relating to ocean navigation. Moreover, the information is of value and assistance to the forecasters in preparing weather forecasts, storm, hurricane and other warnings. A full understanding of the meteorology of the ocean is of tremendous economic value in determining the safest and best voyage routes, the location of dangerous storm areas and meteorological facts of value to mariners.

Vessel reporting stations are maintained at Cape Henry, Va., Sand Key, Fla., Point Reys, Calif., and North Head, Neah Bay, Tatoosh and Port Angeles, Washington, for the purpose of reporting to ship owners the passage of vessels in and out of straits or capes at a distance from the port of destination. This gives owners several hours advance notice of the docking of ships and enables them to assemble stevedores and make other arrangements regarding loading and unloading, etc.

5. EVAPORATION AND RUN-OFF INVESTIGATIONS

Evaporation investigations are conducted in certain arid and semi-arid regions of the west for the purpose of determining the loss of storage water by evaporation. These results are of direct value to engineers in planning city water supply systems and water and irrigation reservoirs.

A special project is being conducted at Wagon Wheel Gap, Colorado, in coöperation with the Forest Service, which involves the study of the effect of forest covers on stream flow, erosion, and run-off. The scheme involves the making of continuous observations in two nearly similar water sheds in their natural forested condition for a term of years; then to denude one of the water sheds and continue the observations for a similar period of years for comparative purposes.

6. RIVER AND FLOOD WORK

This is one of the most valuable and important activities of the Weather Bureau. Service is maintained on all of the navigable rivers of the United States, their tributaries and water sheds. Gages are located at suitable intervals on the rivers for determining water stages and rain gages distributed throughout the water sheds. Stage readings and precipitation are telegraphed daily to district centers where the observations are tabulated, summarized and published on the daily weather maps or in bulletin form. When conditions warrant, warnings of floods are disseminated to all affected interests in the flood areas by telegraph, telephone and other means.

The saving which results from this service consists in transferring goods, stores and other movable property to places of safety. In the south and southwest live stock is pastured in river bottoms many of which are subject to overflow in time of flood. A notice of the coming of the flood permits the owner of the live stock to drive it out of the bottoms, and to safeguard any other movable property that may have been left in the threatened region. Forecasts of extreme low river stages are also of much value to navigation in many rivers.

7. MOUNTAIN SNOWFALL AND WATER SUPPLY INVESTIGATIONS

This service is maintained in the western states in order to give dependable information relative to the seasonal snow cover, its depth and water content. This information is valuable in determining the probable water supply that will be available both for irrigation and power purposes in those states having little or no rainfall in the summer months.

8. CLIMATOLOGICAL WORK

This work consists of the collecting, compiling, charting, and discussing climatological data of the United States, its adjacent oceans and outlying territories. In addition to the observations taken at the more than 200 regular stations of the Weather Bureau, observations of temperature, weather, precipitation, etc., are taken by cooperative observers located in practically every county of the United States. These observations are compiled and printed monthly and disseminated to the public, to commercial exchanges, farmers, engineers and home seekers and many other classes. The value of this work is incalculable. It affects and benefits the entire people; is used in the settling of damage claims, cases at law and for other purposes too numerous to mention. In New York City alone the weather records are brought into court by personal appearance of a Weather Bureau official more than 500 times a year. Several thousand certificates are issued annually over the seal and signature of the Secretary of Agriculture for court use. More than 100,000 people annually make inquiry and are furnished statements regarding past weather records. The economic value of the climatological work of the bureau is enormous.

9. AGRICULTURAL METEOROLOGY

This service consists in the collection and dissemination of information relative to the effect of current weather conditions on farm activities, the growth of vegetation and the development of crops and their advancement as compared with an average season. Advice is given and information disseminated relative to the utilization of the special forecasts and warnings issued by the Weather Bureau in the interest of various lines of agriculture and horticulture and stock raising. A number of lines of investigations are being conducted as to the relationship between weather conditions at various periods of the growing season and crop yields.

(*a*) *Weekly Weather and Crop Bulletins.*—The National Weather and Crop Bulletin issued weekly at Washington contains a detailed summary of the weather conditions throughout the country for the period covered by each issue and indicates the resulting effects on the development of vegetation and farm activities in general. In addition, the Bulletin contains from time to time charts and discussions bearing on the relation of climate to crops and agricultural operations.

The value of this publication is evidenced by the popular demand for it, which has grown to such proportions as to severely tax the printing facilities of the Bureau. There are about nine hundred paid subscriptions to this publication and about twenty-seven hundred other recipients.

In addition, a weather and crop summary is issued during the active agricultural season at each state center, forty-two in number, which is disseminated by bulletins and through the public press.

These services are of much value to those interested in agricultural operations throughout the country, as they afford detailed information as to the prevailing weather in all sections and its effect on farming operations and crop development.

(*b*) *Corn and Wheat Service.*—This service is maintained during the active growing season of each year, April 1 to September 30, in sixteen principal grain states. Daily weather reports are received from one hundred and eighty-seven special reporting stations. Daily bulletins are issued from nineteen points. A weekly corn and wheat region bulletin is issued also at the regional center, Chicago, Ill.

This service is of especial benefit to farmers, dealers, millers and others interested in the cereal crops of the United States, and gives accurate information as to prevailing weather conditions throughout the principal producing areas during the period of crop development. There are constant and persistent demands for extensions of the service into regions not now covered.

(*c*) *Cotton Region Service.*—This service is similar in organization and operation to the corn and wheat region service, and covers the cotton growing states of the country. About two hundred reporting stations are maintained. Bulletins are issued from 26 stations located in the cotton belt. The service is maintained in the interest of cotton growers and others engaged in various lines connected with the industry. It is considered by cotton growers and dealers to be of indispensable value.

(*d*) *Cattle Region Service.*—This service is maintained for the collection and dissemination of information of current weather conditions over the great grazing districts of the west and southwest as affecting the stock industry. Weekly bulletins are issued from a number of central points throughout the year, giving information as to rainfall, temperature, snowfall, and general grazing conditions on the ranges.

10. SOLAR RADIATION INVESTIGATIONS

The importance of sunshine as a weather and climatological factor is fully recognized by meteorologists, climatologists, biologists and others. Until a comparatively recent time, only the duration of sunshine has been recorded. The object of the solar radiation investigations is to measure the intensity of the sun. These data have not only direct application to agriculture, but have a value in many other ways. Illuminating engineers have for a long time been asking for measurements of daylight or sunshine intensity expressed in light units. This information is of special value to them in connection with the designing of factories and other buildings to meet the requirements of state laws relative to natural light.

11. SEISMOLOGICAL INVESTIGATIONS

The object of these investigations is to determine how frequently on the average and how severely the various portions of the country are shaken by earthquakes. From the data thus collected, it is determined what sections of the country are seismic; just where the active cracks are, and their liability to further slips or movements. This information is of prime importance in enabling engineers to avoid the placing of aqueducts for city water supplies, dams and other structures on or across fault lines.

12. VOLCANOLOGICAL INVESTIGATIONS

This work is now confined to studies of the activity of the Kilauea Volcano located on the Island of Hawaii. It has been conducted by the Weather Bureau for little more than a year, is scientific in character, and it is impracticable at this time to assign thereto any pecuniary return to the people.

APPROPRIATION

The appropriation for the Weather Bureau for the fiscal year ending June 30, 1920, amounted to $1,880,200, almost the whole of which is expended for public service, and under existing conditions but little is available for investigation and research. It is very easy to show that benefits accrue to the United States in the proportion of much more than one thousand to one.

PROBLEMS OF METEOROLOGY

It is now necessary to carry on all the practical routine functions of this service while the forecasters and officials are still doubtful concerning many of the most fundamental and important laws and causes of the atmospheric phenomena with which the science deals.

Education.—Very few of the great universities carry any courses in advanced meteorology. In fact, meteorology receives far less attention in the institutions of learning than is given to almost any other branch of science. Tendencies to the more profound recognition of meteorology in education are in evidence, and it is earnestly hoped that much more intensive attention can be given to this science in view of its enormous practical importance in the everyday life of humanity.

Meteorological Laboratories.—Progress in other lines of science has been made in many cases by leaps and bounds through the establishment of laboratories for the conduct of specific investigations. The really great problems of meteorology cannot be brought within the domain of a laboratory, and no real meteorological laboratory yet exists, although certain elemental phenomena of meteorology admit of investigation in the best physical laboratories. Progress is delayed, no doubt, because of the inability to subject the great major phenomena of meteorology to laboratory and controlled investigation. The difficulties involved in these considerations appear to be inherent and insurmountable, leaving to the student only the alternative of minutely analyzing the complex mass of meteorological data, with the view to working out the more or less indefinite correlations and the coefficients of involved equations representing all the factors in operation in arriving at any particular result. The masses of data available are enormous, and meteorology has been criticized for continuing to accumulate, as it must from other important considerations, observations of every sort of atmospheric condition while its discussion is sadly neglected. Students are needed to study this mass of data, and it is often difficult to know where to begin or how to proceed.

Meteorology of the Oceans.—While the mass of data available for land and continental areas is very great, the details of information available from the larger expanses of ocean areas are correspondingly meager and inadequate. Owing to the uniformity of conditions over the ocean areas, as well as their tremendous extent, we should expect the sequence of atmospheric phenomena to be more simple and easy of investigation. The collection of more complete data from ocean areas and its intimate analysis and discussion are, therefore, most important.

Aerology.—The term *aerology* has come recently to be applied to the meteorology of the free atmosphere in contrast to observational data dealing specifically with surface conditions. The appliances and agencies for conducting observations in the free air have been improved, multiplied and made highly practical, and the attainment of practical navigation of the air has not only added to the observational possibilities but has imposed new and important obligations on the meteorologist to make applications of his science to the aid of aerial navigation. Free air observations over the oceans especially are greatly desired, although very difficult to secure. The whole field of investigations in the free air is relatively only recently occupied and appears to be fruitful of the most important developments. The organization of systematic studies of upper air observations to be carried on concurrently with the making of the observations is at once among the foremost problems awaiting consideration.

WORLD METEOROLOGY

The development of the so-called world meteorology as typified by the collection, summarization, and publication of simultaneous weather reports from all civilized countries has been the goal of meteorologists for many years. The desires of a number of international meteorological conferences in pre-war days are being realized by the publication the "Réseau Mondial" by the Meteorological Office of England. The "Réseau Mondial," however, provides only for monthly summaries of pressure and temperature.

In 1914 the Weather Bureau began publication of a daily chart of pressure and temperature of the Northern Hemisphere from telegraphic and cable reports. The machinery for the collection of the data was put out of commission during the war and has not yet been restored.

In considering the resumption of this undertaking it may be viewed from a totally different angle than formerly, viz., is it not possible by sacrificing the time element to prepare a daily pressure and temperature map of the Northern Hemisphere—say 30 to 90 days after the current dates have elapsed, such maps to serve for study purposes only and to be reproduced in a small edition?

Closely coupled with the above proposition is the need already mentioned of more frequent meteorological observations over the oceans, more especially the Pacific. In this connection reference may be made to the Committee of Exploration of the National Academy of Sciences which has had under consideration for several years plans for the exploration of the atmosphere over the Pacific. What I have more particularly in mind, however, is the necessity for more meteorological observations from vessels navigating the Pacific. The ordinary means at our command that can be applied to stimulating interest in meteorological observations by vessel masters may not be sufficient to procure an immediate response to this need. In any event it will be helpful and necessary to secure the active coöperation of all the great dominating interests concerned in the development of the Pacific area.

UNIFORMITY IN PUBLICATION

The one great handicap to the use of the accumulated meteorological data is the fact that adequate summaries for representative stations in different parts of the globe are not available. An important forward step will have been taken when the various meteorological services shall agree to compile according to some uniform plan and publish monthly and annual summaries for representative stations under their jurisdiction. This in effect would be an extension of the data now published by the English Meteorological Office to include other elements than pressure and temperature. The subject is one for international conference and consideration; meanwhile each country should begin the preparation of a digest of its own data for the needs of its own citizens.

TERRESTRIAL WEATHER AND SOLAR ACTIVITIES

A profound understanding of the problems of meteorology requires a correct discernment of the ultimate causes of weather. In recent years much attention has been directed to the establishment of detailed correlations between solar activities and terrestrial weather. It seems most important to proceed cautiously in a matter of this kind, since it would appear that the views being advocated by a few leaders in these investigations must be regarded with skepticism and distrust in the forms in which they have thus far been presented. All are in accord that the sun is the ultimate source of all our atmospheric phenomena commonly designated weather. It seems that the daily sequence of sunshine and darkness; the varied distribution of clear and cloudy skies; diversities of surface cover added to contacts of land and water areas, including the phenomena of evaporation, condensation, and precipitation; the cycle of the seasons, and above all the fluctuating but nevertheless perpetual contrasts of surface temperatures, ranging all the way from the heat of the tropics to the intense cold of the polar zones constitute a complex series of varied and changeable influences seemingly abundantly adequate to cause and explain every feature of our weather conditions, however changeable we may find them.

These differences and contrasts on the one hand perpetually disturb the orderly arrangement of air densities and pressures demanded by gravity. The latter, on the other hand, as perpetually and continuously sets portions of the air in motion, in order to establish and maintain a state of equilibrium, which, however, is never attained. We must clearly recognize that *the ceaseless complex changes in and motions of our atmosphere* represent in fact the only state of equilibrium possible between gravity on the one hand and solar heating of the earth on the other.

The disposition on the part of some to attribute important phenomena of terrestrial weather to minor solar activities is believed to be a mistake. It is in fact believed that apparent changes in solar activity, especially of the thermal character, are in reality altogether of terrestrial origin and that the imaginary correlations between terrestrial and solar activities are really the simple relations between atmospheric phenomena at one point and possibly those at another on the earth itself. It is most important that these major problems of cause-and-effect relation between solar and terrestrial activities should be observed, not only with the greatest possible accuracy but that the observational data themselves should be thoroughly analyzed and discussed, first, to establish the magnitude of the errors of the observation, and second, to determine the possible relations between solar and atmospheric effects.

Seemingly the greatest need in meteorology is that of a master mind to direct itself comprehendingly and intensively to the great problems which the science still presents.

SOME OF THE CHIEF PROBLEMS IN TERRESTRIAL MAGNETISM AND ELECTRICITY

By LOUIS A. BAUER

1. *Analysis of the Earth's General Magnetic Field:* The early completion of the general magnetic survey of the globe, as undertaken by the Department of Terrestrial Magnetism of the Carnegie Institution of Washington and coöperating countries, will afford the necessary data for the investigation of some of the so-called "greater problems of the earth's magnetism." One of these is the determination of the various systems of magnetic and electric forces which together make up the total terrestrial magnetic field as observed at the earth's surface. It is known from previous analyses that the earth's magnetic field consists of an internal system of forces, which constitutes by far the major portion; secondly, an external system, supposedly to be ascribed to electric currents circulating in the earth's atmosphere; and, thirdly, a system possibly such as that of vertical electric currents which pass from the atmosphere into the earth and vice versa. The definite settlement of the question as to the existence of the third system is regarded by Sir Arthur Schuster as one

of the chief outstanding problems in terrestrial magnetism. This problem involves the question whether or not the magnetic forces of the earth can be entirely referred to a potential. If not, then the question arises as to the existence of vertical electric currents, which will be of interest both to students of the earth's magnetism and of the earth's electricity, using the latter term in its most general sense so as to include electric phenomena arising from systems both below and above the earth's surface. The ocean work of the Department of Terrestrial Magnetism has been arranged with the special view of obtaining the requisite data for the solution of this interesting question. But for the war all the necessary data and the results of their discussion would be available now.

2. *Analysis of Continental and Regional Magnetic Fields:* The detailed magnetic survey of the United States—the largest land area over which magnetic surveys with the requisite accuracy and detail have been made—affords further opportunity for the investigation of the problems referred to in the previous paragraph, at least so far as the investigation of those problems can be undertaken for a limited region of the globe. In the respect that the magnetic constants of the earth's general field depend in a very large measure upon the area embraced in the investigations, problems of terrestrial magnetism differ fundamentally from those of geodesy. Concerning this point, a preliminary study was made by the writer in connection with a brief discussion of "Some Results of the Magnetic Survey of the United States" printed in *Science*, May 22, 1908, (812–816).

It was found that the magnetic forces observed at any given point in the United States are the resultant effects of (1) a general or terrestrial magnetic field arising from the general magnetic condition of the earth, (2) a general, terrestrial disturbing cause which distorts the general magnetic condition of the earth at the place of observation, related possibly in some manner to the rotation of the earth, (3) a continental disturbing effect, arising largely from that portion of the North American continent above the general ocean bed, (4) a regional disturbance caused by low-lying magnetized masses or earth-currents covering a certain region, and (5), a local disturbance to be referred to magnetized masses or currents in the immediate vicinity.

A mathematical formula of limited spherical harmonic terms based upon the entire earth cannot, of course, include also disturbances of such restricted areas as are embraced under Nos. 4 and 5, namely, regional and local disturbances, but it appears that such a formula cannot adequately represent even the *continental* effects. In the spherical-harmonic formulae the terms of the various orders advance and recur by trigonometric functions of multiples of the longitude and the latitude. Thus a term involving six times the longitude is supposed to continue around the earth six times in unaltered manner. However, such a term arising, for example, from the North American continental shelf, which in the

United States has a width in longitude of about 60°, would not continue around the earth but would in all probability die out before the European or the Asiatic continent is reached. In brief, the higher harmonic terms of the mathematical formula introduced by Gauss and based on the potential theory, could at the best but simulate or "counterfeit" the facts of nature and not represent the actual truth.

Thus the question arises how far it is really worth while to proceed in the establishment of such a theoretical formula. Although the mathematical expressions may be extended so as to include forty-eight or more unknowns, it has been found that the computed results, magnetic declinations, for example, depart so much from the actual observed results—several degrees—that they could not be used even for purely practical purposes such as those of the surveyor and the navigator. The conclusion therefore reached was that it may be found sufficient for many purposes to restrict the theoretical formulae to a certain finite number of terms involving a limited number of unknowns which represent, from a physical standpoint, the chief and principal facts of the magnetic condition of the earth, as resulting, for example, from system (1) mentioned above, or perhaps better, systems (1) and (2). The magnetic field represented by this limited expression might be called the normal, or reference field, just as the geodesist, who restricts the constants to be determined for the figure of the earth to two or three, calls his adopted figure, the "spheroid of reference." Residuals from the magnetic field of reference would then receive separate or special treatment in accordance with their extent and their character.

Respecting the existence of possible vertical currents, the preliminary analysis of the data of the magnetic survey of the United States indicated a definite system, related in some manner to the general atmospheric circulation and with an average current strength of about $^1/_{30}$ ampere per square kilometer, just as the writer had found in his previous investigations pertaining to the entire earth.[1] The additional data acquired in the United States since the preliminary analysis, will make possible a more complete investigation of this important matter. Of special interest in this connection will be found the quotations regarding Dr. Wilson's investigations (see paragraph 4).

We see then that the data obtained by the United States Coast and Geodetic Survey in the prosecution of the magnetic survey of the United States afford opportunities for interesting and fruitful research of various kinds. There may be mentioned, for example, the correlation between magnetically disturbed areas and geological formations or local deviations of the plumb-line, in which Section *a* (Geodesy) is also interested. Possibly also the data obtained in the United States may throw some light ultimately upon the problem as to the variation of the magnetic elements with altitude, and upon the question as to the effect of local or regional disturbances upon the secular changes of the earth's magnetism.

3. *Effect of Altitude upon the Earth's Magnetic and Electric Elements:* One of the important problems, both from a practical and a theoretical standpoint, is that of making magnetic and electric observations in the upper regions of the atmosphere. A number of investigations of extreme interest fall under this head. How the magnetic data for the United States may contribute towards the solution of some of the problems has already been stated. The preliminary analysis mentioned gave some indication of dependence of vertical-intensity residuals upon altitude. A joint investigation by the Department of Terrestrial Magnetism and the United States Coast and Geodetic Survey was made of the mountainous region about Pike's Peak, Colorado, directly after the eclipse observations of June 8, 1918. The magnetic stations ranged in altitude from about 6,000 feet to 14,000 feet. Whether any definite result may be derived from such mountain observations as to rate of variation of magnetic elements with altitude, in view of the minuteness of the probable variations, depends upon the successful elimination of the local disturbances caused by magnetic rocks. It was found that the region investigated was not wholly free from such disturbances. It will be necessary for the solution of the problem to devise instruments of sufficient sensitiveness and stability to make possible magnetic observations of the required accuracy in air ships. To show the necessity of altitude magnetic observations, it will suffice to say that an infinite number of distributions of magnetic and electric systems within the earth may be found which will satisfy observations made simply on the surface of the earth.

Complete atmospheric-electric and radioactive observations in the upper regions of the atmosphere are of fundamental importance to the problems of the earth's electric field: of its maintenance, and of its variations. It is very much to be hoped that such observations may soon be systematically carried out in the United States.

4. *Variations in the Earth's Magnetic and Electric Phenomena:* The study of these variations opens up a wide field of research of interest not only to the Section of Terrestrial Magnetism and Electricity but also to other sections of the American Geophysical Union. In investigations as to a correspondence between manifestations of the earth's magnetic, or electric, activity with the Sun's activity, one of the fundamental questions is what to take as the complete measure of activity, either for the earth or for the sun. This question also enters into certain meteorological investigations. In this way, and as appears later, some of the problems of Section *d* (Terrestrial Magnetism and Electricity) also touch upon some of those of Section *c* (Meteorology). The following quotations from the Annual Report of the University of Cambridge for 1919, relating to investigations made by Dr. C. T. R. Wilson at the Solar Physics Observatory, are of particular interest here:

"The results obtained in these investigations have suggested a theory, which both accounts for many of the more important phenomena of thunderstorms, and relates them to those of fine-weather atmospheric electricity and terrestrial magnetism....

"The order of magnitude of the currents in thunderstorms, as indicated by the results of observations of lightning discharges, is such as to suggest the possibility of correlating the phenomena of atmospheric electricity with those of terrestrial magnetism."

5. *Magnetic Storms, Polar Lights and Earth Currents:* Many problems of interest to several sciences come under this head. The various pronounced manifestations of solar activity and terrestrial magnetic and electric activity during the past month, especially March 22–23, have called renewed attention to these problems. The magnetic disturbance of March 22–23 was world-wide and was one of the severest recorded. On the evening of March 22 one of the most remarkable displays of the Aurora Borealis was witnessed at Washington and at points in even lower latitudes; at a corresponding time there was a fine display of the Aurora Australis at the magnetic observatory of the Department of Terrestrial Magnetism at Watheroo, Western Australia. During the magnetic disturbances and auroral displays the generated earth-currents caused interruptions on cable and telegraph lines. The sun-spot activity during this period was of a very marked and interesting character. Dr. Abbot reported that the solar constant, as observed by the Smithsonian Institution party at Calama, Chile, on March 23, had reached the low value of 1.866, whereas from January 1 to March 22 it had varied from 1.93 to 2.00; on March 24, the value was 1.905, and thereafter it went up again. This case illustrates well the joint interest of various sciences.

The general effect of magnetic disturbances is to diminish for a time the intensity of magnetization of the earth's field, in severe cases as much as ten percent. It took about three months after the severest magnetic storm on record, the one of September 26, 1909, before the earth's magnetic intensity had returned to its approximate normal value. An interesting theory[2] of magnetic storms has recently been advanced by Prof. F. A. Lindemann, director of the Department of Physics at the Univeristy of Oxford. To overcome some of the difficulties of Dr. Chapman's theory[3] of a stream of charged particles, Lindemann suggests the alternative of clouds of ionized gas ejected from the Sun and driven away by light-pressure. Such a cloud would be completely ionized, and therefore invisible; it would be kept generally together because it contains both positive and negative ions; and yet it would expand sufficiently to satisfy observed facts about the duration of magnetic storms as the earth passes through the cloud—in our upper atmosphere the ions would recombine and cause the storm. The theory is worked out quantitatively and found to satisfy known facts.

6. *Magnetic and Electric Observations During Solar Eclipses:* Intensive observations under this head have been made, especially since the total

solar eclipse of May 28, 1900, which occurred in the southeastern part of the United States, the writer being then in charge of the magnetic work of the Coast and Geodetic Survey. Nearly every prominent solar eclipse since then has been taken advantage of and coöperative observations over the entire globe have generally been made in accordance with a program outlined by the Department of Terrestrial Magnetism. Briefly stated, the general result has been that an appreciable magnetic effect, though of a minute order, is recorded during the period of a solar eclipse, similar in character to the variation experienced by the earth during a solar day, and differing from it only in magnitude. It is believed that the detection and study of an eclipse magnetic effect will be of great importance to theories of the earth's magnetic field and of its variations.

During the famous solar eclipse of May 29, 1919, the Department of Terrestrial Magnetism had two expeditions in the belt of totality: at Sobral, Brazil, under the charge of D. M. Wise, assisted by A. Thomson; and at Cape Palmas, Liberia, under the writer's charge, assisted by H. F. Johnston. At Cape Palmas there was experienced the longest totality (6 minutes, 33 seconds) enjoyed by any of the eclipse parties. The geophysical observations at the two stations will have an important bearing upon the complete interpretation of the light deflections observed by the British expeditions stationed at Sobral, Brazil, and the Île of Principe.[4]

Besides the magnetic results, interesting electric results were also obtained at Sobral, which, briefly stated, are:[5]

a. The potential gradient showed a well-formed minimum beginning with totality and extending until about 20 minutes after totality; the values observed during this period were about 20% lower than the mean derived from two equal periods immediately preceding and following it. *b.* During the period of potential-gradient minimum the fluctuations of the gradient were very much smaller than during the similar periods preceding and following. *c.* The positive and negative conductivities (and, therefore, the total conductivity also), each showed an increase of the order of 20%, beginning just after totality and continuing for about 15 minutes. *d.* The air-earth current density (product of simultaneous values of potential gradient and total conductivity) showed a greater constancy during the period in question than for any equal period throughout the forenoon of the day of the eclipse.

The results for May 29, 1919, at Sobral, are in general agreement with those obtained at Lakin, during the eclipse of June 8, 1918,[6] notwithstanding the great difference between the two stations as regards latitude, elevation, general topography, and distance from sea.

7. *Terrestrial Magnetism and Seismology:* Records of seismic disturbances are frequently obtained on magnetograms and thus arise problems of joint interest with the Section of Seismology. Prof. Reid has made a special study of these magnetograph records and finds that the oscillations

of the magnets producing the records are chiefly mechanical in their origin.[7]

8. *Terrestrial Magnetism and Electricity, and Physical Oceanography:* The problems of joint interest to the two sections relate to the magnetic and electric observations over the oceans, the delineation of disturbed regions, especially near ports, the effect of ocean depths on changes of the magnetic elements as observed over the surface, etc.

9. *Terrestrial Magnetism and Electricity, and Volcanology:* Besides local changes in the distribution of the earth's magnetic forces in the neighborhood of volcanic eruptions, there are other points of joint interest to students of volcanology and of terrestrial magnetism and electricity. As an illustration may be cited the remarkable circumstance of a world-wide magnetic disturbance which took place almost simultaneously over the entire earth, beginning at 7 h. 54 m. St. Pierre local mean time on May 8, 1902. According to the reports at the time of the Mont Pelée eruption the town clock at St. Pierre was found stopped at 7 h. 50 m.; how accurately this clock kept local mean time is, of course, not known and it may be that the time as obtained from the magnetic records will have to be regarded as the most accurate determination of the beginning of that remarkable eruption. Magnetic disturbances occurred again during the eruptions of May 20 and May 21, 1902.

At the meeting of the "Congress of Arts and Science," held at St. Louis, 1904, L. A. Bauer gave a preliminary discussion of the magnetic disturbances just referred to in connection with his paper on "The Present Problems of Terrestrial Magnetism."[8] The precise causal connection, if any, between the Mont Pelée eruption and the magnetic disturbance has not been completely worked out at present. The connection may have to be sought through the fact that there were remarkable electric phenomena during the eruption which may have caused a sufficient change in the electrification, or in the electric currents, of the earth's atmosphere to have produced in turn a magnetic effect.

During the eruption of Krakatoa, Java, of 1883, there was no world-wide magnetic disturbance. However, a local disturbance, of discontinuous character, occurred at the near-by Batavia Magnetic Observatory, this disturbance lasting merely during the rain of volcanic ashes upon Batavia, and the magnetic effect was attributed by the director of the Batavia Observatory to the magnetic character of the ashes. The electric phenomena in connection with the Krakatoa eruption were also very marked and changes of interest to students in terrestrial electricity may have occurred.

The question of electric currents in the earth's crust in connection with volcanic eruptions is also a matter of interest. Thus Palmieri[9] made earth-current observations in the vicinity of Vesuvius during the period 1889–1893. His observations seemed to show a parallelism with the activity of the volcano. He furthermore found that the action of Vesuvius was such as to mask the diurnal variation in the observations of earth currents.

Reference may also be made to the investigations by Folgheraiter and others in Italy, B. Brunhes and P. David, in France, with regard to the magnetization of lava beds.

10. *Laboratory Problems in Terrestrial Magnetism and Electricity:* One of the prime purposes of the Section of Terrestrial Magnetism and Electricity should be to stimulate research in these fields as widely as possible so as to increase the number of workers, especially at our universities. About the middle of the last century very much of the investigational work in the United States in the subjects here considered was carried out by university professors. It must be one of our endeavors therefore to present and suggest problems which might be advantageously taken up at universities, either in connection with laboratory work or field investigations, over a region of greater or less extent.

Among some of the problems for experimental and theoretical investigation may be mentioned those with regard to the origin of the earth's magnetic field, origin and maintenance of the earth's electric field, improvement of instrumental appliances for magnetic and electrical measurements on the surface of the earth and at altitudes above, the effect of pressure upon the critical temperature of magnetization, studies in magnetism, in general, etc.

In conclusion, let it be stated that the incompleteness of this sketch of some of the chief problems of concern and interest to the Section of Terrestrial Magnetism and Electricity is fully realized. Other topics of importance could profitably have been dilated upon, especially with regard to the question of the origin of the earth's magnetic field and as regards certain outstanding problems in the fruitful subjects of atmospheric electricity, earth currents, polar lights, upper-air electric phenomena bearing upon radio-telegraphy, etc. It must suffice, owing to the limits imposed upon the present paper, merely to have called attention to these additional problems, and to reserve fuller treatment for a subsequent occasion. It is hoped, however, that enough has been said to show the intimate relation of the various problems not only to the sister branches of geophysics represented by the other sections of the American Geophysical Union, but also to the general subjects of physics, astronomy and geology.

[1] See *Terrestrial Magnetism and Atmospheric Electricity,* **9,** 1904 (127, 128).

[2] *Philosophical Magazine* for December, 1919 (669–684).

[3] *Proceedings Royal Society, A,* **95,** 1918 (61–83); also *Monthly Notices of R. A. S.,* Nov., 1918 (70–83); and *Observatory, London,* **42,** No. 539, May, 1919 (196–206).

[4] See L. A. Bauer's "Resumé of Observations Concerning the Solar Eclipse of May 29, 1919, and the Einstein Effect," *Science,* March 26, 1920 (201–311).

[5] See article by S. J. Mauchly and A. Thomson, *Terrestrial Magnetism and Atmospheric Electricity* for June, 1920.

[6] *Terrestrial Magnetism and Atmospheric Electricity,* **24,** June, 1919 (96).

[7] See *Terrestrial Magnetism and Atmospheric Electricity,* **19,** 1914 (57–72, 189–203); also *Bulletin of the Seismological Society of America,* **4,** 1914 (204–214).

[8] See *Publications of St. Louis Congress of Arts and Science,* **4,** 1904 (750–756).

[9] Palmieri, L., "Osservaziones delle correnti telluriche," *Rend. d'Acad. Napoli,* **3,** 1890 (225, 250); **4** (164, 228); **5** (216).

THE PROBLEMS AND FUNCTIONS OF THE SECTION OF PHYSICAL OCEANOGRAPHY OF THE AMERICAN GEOPHYSICAL UNION

By G. W. Littlehales

The former and present function of the ocean in the history of the earth and in its economy has forged bonds of kinship between oceanography and many other branches of science. Ever since the ocean became the world-encompassing highway of communication, its surface aspects, embracing the movements of the waters in waves, tides, and currents, have been subjects of observation. With the advance of the physical sciences and a knowledge of the extent of the ocean came the realization that so large an expanse of a substance having the highest known capacity for heat must, to a large extent, govern the external temperature of the earth and exercise an important influence as a factor in geophysics. But centuries of voyaging did not extend marine observations beyond the delineation of coasts and the service of navigation; and, in the middle of the nineteenth century, the sea remained unfathomed, and the observations of the physicist, the chemist, the geologist, and the biologist did not extend beyond the shallow coastal waters.

In setting forth the principal deep-sea expeditions, by nations and states, through the names of the vessels engaged and the period of their service, we shall serve ourselves the purpose of reflecting the progress of the attempts that have been made to ascertain the physical characteristics of that vast region of the earth's surface which is occupied by the deeper waters of the ocean:

Austria
- Pola (1891–1910).

Belgium
- Belgica (1897–9).

Denmark
- Ingolf (1895–6).

France
- $\left.\begin{array}{l}\text{Travailleur}\\ \text{Talisman}\end{array}\right\}$ (1880–3).
- Caudan (1895).
- Français (1903–5).

Germany
- National (1889).
- Valdivia (1898–9).
- Gauss (1901–3).
- Planet (1906–14).

Great Britain
- Lightning (1868).
- Porcupine (1869–70).
- Challenger (1873–6).

Investigator (1887–1902).
Discovery (1901–4).
Scotia (1902–4).

Holland
William Barents (1878–84).
Siboga (1899–1900).

Italy
Washington (1881–2).
Vettor Pisani (1882–5).

Norway
Voringen (1876–8).
Fram (1893–6).
Gjoa (1903–5).
Michael Gars (1900-20)

Russia
Vitiaz (1886–9).

Sweden
Vega (1878–80).
Antarctic (1901–3).

United States
Albatross (1883–1920).
Blake (1876–97).
Narragansett (1871–3).
Nero (1900).
Thetis (1895).
Tuscarora (1873–6).

Principality of Monaco
l'Hirondelle (1885–8).
Princesse Alice I (1891–7).
Princesse Alice II (1895–1914).

These expeditions and many others of lesser import, operating for the most part in seas remote from the countries in which they were fitted out, have contributed much of the literature of oceanography in which we find set forth the dynamic meteorology and climatology of the ocean, the theories of the tides and waves and the observed facts concerning them, the depths of the ocean, the temperature, the composition and circulation of oceanic waters, the nature and distribution of marine organisms at the surface and in the depths, and the origin and distribution of marine deposits over the floor of the ocean. But the ocean is so vast that the accumulation of facts of observation concerning it—extensive though it be—is but a sparse array in geographical distribution and constitutes but a skeleton of knowledge in relation to the configuration of its basins, the nature and distribution and thickness and stratification of the deposits which cover the bottom, and the physical and chemical properties and movements and mode of operation of its waters in producing their effects in the economy of the earth.

It is not alone through expeditions upon the ocean that oceanography has progressed; investigations in marine laboratories and institutions of research and discoveries in cognate sciences have sometimes yielded more advancement than distant and perilous voyages.

Advancement in the nature of the application of the philosophy of method has enabled oceanography to profit in its later stages of development. The system according to which progress is now being sought is the study in detail of definite stations in the ocean occupied in concert, and, as we hope it will be, by international coöperation, and periodically revisited for the purpose of observing the variations of physical condition whose import, when it comes to be understood, will enhance all those wealth-producing sources which operate in seasonal cycles. Observations of temperature, salinity, gas content, and currents made as nearly as possible at the same instant at series of points or stations and throughout a network of lines distributed in the depths beneath a given area of the ocean, and repeated every three months have afforded the means of making synoptic charts which disclose the existence of bends or undulations like the waves formed on the boundary surface between water layers of different densities. It is the mathematical investigation of the variations with time of the changing network of lines of equal values of the physical elements in their distribution in the depths that promises to introduce oceanography into the ranks of the exact sciences by enabling oceanographers, by mathematical laws, to predict effects from a few observations strategically placed.

Conspicuous among the features of the resumption of American oceanographical operations, after the interruption occasioned by the exigencies of the late times, are the following: The International Ice Observation and Ice Patrol Service in the North Atlantic Ocean, employing the vessels of the United States Coast Guard under an arrangement by which the cost is shared proportionately by the nations participating in the London Conference of 1913, is engaged (coördinately with the primary duties of ascertaining the locations and progressive movements of the limiting lines of the regions in which icebergs and field ice exist in the vicinity of the Grand Bank of Newfoundland and the dissemination of the information so ascertained for the guidance and warning of navigators) in gathering an important accumulation of oceanographical and meteorological observations. Year by year, observations at recorded times, extending from the surface to the bottom, are made in well determined geographical positions throughout the patrolled region for determining the temperature and salinity of the water by readings in series at definite depths, the direction and rate of movement of the waters in the different depths, the collection and preservation of plankton and samples of the water from ascertained depths, and in recording the state of the weather and the sea together with the barometric pressure, the humidity and the temperature of the air. These observations are published annually in the Bulletins of the United States Coast Guard, Treasury Department.

Closely related to these investigations from the standpoint of the advancement of oceanography, is the accumulation of observations resulting from the annual returns of the schooner Grampus in the Gulf of Maine and its vicinity, for the study of the correlation between physical oceanography and biological oceanography in these waters, under the joint auspices of the United States Bureau of Fisheries and the Museum of Comparative Zoölogy of Harvard University.

At La Jolla, near San Diego, California, there has grown up an institution by the name of the Scripps Institution for Biological Research, whose operations, recently brought under the auspices of the University of California, constitute an exemplar of intensive oceanographical investigation. By systematically and repeatedly tabulating and mapping standardized values of the temperature, salinity, density, currents, and gas content of the water of the Pacific Ocean, serially observed at ascertained intervals of depth from the surface to the bottom in fixed locations, the variations of these physical elements, with time and locality, in their distribution in the depths, have been revealed to an important extent within the confines of the oceanic tract in the region of the seat of the Institution, stretching from San Diego to Point Concepcion and embracing an area of more than 10,000 square miles.

It is the present purpose of the Section of Physical Oceanography to foster the labors of these agencies and the similar ones which are contributed by the Navy and the Coast Survey and to seek opportunities to supplement them and link their operations, as far as may be, into coördination with the operations of the oceanographers of Japan, of Australia and New Zealand, of the North Sea International Council of Exploration, and the Mediterranean Sea International Council of Exploration. And, through the formation of committees, to provide that consideration shall be given to the problems of evaporation and heat transference and the interrelations between oceanography and meteorology, to the problems of dynamic oceanography including the variations of mean sea-level and the tides and their manifestations in the depths as well as the surface, to the investigation of the chemical and physical properties of the waters including the penetration of light, to the investigation of the origin and distribution of bottom deposits, to the problem of ascertaining the conformation and topography of the basins, and to the ways and means of advancement in the domain of physical oceanography.

THE PROBLEMS OF VOLCANOLOGY

By HENRY S. WASHINGTON

INTRODUCTION

Of the various sciences represented in the American Geophysical Union that of volcanology is perhaps the most complex and has probably most points of contact with the other geophysical sciences. This complexity and variety in the problems presented by the study of volcanoes arises, in part, from the fact that they are, as has been well said, "natural laboratories." Also the distribution and many of the activities of volcanoes are closely connected with some of the physical, as well as the chemical forces that are involved in the formation and in the present condition of the earth.

In presenting some of the main problems of volcanology, we may begin with those that are essentially and more purely volcanological, and then take up seriatim the consideration of certain problems in which the other sciences represented by sections of the Union may enter. Because of the limitation of space only a brief enumeration and presentation will be made, and no adequate discussion will be undertaken. I am much indebted to various members of the Union for some valuable suggestions, which will be mentioned in their proper places.

VOLCANOLOGY

A word may be said as to the name of the science. The spelling, *volcanology*, (rather than *vulcanology*), has been adopted officially by the Executive Committee of the Union, following the report of a sub-committee. The decision was arrived at chiefly in accordance with the dictum of the Century Dictionary (*s. v. vulcanism*): "The words *volcano* and *volcanic* are firmly fixed in English, and the former is in universal and exclusive use among those who speak that language. Hence all the derivatives should be spelled accordingly."

It is now well recognized that there are different types of volcanoes, distinguished by their form and structure, as well as (generally) by the differing kinds of volcanic activity, and that these differences are to a large degree dependent on, or at least coincident with, the differing chemical types of the volcanic material, such as, for instance, whether rhyolitic, andesitic, or basaltic. It follows that the activity or form of any one volcano, such as Vesuvius, Kilauea, or Stromboli, cannot be taken as typical of all volcanoes generally. This important principle does not seem to be generally recognized.

One of the first and most fundamental needs for the proper study of volcanoes is the compilation of a complete and fairly detailed catalogue of volcanoes (suggested by Wood and the writer). Such a catalogue should list all known active, dormant, or recently extinct volcanoes. It should give briefly, so far as known or ascertainable, the geographical position; the hypsometrical and areal data, including those of the crater; the general petrographical characters of the lavas, with the general order of succession of the different types; whether active, dormant, or extinct, with the geological period of commencement if possible; the general types of volcanic activity; a chronological list of the known eruptions, with some indication of the degrees of intensity; and a bibliography of the chief sources of information as to each volcano; with possibly such other data as may present themselves.

The older lists, as those of Daubeny (1848), Landgrebe (1855), Scrope (1862), and Fuchs (1865), are far from being complete; while even the more recent ones, such as that of Mercalli (1907), though they list many volcanoes not known to the earlier volcanologists, are yet themselves not complete, nor do they give many of the varied data mentioned above as desirable. The compilation of such a catalogue might be entrusted to a special committee.

Another great desideratum is the keeping of a systematic record of all known volcanic events, eruptions, indications of renewed activity, and so on. This should be as complete as possible, and might be compiled by another, or the same, special committee, possibly with the assistance of some news-clipping bureau and correspondents at various volcanoes. In such a record, and in all other official volcanological literature, the sensational features should have a minimum of stress laid upon them.

If possible, some systematic investigation of the little known volcanoes and volcanic regions should be undertaken. This applies especially to those of the islands in the Pacific and Atlantic Oceans, and Daly, in an important paper, has called attention to the paucity of our knowledge as to the first of these. It is possible that such an investigation may be started by the coming Pan-Pacific Congress at Honolulu, at which the scheme will probably be proposed. For this purpose the assistance and coöperation of intelligent persons living in, or trading among, the various islands may be obtained, as well as, possibly, that of some private expeditions. Even the collection of chance specimens from the little-known islands would be of great value.

In this connection it may be suggested that the early history of our own western volcanoes should be investigated, as by search through the records of the early explorers and settlers, for possible mention of volcanic activity, and the verification or disproof of some such reported occurrences. The lavas of these volcanoes are well known, but the character of their volcanic activity has been much neglected.

Apart from the purely scientific question as to how and why a volcano "works," is the practical, and scientifically equally interesting, one of the prediction of eruptions. Unfortunately, with two or three exceptions, we know of volcanic activity almost entirely as displayed only during eruptions, and especially major ones. It is much like studying a complicated machine only when it is working at full speed; we can understand better the relations of the parts and their working if the machine is examined when the parts are moving slowly or are at rest.

For the prediction of eruptions, and for the solution of many other volcanological problems, prolonged, continuous, and systematic observations on volcanoes are absolutely necessary, not only during eruptions, but during the quiet intervals that precede and follow eruptions. Such observations have been carried on at Vesuvius, at first by Mercalli from 1892 to 1906, and by Malladra from about 1912 to the present time. Similar observations have also been carried on for nearly ten years at Kilauea by Jaggar, at times with the assistance of Perret, and Wood. It is hoped that the very detailed observations of Malladra will soon be published; they will form an invaluable contribution to volcanology. The records of the Kilauea observing, kept by Jaggar since 1911, are especially full and detailed and constitute what would be, if published, one of the most valuable contributions to volcanology yet made. It is of the greatest importance to our science that both these stations be maintained, and their observations published, so as to continue the important work so well begun. And it is also important that similar stations be established and maintained at other volcanoes of types different from these two. In its type of activity Kilauea is very unusual, while Vesuvius, though more normal in its form of activity, is very unusual in the character of its ejected material. Stations for continuous observations over a long series of years are needed at volcanoes of the rhyolitic, andesitic, and basaltic (besides Kilauea) types. These should be established at volcanoes which are more or less continuously active, and with a fair prospect of some eruptions from time to time. Several volcanoes in Japan and Java suggest themselves as favorable, and there might be mentioned also Apia in Samoa, Izalco in San Salvador, Stromboli and Santorini in the Mediterranean, and some volcanoes along the Andes and in Mexico. The establishment of such stations offers, of course, many difficulties, but it would be well for the Section to bear this in mind as one of the most important, even if one of the most difficultly realizable, objects of effort.

At any such station the continuous observations to be made would be many and various, including, for instance, daily notes on the state of the crater and fumaroles, the noises emitted, the microseismic vibrations, the weather and barometric data, etc. With all this, there would be opportunity for the collection of specimens from the various flows, fumarolic salts, gases from the lava; measurement of the earlier flows; observation of the temperature of the lavas; and many other matters. The collection of volcanic gases, and study of the best methods for this, are of special importance.

It is not probable that many of these stations will be maintained, at least in the near future, and, as the study of the phenomena of great eruptions is also of great importance, it would be very desirable if some arrangements could be made (possibly international as well as national), through which any usually dormant volcano that was reported to be showing signs of renewed activity or to be in a state of eruption might be visited immediately by competent observers. This has been done for several great eruptions, such as Santorini in 1866, Mont Pelée in 1902, Vesuvius in 1906, and Sakurajima in 1914; but always in a haphazard way, thus diminishing materially the value and scope of the observations in some cases. The immense value of such visits, however, is shown by such monumental works as those of Fouqué on Santorini, the British and Dutch at Krakatoa, and of Lacroix at Pelée, of Perret, Omori, and Kato at Sakurajima, with others of scarcely less importance. It may be mentioned here that we may hope soon to have a similarly complete and monumental report by Perret on the Vesuvius eruption of 1906.

Among other problems with which volcanology has primarily to deal the following may be mentioned:

Study of the rôle played by earth strains is suggested by Wood, who points out that, although such factors as accumulated gas tension and possibly others, may enter, yet "a general cause of volcanic extrusion [may be] due to the so-called mountain building forces as builders of strain, and to certain variable stresses acting as trigger forces." He suggests that study of this phase of volcanology would be of interest also "to the geological seismologist, the physical geologist, and the student of isostasy." The importance of arriving at some estimate, however crude, of the magnitude of volcanic forces, possibly as a measure of crustal deformation forces, is pointed out by Wright.

A systematic and properly conducted study of thermal gradients in the crust is suggested by several (Daly, Van Orstrand, and the writer). Such a study should be carried out at carefully selected points, the stations being chosen with due regard for both the proximity and the absence of volcanoes, differences in the underlying rocks and their geological structure, the absence of secondary sources of heat (such as the proximity of ore bodies), and with proper and well-controlled means of measuring the depths and temperatures. It is reasonable to suppose that such a systematic project might be carried out with no very great difficulty or prohibitive expense. It is needless to say that the results obtained by such properly controlled means would be of the greatest value for our study of the interior of the earth, and would supersede many of the, for the most part, haphazard and not mutually comparable data that are now available.

Connected with this matter is that of the investigation of the thermal gradient and the sub-surface conditions in the mass of an active volcano, both on its flanks and within the crater. Such investigations might throw light on the rôle of gas inter-reactions as causal in the production or maintenance of volcanic heat, as has been suggested by Day, and they might also be of great practical importance in the study of the possible utilization of volcanic heat as a source of energy, a matter to which my attention was directed in Italy, both before the war and during the past year. The study of the possibilities of regions of geysers and hot springs, as the Yellowstone, and in the Hawaiian Islands, along this line, is also suggested. In this connection the present successful utilization of the soffioni at Larderello in Tuscany by Ginori Conti may be recalled.

The problem of radioactivity as a source of heat, and the possible formation of a special committee for this purpose, is suggested by Daly, who also suggests the study of the "meaning of the earth's rigidity."

As minor matters may be mentioned the study of geysers and their mode of action, and also the formation or unification of a terminology for lava structures, since "aa" and "pahoehoe," the two terms most commonly used, by no means cover all the types that are met with.

VOLCANOLOGY AND GEODESY

The apparently intimate correlation between the average chemical composition of the igneous rocks of the continents, oceanic floors, and of smaller petrographical provinces (as a means of arriving at comparable densities of their masses), and their elevations, together with the bearing of this study on the theory of isostasy, has been pointed out recently by the writer. W. Bowie suggests the continuation of such investigations "as a contribution to the study of isostasy." He also suggests the advisability of establishing gravity stations in the immediate vicinity of volcanoes, the selection of appropriate localities to be made with the coöperation of volcanologists. He thinks that there is reason to believe that "areas where volcanoes are active are probably in a high state of isostatic equilibrium," and the determination of this and analogous points would be of great service to both sciences.

VOLCANOLOGY AND SEISMOLOGY

At the outset it must be emphasized that, contrary to a wide-spread belief, there is little connection between volcanoes and most earthquakes, though their frequently contiguous localization may be due to one and the same cause, such as a fracturing of the earth's crust. By far the great majority of recorded earthquakes (it has been estimated at about 95%), and these the most important, are due to crustal movements and, so far as we know, have no direct connection with volcanoes or their activity. The small remainder of earthquakes of volcanic origin are mostly very local and of comparatively small magnitude. Notwithstanding this, the two sciences are in contact at several points.

The study of the small number of truly volcanic earthquakes is, of course, intimately connected with the study of the volcanoes that produce them; while, conversely, the question of the influence of earthquakes as possibly initiating volcanic eruptions or other activity is an interesting field for study.

The study of volcanic tremors of small amplitude and energy, during periods both of quiet and of eruption, is of great importance, and would naturally form part of the systematic, continuous study carried on at a volcano station, as has been done both at Kilauea and Vesuvius. This may be expected to be of very material aid in the future prediction of eruptions. For such observations some of the forms of seismographs and other seismological instruments, duly modified to suit the different conditions, are necessary, and seismological principles or data would also enter into the study.

VOLCANOLOGY AND METEOROLOGY

As has been already noted, the systematic observation of certain meteorological data, especially barometric pressures and rainfall, would form an important item in the routine observations at a volcano station; the former entering into the question of the "trigger forces" needed to start an eruption, and the latter as bearing on the question of the origin of the water present in volcano clouds and gases.

Humphreys called attention some years ago to the influence of the finest volcanic dust, blown into the upper regions of the atmosphere during first magnitude eruptions and suspended there for prolonged periods, in lowering the solar constant of radiation, and thus affecting the weather over wide areas for long periods. This has been invoked to account for periods of glaciation. A knowledge of the chronology of volcanic eruptions is necessary for their proper correlation with meteorological and radiation data, and the further study of this theory.

In this connection mention may be made of the possible utility of the study and identification of volcanic dust collected at great distances from a volcano in eruption, as bearing on the study of the currents of the upper atmosphere.

The possible influence of the supposed blanketing effect of carbon dioxide on past climates has long been a favorite hypothesis, and as it is known that this gas is given off by volcanoes, a possible line of research is indicated connecting climates with periods of great volcanic activity—a matter that has already received some attention.

A minor problem, and one that has been little studied, because of the rarity of its occurrence, is that of the blue or green color presented by the sun during some eruptions, as that of Krakatoa in 1883. Especial attention might be paid to this by the observers of future eruptions.

VOLCANOLOGY AND TERRESTRIAL MAGNETISM

Bauer calls attention to the "local changes in the distribution of the earth's magnetic forces in the neighborhood of volcanic eruptions." He also lays special stress on the investigation of such phenomena as the world-wide magnetic disturbance that took place coincident with the eruption of Pelée in May, 1902, and the fact that no such world-wide disturbances seem to have occurred in connection with that of Krakatoa in 1883. This is a problem in which the observations of local observers might be of great value, were their attention called to its importance and their observations properly directed. The lightning flashes in the volcanic cloud, and other such phenomena, during eruptions, are also of interest and are probably worthy of study.

Bauer also suggests the problem of electric currents in the earth's crust during volcanic eruptions, such as have been observed by Palmieri at Vesuvius. A minor point is the magnetization of lava flows, and that of clay beds covered by them; while there might also be mentioned here the study of fulgurites—the vitreous tubes and patches formed by lightning strokes fusing the rock of the summits of volcanoes, (as Ararat), and other places.

VOLCANOLOGY AND PHYSICAL OCEANOGRAPHY

The most important point of contact between these two sciences is presented by submarine eruptions. Far too little is known of these occurrences, either as to their characters or their distribution over the ocean floors. For their study a most important and but little gleaned field of information is furnished by ships' logs, and I would suggest here the advisability of examining the thousands of ships' logs for records of submarine eruptions. This might be done through a duly appointed committee, with the coöperation of the Section of Oceanography, and with the assistance of the naval and other maritime authorities and ship owners, in whose hands the logs may now be. The data revealed by such an investigation would be invaluable for the study of submarine eruptions, and also for that of other important geophysical problems, such as earth fracture lines and lineaments, the origin of "tidal waves," etc. The submarine cable companies might also furnish valuable data on this subject from their records of broken cables, as in the case of the Aeolian Islands. I recommend this project most strongly for the consideration of the two Sections most interested.

Another problem is that of the material of the deep sea deposits, the study of which was first taken up by the "Challenger" expedition. The "red clays," as is well known, are composed largely of the decomposition products of volcanic material, and their study should be carried out more systematically and extensively than is being done at present. Further study also of the included, and too little known, chondritic and manganese nodules would also be of interest; while the study of blocks of rock (many of them volcanic) dredged from the depths also presents many features of interest. Some provision or arrangement might be made for the preservation and study by some central body of any such material that may be collected in the future. In connection with this the importance is urged of making special effort to obtain rock specimens, or sea floor deposits, from the immediate vicinity of submarine eruptions, or from areas of the sea floor that are known to be specially subject to such disturbances. These would throw much light on the petrographical and other characters of such volcanoes, and the information thus yielded would be very useful in several ways.

VOLCANOLOGY AND GEOPHYSICAL-CHEMISTRY

It is needless to dwell on the close connection between these two sciences —closer indeed than that between any other pair of the geophysical sciences. Some phases have already been alluded to on a previous page of this report, and the matter is also touched on in the portion devoted to the Section of Geophysical-chemistry. It will suffice here merely to indicate briefly some of the main points of interest.

The study of the formation of volcanic rocks and of their component minerals is one of the utmost importance to some phases of the study of volcanoes. Of similar importance is the study of the gases contained in volcanic and other igneous rocks, as well as of the gases given off by volcanoes in activity, with experimental laboratory study of their equilibria and possible interreactions. As is well known, all these, and similar problems form objects of investigations carried out at the Carnegie Geophysical Laboratory.

The formation of volcanic salts and other fumarole products, as well as the action of the fumaroles themselves, their temperatures and other characters, also come under the domain of geophysical-chemistry. Among other subjects may be mentioned: the determination of the actual temperatures of lavas of different compositions, and the thermal and luminous radiations of molten lava; the study of volcanic flames with the spectroscope; the melting-points or intervals of solidified lavas, especially as compared with the temperatures at which they issue; the expansion of the included gases as a factor in producing the various types of lava texture, such as the scorias and pumices; the formation of jointing through cooling, whether on a large or a small scale; the specific heats, compressibilities, thermal expansion coefficients, densities, and other physical or physico-chemical data, of rocks and rock minerals; the viscosity of lavas and its relation to temperature, chemical composition, and gas content. Many others suggest themselves, but the possible list is too long for enumeration here.

VOLCANOLOGY AND MISCELLANEOUS SCIENCES

In a previous section we have already touched on the connection with engineering in the possible utilization of volcanic heat. There may also be mentioned the application of engineering to the prevention of destruction by lava flows or eruption-bred torrents of mud on the flanks of volcanoes.

In connection with botany and related sciences there may be mentioned the subject of the effect of volcanic gases on different kinds of vegetation, a study that is being made at Vesuvius. The problems of the reclamation for agriculture of areas covered with recent lava flows or thick deposits of volcanic ashes or dust is also one of great practical importance in many volcanic regions. It may be mentioned, in this connection, that the recent expedition to Katmai, under Dr. Griggs, was undertaken primarily for the study of the vegetation of the region devastated by the eruption. Though the conditions are almost diametrically opposed, the main object was like that of the study of the Salton Sea flora by the Carnegie Desert Laboratory. Study of methods for the commercial extraction of potash from certain types of lava, as those of the Italian volcanoes, is also a matter of possibly great importance for agriculture in the future.

Finally, to leave the earth, the suggestion is made that some knowledge of the character and chemical composition of the rocks of the moon, and thus possibly a decision of the question of the volcanic or non-volcanic origin of its "craters," might conceivably be arrived at by astronomers, through study of the maximum polarization angle of various portions of the moon's surface, such as the floors of the "seas" and the "walled plains." From the determination of this, calculation of the index of refraction of the material is a simple matter. The difference between the refractive index of a rhyolitic glass (*ca.* 1.490) and that of a basaltic glass (*ca.* 1.590) is so great that it would seem to be possible to obtain some general or at least roughly approximate knowledge of the general composition of the surface rocks of the moon, especially if they are glassy lavas. We should be able, in any case, to determine thus whether all parts of the moon's sur face are composed of the same material.

AN OUTLINE OF GEOPHYSICAL-CHEMICAL PROBLEMS

By ROBERT B. SOSMAN

The subject-matter of geophysical-chemistry may be defined as "the physical properties and chemical reactions of the substances and aggregates that make up the earth". It may therefore be roughly divided into two parts: A. Properties and reactions of materials accessible at the earth's surface. B. Properties and reactions of materials in the earth's interior.

Each of these may again be sub-divided as follows:

1. Properties and reactions of individual chemical substances; for example, the silicate minerals.
2. Properties and reactions of aggregates; for example, oceanic water, silicate rocks.
3. Properties and reactions of larger units of matter; for example, glaciers, batholiths.

A. MATERIALS AT THE EARTH'S SURFACE

CHEMICAL SUBSTANCES

A relatively small number of "common" oxides serves to make up practically 98% by weight of the outer ten miles of the lithosphere. All the other elements and compounds known to chemistry are included in the remaining 2%. From the geochemical standpoint, therefore, we may divide chemical substances into two classes: "abundant" and "rare."

The "abundant" oxides are, in the order given by averages of a great many analyses of terrestrial rocks, as follows:

SiO_2	About 60 per cent by weight
Al_2O_3	About 15 per cent by weight
FeO Fe_2O_3	About 6 per cent by weight

CaO	About 4.9 per cent by weight
MgO	About 3.7 per cent by weight
Na_2O	About 3.3 per cent by weight
K_2O	About 3.0 per cent by weight
H_2O	About 2.0 per cent by weight
CO_2	About 0.7 per cent by weight

An understanding of the chemistry of these oxides and their combinations is essential to the progress of petrology. Their study should proceed from the simple to the complex, i.e., should begin with the individual oxides, then proceed to their two-component systems, then the ternary systems, and so on. Upon this fundamental basis is then erected the structure of physical properties for each system: densities at all accessible temperatures, mechanical properties, fluidity, surface tension, specific and latent heats, etc.

The study of these systems may be divided on practical grounds into (1) investigations of the anhydrous oxides and silicates (taking in the first eight oxides in the list above); (2) investigations involving *hydrous* silicates, as well as combinations containing both carbon dioxide and water.

(1) *Anhydrous Silicates.*—Work on the first group involves high-temperature researches under ordinary atmospheric pressure conditions, except in the case of systems containing the oxides of iron, where the oxygen pressure must be controlled, and systems containing the alkali silicates, where attention to moisture, carbon dioxide, and volatility of the oxides is necessary in certain cases. Considerable progress has been made in the study of the anhydrous silicates. The phase rule diagrams of the four ternary systems of SiO_2, Al_2O_3, MgO, and CaO are now complete, and a large amount of data is at hand on the alkali feldspars, the forms of silica, portions of several quaternary silicate systems, etc.

(2) *Silicates with Volatile Components.*—Work on systems involving the volatile components CO_2 and H_2O must be done, for the most part, under pressure, and with apparatus designed especially for this purpose. The methods are well in hand and progress is being made in assembling experimental data. The theoretical side, involving the complications due to pressure as a variable in addition to temperature, is also being carried forward by several investigators.

So much for the 98%. But the remaining 2% contains many natural substances of such great economic as well as geologic interest that they must also receive attention. These may be roughly classified as in the following examples:

The Sulfide Ores (e.g., sulfides of iron, nickel, zinc, copper, lead, cobalt, cadmium, mercury, silver).—These must be studied both in their dry melts (to obtain their fundamental characteristics) and in relation to water solutions under atmospheric pressure (problems of oxidation and secondary enrichment). A distinct problem of the sulfides is their relation to the silicates in the igneous rocks (differentiation of sulfide-bearing bodies, as at Sudbury, Ontario).

Volcanic Gases and Salts.—These are of particular interest in their relation to volcanic activity, as at Kilauea and Vesuvius. Research on gases, including the various gas mixtures evolved from volcanic vents, is of a peculiarly trying character on account of the invisibility and intangibility of the substances handled, as well as the difficulty of collecting and transporting samples of the natural products. A special phase of this work is the study of the complex gases given off by fumaroles and hot springs. In addition to chemical composition and equilibria of the gases, data are needed on the physics of the flow of such gases from vents, as related to volume, temperature, and pressure at the point of emission.

The volcanic "sublimates", such as sulfur, ammonium chloride, arsenic sulfide, copper chloride, magnetite, may be mentioned in this connection, as well as the minerals accompanying fumarole and hot-spring activity.

The Oxide Ores (e.g., ores of iron, chromium, manganese, tin).—The study of these ores involves high-temperature investigations similar to those on the silicates, and also studies of the hydrated and colloidal oxides.

The Natural Hydrocarbons.—Organic chemistry of a very complex kind is involved in the formation and alteration of natural gas and petroleum, and many problems of physics and physical chemistry, such as adsorption, surface tension, and colloid phenomena, are also involved in their underground storage and movements.

Other substances—for example, the silicate ores, the carbonate ores, the titanium minerals—may be similarly grouped for purposes of experimental study, but it is hardly necessary here to make a complete inventory of such groups.

Running along with all these investigations is the general research necessary to develop experimental methods and apparatus, and to keep the general theory of physics and chemistry abreast of the newly accumulated facts.

AGGREGATES

In the preceding paragraphs we have mentioned some of the researches that are necessary on the chemical *substances* of the earth's surface. We come next to *aggregates*, including the igneous rocks, the pyroclastic and sedimentary rocks, the oceans and other bodies of water, and the atmosphere.

Igneous Rocks and Magmas.—Among the aggregates the igneous rocks are the most important, constituting as they do the original matter from which the others have been derived. They are poly-component systems with seldom fewer than six oxides, and it seems out of the question at present to give a complete phase-rule discussion of the chemistry of any such complex system. It does seem possible, however, to cover the field of actually occurring rocks by partial systems based upon stable minerals, themselves combinations of two or more oxides.

Experimental work has progressed far enough to show that the fusion and solidification phenomena of the igneous rocks are capable of systematic treatment of the kind mentioned above, even when the volatile components H_2O and CO_2 are included.

Following this fundamental information on the fusion diagrams, we must know the densities of the igneous rocks and their magmas at all temperatures, with their changes of volume during solidification; the textures and structures produced by various conditions of solidification; the latent heats concerned in fusion of the magma and in assimilation by it of other rocks; and many other physico-chemical data.

Some of the phenomena of differentiation of silicate rocks are probably to be treated as results of simple crystallization backed up by the effects of gravity in causing sinking or flotation of crystals. This is the only method of differentiation that has been experimentally proven, but the separation of two or more liquid phases, and perhaps other phenomena also, may take part in this little-known process.

The phenomena of the movements of igneous magmas in the lithosphere must next be attacked. This involves studies of viscosity and its changes with temperature; energy transfers accompanying movement, including perhaps the conversion of potential gravitational energy into heat energy by movement; differentiation due to movement; and the effects of the separation of gaseous constituents in causing differentiation, movements of magmas, and transfers of energy.

While the igneous rock is solidifying, processes of metamorphism may go on as a result of the passage of its more volatile constituents through the igneous rock itself and through its bordering rocks. The rise of temperature in the inclosing rocks will also initiate reactions and movements of material. The chemistry of these processes (alteration, serpentinization, contact metamorphism and replacement, formation of pneumatolytic dikes and veins) will have been included under the chemistry of the silicates with volatile components, but the rates of the reactions and the transfers of energy accompanying them must be studied as a separate problem. The question of the permeability of crystalline substances to volatile components may perhaps arise. Studies of the inversion of polymorphic forms, properties of liquid inclusions, and the effects of increased temperature on neighboring rocks must be made in order to obtain data on the original temperature of the magmas.

Sedimentary Rocks.—No sooner does the solidified igneous rock find itself at or near the earth's surface than it becomes the object of the group of processes known as "weathering." This is a subject that has been nearly at a standstill since the publication of Merrill's "Rocks, rock weathering, and soils" about fifteen years ago, which summarized our knowledge up to that date. It involves physical factors such as the disintegrating action of periodically fluctuating temperatures, together with the study of relative rates of reaction within and in the presence of dilute water solutions, with the complications due to colloidal phenomena and reaction in capillary spaces—a difficult field and one worthy of well-planned effort.

Rock disintegration, corrasion, and transportation, included in the general process of erosion, bring in physical and mechanical problems such as the rounding of fragments by attrition; their sorting by air and water movements; their distribution and redistribution through the agency of winds, currents, and waves; the production of particular structures such as ripple marks; the movement of unconsolidated sediments as in sand dunes, soil creep, and solifluxion; and similar questions. Reference need only be made here to summaries of these problems by Vaughan and by Merwin before the Geological Society of America in December, 1919. Experimental physics will be found deeply involved in all such problems. Colloidal chemistry will also enter, as, for instance, in the question of the precipitation and re-solution of fine suspensions; likewise biological chemistry, in the precipitation and consolidation of calcium carbonate, ferric oxide, and other products associated with organisms.

Chemical questions allied to those of weathering will enter into the problems of the consolidation and alteration of sedimentary rocks, including cementation and recrystallization, the formation of low-temperature veins, silicification, and the growth of concretions. The elastic constants of porous aggregates offer an example of the physical data that are likely to be needed in this same connection.

Pyroclastic Rocks.—The question of the origin of these rocks brings up the problem of explosive volcanism. In its larger aspects this belongs properly to the Section of Volcanology, but the products of this type of volcanic activity are very wide-spread, and the particular physical and chemical questions arising from the state of subdivision and modes of distribution and alteration of the products deserve special mention in this rather rough grouping of geophysical-chemical problems. Examples are: the molecular mechanism of the explosion, the peculiar properties of flowing dust-clouds, the physics of the projection of volcanic bombs, the sorting of products by air currents and the weathering of "ash" to form colloidal products.

The Metamorphic Rocks.—The point where a rock ceases to be "igneous" or "sedimentary" and becomes "metamorphic" is not now exactly defined, though subject to exact definition, but a distinct set of physical and chemical questions undoubtedly enters into the problems of dynamic and thermal metamorphism. The effects of *non-uniform* pressure in causing the flowing of crystalline substances and aggregates and their solution and redeposition, as well as the forces exerted in their recrystallization, are of particular interest. The structures resulting from movement in silicate rocks under differential pressure are in need of quantitative study. Transfers of mechanical energy and its conversion into heat are also involved. Consolidation and recrystallization as a direct result of *uniform* pressure are likewise to be considered.

Mention should be made in this connection of the physics and chemistry involved in faulting as well as in jointing and minor movements of the solid rocks. These phenomena have often been treated under the principles of elastic theory as applied to homogeneous bodies, yet there can be no question that the elastic properties and conditions of rupture of aggregates must differ in many essential particulars from those of homogeneous bodies. Here is a considerable field for experimentation.

Bodies of Water and the "Chemical Sediments".—The chemistry of the deposition of salts from sea-water has already been made the subject of special research, and van't Hoff's results in this field are already familiar. The deposition of calcium carbonate awaits a similar thorough study. Allied questions are the formation of dolomite, the deposition of various salts from inclosed bodies of water, the deposition of phosphate rocks, the precipitation of colloidal suspensions of clay and other substances, and the origin of the great deposits of sedimentary iron ore.

Problems of fresh-water bodies and streams include the chemistry of bog iron ores, and the amounts of insoluble and colloidal solids and of soluble salts carried by streams. Problems relating to underground waters may also be included here, such as: the dissolving and recrystallizing activity of underground waters; connate waters and their possible chemical changes; movements of underground waters and their relation to the alteration and concentration of gas and oil; principles of hydraulics governing the flow of wells and springs; and the characters which will serve to differentiate between vadose and juvenile waters.

The Atmosphere.—The physics and chemistry of the atmosphere considered as a chemical unit or aggregate, while logically a part of geophysics and geochemistry, are usually considered to be part of the province of meteorology; and in fact most of the investigations in this field are being made by meteorologists, or physicists employed in meteorological organizations. These questions will therefore not be further considered here. Data bearing on the origin of the earth's atmosphere and its possible changes of composition in the past—for example, the composition of the gases found in rocks and the gases dissolved by the waters of the oceans—are, however, of direct interest to the present Section.

LARGER UNITS

The larger units of matter at the earth's surface are for the most part covered by other Sections. The atmosphere as a unit is treated by the

Section of Meteorology and the atmospheric-electric branch of the Section of Terrestrial Magnetism and Electricity; the oceans by the Section of Physical Oceanography; the large land masses by the Section of Geodesy; volcanoes as units by the Section of Volcanology.

The larger geologic units (for example, petrographic provinces) and the rocks, considered as geologic units, might be considered to fall within the province of geology rather than geophysics, and the same may be said of the earth's glaciers and ice-sheets.

There are certain problems connected with these larger units, however, that may be neglected by the Sections mentioned, not from any lack of appreciation of the importance of the problems, but solely by reason of lack of training and lack of acquaintance on the part of their personnel with the technique involved; just as some of the geophysical-chemical problems mentioned above may be, relatively, neglected by reason of the geophysical-chemist's lack of training in other branches. For example, the physics of the flow of a glacier, considered as a unit, is not likely to be adequately handled by those trained only in the methods of glacial field geology, and it very properly becomes a subject for research under the present Section. The question of the chemical composition of a particular stratum of the atmosphere and the chemical equilibrium obtaining therein, while of great importance to the meteorologist, might fail of adequate treatment by an organization numbering no chemist on its staff. The distribution of certain rock-forming oxides according to "petrographic provinces" may be of primary interest to the geologist, yet the physico-chemical basis for that distribution—the question whether it represents an "original heterogeneity" or a result of differentiation—is a problem for this Section. The flow of rock aggregates of varying composition, again a matter of geological interest through being concerned in the processes of mountain-building, is a problem for the geophysicist. These examples will serve to indicate that there may be many points in which the Section of Geophysical-chemistry may be of direct assistance, through researches of its own type, in the work of other Sections.

B. PROPERTIES OF MATERIALS IN THE INTERIOR OF THE EARTH

The late G. K. Gilbert has made apt reference[1] to the earth's interior in these words: "Once it contained the forges of blacksmith gods; or it was the birthplace of our race, or the home or prison of disembodied spirits * * * * Science now claims exclusive title but holds it chiefly for speculative purposes." Upon the Section of Geophysical-chemistry more than upon any other will fall the duty of maintaining the validity of the title while bringing the property into productive use.

The outstanding difference of condition to which substances in the earth's interior are subjected, as contrasted with substances at the surface, is the tremendous difference in hydrostatic pressure. Research on the properties of substances under high pressure is thus of first importance in relation to the interior of the earth. While we cannot at present go experimentally much beyond 12,000 megabars, a pressure corresponding to about 45 km. (about 29 miles of depth), yet a really adequate knowledge of properties in this range would give us an insight into the conditions in the interior beside which our present knowledge is equivalent to almost total ignorance.

It is most important to know in this connection the compressibility of the substances concerned, at various temperatures, and in both the liquid and the crystalline state, with its dependent constants such as change of melting-point with pressure, and effect of pressure upon solubility. Other important data are: the existence of new polymorphic forms of substances; the effect of pressure upon rigidity and its related elastic moduli; the effect of pressure upon diathermancy, thermal conductivity, specific heat capacity, and magnetic susceptibility; and the effect of pressure in modifying equilibrium in homogeneous as well as heterogeneous systems.

The properties mentioned in the preceding paragraph are all properties of substances in equilibrium. The effect of pressure upon rates of reaction and rates of diffusion and crystallization are also of importance. If the planetesimal hypothesis of the origin of the earth, or some modification of it, is true, it is possible that the interior is very far from a state of chemical equilibrium, and that redistribution of matter and energy may be going forward actively, with what results—whether rising or falling average temperature, increasing or decreasing volume, etc.—it is impossible now to say.

The preceding considerations apply also to the larger units of structure of the interior—although what these units may be we can only guess—and also to the earth itself as a unit. Complex mathematical analysis is necessary in applying the data to the units of structure as well as to the whole of a body as large as the earth, where the force of gravitation is itself variable with depth. These applications can in many cases be best made by those familiar with the properties in question as measured in the laboratory. The course of earthquake waves, to take an example of interest to the Section of Seismology, is dependent both upon the elastic constants of the earth's materials, and the possible reflection and refraction of waves at the boundaries of internal structural features.

In this connection, the members of this Section could do a service to the other Sections by making clearer the real meaning of some of the physical concepts and physical constants involved in geophysical problems. For instance, much confusion has been caused by the fact that there is more than one kind of "rigidity". The geologist to whom the statement that "the earth has the rigidity of steel" is rather vague may take temporary comfort from the fact that the statement also needs much qualification and explanation to the physicist.[2]

General Geophysics.—The physics of the earth considered as a unit (the classical "geophysics") is for the most part either covered by other Sections of the Geophysical Union or is customarily considered as a part of geology. Certain phases of geophysics, however, are not thus assignable and may be mentioned here in order that all of the groups of problems of the science may receive attention in this assembly of surveys.

The form and gravitation relations of our suspended spheroid, its magnetic and electrical properties, its properties as a vibrating body, and the physics of its air and water envelopes, are the obvious fields of appropriate Sections. Hypotheses of its origin and the logical deductions therefrom may confidently be left to the geologists, among whose faults that of narrow-mindedness and lack of a broad outlook in time and space have seldom been numbered.[3] Its properties as an absorbing and reflecting body for external radiation are being well handled by the astrophysicists and meteorologists.

The problem of the earth as a body radiating its own heat into space, however, is an example of a problem that may fail of treatment as a unit problem by any one of the above-mentioned groups acting alone. We are dealing not with a solid homogeneous spheroid of uniform surface temperature radiating freely into space, but with a rather heterogeneous body blanketed with several kinds of heat insulators whose composition varies both with depth and with time. Factors in the problem are: the production of heat by shrinkage; the contributions of heat from radioactive sources; the earth's present thermal conductivity and thermal gradient; the effect of varying carbon dioxide, water, ozone, inorganic dust, and clouds, upon the heat loss; and the effects of the possibly very different atmospheres with which the earth has been blanketed in past ages. The temperature at a given time and at a given distance from the center, as for instance at the solid surface of the land, depends upon a complex set of factors, and may well have been periodic in its variations.

The earth's volume and shape may have been similarly variable. In addition to the variation of temperature, already mentioned, the following are among the factors to be considered: the tides in the solid earth (on which extensive experimental work has recently been in progress) and the earth's properties as an elastic body; the viscosity of the earth as a whole, with relation to long-continued forces, and the existing state and method of maintenance of isostatic equilibrium in its surface layer; its breaking strength and form of rupture under forces changing too rapidly to cause flow; and the lag of elastic and viscous responses to changing forces, as in the case of the addition or removal of continental ice sheets.

Limitations of space forbid more than a sketchy outline of the problems set before the Section of Geophysical-chemistry, but it is hoped that the outline may have been sufficient to indicate the very fundamental character of those problems.

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[1] U. S. Geol. Survey, *Prof. Paper* **85C,** 1913 (35).

[2] See Lambert, *Journ. Wash. Acad. Sci.,* **10,** 1920 (122–143).

[3] In this connection it is important that the study of the composition and probable sources of the matter now being received by the earth, in the form of stony and metallic meteorites, be continued and extended.

THE IONIZATION OF STRONG ELECTROLYTES

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It is well known that in all branches of knowledge it is sometimes difficult to express certain thoughts with accuracy on account of the fact that the meaning of words changes. So in science there are periods when either the same words have a dissimilar meaning, or unlike words have the same meaning, to different workers. This sometimes becomes so marked that old hypotheses may seem to disappear, or new ones to appear, when only the significance of the technical terms has undergone a change. In other cases old terms may be used in new ways to suggest valuable new points of view.

The recent paper by A. A. Noyes and D. A. MacInnes in these PROCEEDINGS[1] which supports the idea of Sutherland,[2] Milner,[3] Ghosh,[4] and Bjerrum,[5] that strong electrolytes are completely *ionized* in solution, may be considered as belonging to the last category. They use the term ion in the sense of a charged particle, and not as a charged particle which is free to move in the electric field, which is the older meaning of the term as applied to solutions, though it must be admitted that their use of the word is in harmony with the meaning attached to it in recent papers in physics. However, it is not the terminology, but the idea it is apt to convey to workers in various fields, which is to be the subject of attention in this short note. The idea developed by Bragg, that a solid salt consists not of a collection of molecules, but of atoms, has led to the hypothesis that such salts are collections of polar atoms, which are often called ions.

Let us suppose that in the gaseous state in a system there are sodium atoms in combination with chlorine atoms. These pairs are molecules, since, on account of their considerable relative distance from other particles, their electromagnetic fields are largely self contained. It is probable that if such molecules remain at temperatures at which the solid salt exists, the distance of the sodium from the chlorine atom, will not be very different from the similar distance in the solid state. On the basis of the nomenclature which considers that the solid salt consists of ions alone, it might be said that the gaseous molecule is also ionized (called intra-molecular ionization by Thomson), but still remains together as a single molecular particle, though it seems preferable to use the terms presented by Lewis and Bray, and consider the molecule as polar, and as made up of polar atoms. If many of these gas molecules pass into an aqueous system, then it is probable that the polar atoms in practically all of them, will separate to greater distances than that between unlike atoms in the gaseous molecules. In any event the sodium chloride particles would not persist as molecules, since their electromagnetic fields would interact with those of the solvent (this action is commonly known as hydration or solvation). If the terminology is such that the salt is considered as 100% ionized in the solid or gaseous state, then it would be absurd to say that it is less than totally ionized in solution. However, many of the pairs of positive and negative polar particles, are so close together that they are not free to move in the electric field, and so in the accepted sense of the term, there are un-ionized or non-ionized aggregates. On the basis of the complete ionization convention these may be called *bound*, and those which are far apart, *free* ions.

From this standpoint let us consider that there exist at any instant in the solution, all degrees of firmness of binding, from ions which are far apart and almost entirely free, down to those which are most closely bound, and therefore are not ions in the accepted sense. Suppose that the problem is assigned of determining the degree of separation or of dissociation of the electrolyte. It is obvious that according to the way in which the distinction is drawn between the dissociated and undissociated fractions, the calculated results will differ.

In order to prevent a confusion in terms in my own class work I have, for the past seven years used the expression, highly ionized salts, acids, and bases, are completely ionized or polarized in aqueous solution, but what may be considered the mean degree of separation or of dissociation, depends upon the method by which it is determined, since it is not to be expected that different measuring devices will draw the line of distinction at exactly the same place.

It is of interest in this connection to note that while in the papers previously cited the hypothesis is presented that the ionization is complete, and while according to the commonly accepted theory the ionization of such electrolytes, when uni-univalent, is about 0.85% at tenth-normal concentration, G. N. Lewis, and Lewis and Linhart,[6] have advanced the idea that the corresponding value is 68.4% (as calculated from the data of Hall and Harkins on sodium and potassium iodates). This they call the thermodynamic or corrected dissociation. It may, therefore be considered that such salts are 100% polarized or ionized, 85% electrolytically dissociated, and 68% thermodynamically dissociated. All of these conceptions are probably of use, and no one of them refers to the same feature of the phenomenon as any of the two others.

The confusion arises partly, as has been stated, from the attempt to consider a kinetic or a statistical problem as giving an average or mean which has no meaning from the standpoint of the condition in the solution, but has a considerable value as the basis for calculations by the use of empirical methods, as for example calculations of solubility or of electromotive force. Those who rally to the support of the complete ionization theory are doing so largely to do away with these mean values, and their endeavor is to introduce a statistical theory which shall be more closely descriptive of the phenomena. However, I wish to point out that to go over completely to this point of view at the present time, before the statistical theory has been worked out in an entirely satisfactory manner, is to lose some of the useful features of the older theory.

Only one of these features will be discussed. In 1911 I showed[7] that the effects of salts upon the solubility of unibivalent salts, are such as to almost inevitably lead to the conclusion that unibivalent salts ionize, or dissociate in steps, to form what were then called intermediate ions. According to Milner's general nomenclature these might be called associated ions. A calculation made by G. N. Lewis showed[8] that the transference and solubility results on this type of salts, could only be brought in harmony, on the assumption that the intermediate ion is so largely separated into its component ions, and that while it moves together under the influence of the electric field, it offers practically the same resistance to motion through the solution as if it were completely separated. Nevertheless, that there is such a union into an "intermediate" or associated, or possibly "complex" ion is clearly indicated by the phenomena. Noyes and MacInnes, though they do not consider this problem, seem to take the point of view that such relations can be accounted for by changes in the activities of the ions. However, the solubility relations of such salts could have been predicted from the standpoint of the intermediate-ion theory without any further knowledge of the relations other than those of the ordinary theory of solutions; but the activity concept, like other thermodynamic relations, is not specially fruitful in this particular sense, though it is a powerful tool in many calculations where the premises are sufficiently known. In the end the complete ionization theory will undoubtedly be forced to consider in this case that the uni-univalent ion is often associated with the bivalent ion, and so will lead to the same conception as the older theory; but it may in the end be able to overcome the imperfections in the latter which are due to the fact that it ignores too greatly the kinetics of the phenomena, though it should be recognized that many of those who use the older theory are fully aware of the statistical features of the problem. In a certain sense it is probably nearer the truth to say that there is only one theory which is supported by the most advanced workers in the field, but that some of them pay attention to one set, and others to another set, of the features of the general theory, which will not be even approximately satisfactory until it is put much more completely into the form of partial differential equations.

Even though many physical chemists may dislike such a use of the term, it may be possible that the work ion will be used most largely in the sense in which it has been used by Noyes and MacInnes. In order to preserve some of the more valuable features of the older use of the term, in case this should occur, I wish to suggest that then a distinction be made, according to the suggestion made earlier in the paper, between ionization, and dissociation, the former term to be used whenever the particle becomes charged, the latter only when there is an obvious separation. Of course this does not surmount the difficulty that there are all degrees of separation, so there will be actually no such thing as a certain percentage dissociation. However, it may be said that the *apparent* percentage dissociation as determined by the conductivity or the thermodynamic method, has a definite value, which may, in certain cases, depend not only upon the experimental method employed, but also upon the additional hypotheses used in the calculation. What has been styled the unionized salt, may be called the undissociated salt. Intermediate or complex ions under the old nomenclature, may keep their older designations, or may be termed ion associates or complexes.

Unless those who speak of the complete ionization of all strong electrolytes do something of this sort, they will be plunged into difficulties if they attempt to use the energy concept (activity) to describe such phenomena as those in which the mercury and cadmium complexes seem, according to the commonly accepted view, to play an important part. They will have to explain also the slightly dissociated electrolytes, and those electrolytes in formic acid, which, although largely dissociated, follow the mass law, as has been found by Schlesinger.

It is always best when possible to devise new terms to express new ideas, since otherwise the point of view of all of the older literature is confused. It may be preferable, as has been suggested to the writer by Professor Stieglitz, to consider a gaseous salt molecule as made up of *polar atoms,* and not of ions, and to use the term ions only in its historical sense, that is to designate particles which migrate in the electrical field.

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[1] Noyes and MacInnes, these PROCEEDINGS, **5,** 1919; *J. Amer. Chem. Soc.,* **42,** 1920 (239).

[2] *London, Phil. Mag.,* Ser. 6, **14,** 1907 (3).

[3] *Ibid.,* **35,** 1918 (214, 354).

[4] *J. Chem. Soc.,* **113,** 1918 (449, 627).

[5] Bjerrum, *Zs. Elektrochem.,* **24,** 1918 (321).

[6] *J. Amer. Chem. Soc.,* **33,** 1911 (1807, 1827, 1836).

[7] *Ibid.,* **41,** 1919 (1951); **34,** 1912 (1631).

[8] *Ibid.,* **33,** 1911 (1864).

ON A CONDITION FOR HELMHOLTZ'S EQUATION SIMILAR TO LAMÉ'S

By ARTHUR GORDON WEBSTER

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Communicated July 14, 1902

During the last thirty years the writer has been very much interested in the diffraction of sound, a subject suggested to him for theoretical treatment in 1888 by his teacher, the great von Helmholtz. Considering the great amount of paper spoiled in futile attempts to further the subject, the pessimistic view of Lord Rayleigh, and the amount of experimental results obtained by the writer, but not published, it seems proper, in accordance with a policy recently announced by the writer, to publish whatever he has in storage, however modest. A small attempt was made in a paper "On the Wave Potential of a Circular Line of Sources (*Proc. Amer. Acad. Arts Sci.,* December, 1911), an improvement on which has been recently made. The following paper is taken from a drawer, endorsed February 20, 1908, and like the other was written in the attempt to advance the theory of the megaphone.

The condition obtained by Lamé that a singly infinite family of surfaces shall be the equipotentials for some distribution is well known. It occurred to me to examine the condition that there may be a function V satisfying the differential equation investigated by von Helmholtz,

$$\Delta V + k^2 V = 0, \tag{1}$$

which comes from the wave equation

$$\frac{\partial^2 \varphi}{\partial t^2} = a^2 \Delta \varphi, \tag{2}$$

when we assume that φ contains a simple harmonic function of the time, the function V depending upon a single parameter q. If this is the case we have

$$\frac{dV}{dq}\Delta q + \frac{d^2V}{dq^2}h^2{}_q + k^2V = 0, \tag{3}$$

where Δ_q stands for the Laplacian and h_q for the gradient of q. Dividing by dV/dq we have

$$\Delta q + h^2{}_q\frac{d(\log dV/dq)}{dq} + \frac{k^2}{d(\log V)/dq} = 0. \tag{4}$$

Since V and its derivatives depend on q alone, we may write this

$$\Delta q + h^2{}_q\text{-}\varphi(q) + \frac{k^2}{f(q)} = 0 \tag{5}$$

where

$$f(q) = \frac{d\ (\log V)}{dq},\ \log V = \int f(q)dq + C,\ V = \mathrm{A}e^{\int f(q)dq}$$

$$\frac{dV}{dq} = \mathrm{A}e^{\int f(q)dq}f(q),\ \log\frac{dV}{dq} = \log \mathrm{A} + \int f(q)dq + \log f(q)$$

$$\varphi(q) = d\frac{\left(\log\dfrac{dV}{dq}\right)}{dq} = f(q) + \frac{f'(q)}{f(q)}.$$

Thus we may write (5).

$$\Delta q + h^2{}_q\left\{f(q) + \frac{f'(q)}{f(q)}\right\} + \frac{k^2}{f(q)} = 0, \tag{6}$$

which is the required condition. This may be put in another form as follows: Multiplying by $f(q)/h^2{}_q$ we have

$$\frac{df}{dq} + f^2 + \frac{f\Delta q + k^2}{h^2{}_q} = 0. \tag{7}$$

Now although it is not necessary for either Δq or $h^2{}_q$ to be functions of q alone, since the first two terms of (7) are such, the third term must be, say

$$\frac{f\Delta q + k^2}{h^2{}_q} = Q(q)f(q). \tag{8}$$

The equation

$$\frac{df}{dq} + f^2 + Qf = 0, \tag{9}$$

is not linear, but may be made so by substituting for f its reciprocal R. We then have

$$\frac{dR}{dq} - QR = 1 \tag{10}$$

of which the solution is

$$R = \frac{1}{f} = e^{\int Qdq}(B + \int e^{-\int Qdq}dq). \tag{11}$$

From (11) and (8) we accordingly obtain

$$\Delta q = Qh^2{}_q - k^2R = Qh^2{}_q - k^2e^{\int Pdq}(B + \int e^{-\int Qdq}dq), \tag{12}$$

which is the desired condition, containing one arbitrary constant and one arbitrary function, and reducing to Lamé's condition when $k = 0$.

It is easy to show that (12) is satisfied by a set of parallel planes, concentric spheres, or concentric circular cylinders, all these being parallel surfaces. It is not satisfied, as is the case with Laplace's equation, by a set of confocal ellipsoids. It was owing to the impossibility of finding other sets of surfaces to test the result, together with compunctions about Huygens' principle, that caused the writer to hold back the result. Recently, however, Somiliana has published a proof that these are the only sets of parallel surfaces that allow propagation according to Huygens' principle (*Atti Torino*, 54, (974–979). Sulle relaziono fra il principio di Huygens e l'ottica geometrica) in which he is obliged to use the formulae of differential geometry. I therefore venture to publish the above more simple result.

THE ABSORPTION OF X-RAYS BY CHEMICAL ELEMENTS OF HIGH ATOMIC NUMBERS

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Communicated August 19, 1920.

Introduction.—A critical absorption wave-length characteristic of a chemical element is a wave-length such that the element absorbs X-rays longer than the critical value less than it does X-rays shorter than that value. Each chemical element has one critical absorption wave-length associated with its K series of characteristic emission lines. Duane and Hu[1] have shown that in the K series of rhodium the critical absorption wave-length is about one-fourth of one per cent shorter than that of the shortest line (the γ line) in the K emission series. The authors[2] found that the critical absorption in the K series of tungsten has a wave-length of about one-half of one per cent shorter than that of the $K\gamma$ emission line. Since the other characteristic X-ray series have longer wave-lengths than those of the K series, the K critical absorption wave-length is the shortest X-ray wave-length now known to be characteristic of a chemical element.

In the research described in this note the authors have measured the critical absorption wave-lengths in the K series of most of the available chemical elements from tungsten to uranium, both inclusive. They used an ionization spectrometer, and examined spectra of the first, second and third orders. In 1918 Duane and Shimizu[3] measured four of these wave-lengths in spectra of the first order by the ionization method. Measurements with the same spectrometer of the K critical absorption had previously been made for most of the chemical elements down to manganese (atomic number 25).[4] De Broglie,[5] and Siegbahn and Jönsson[6] have published measurements of the K critical absorption by means of photographic spectrometers for the chemical elements of high atomic numbers included in this research. They examined spectra of the first order. Columns 3 and 4 in the table contain the values of the wave-lengths they give.

Apparatus and Method.—The general method of making the measurements does not differ essentially from that employed by Duane and Shimizu.[3] We have been able, however, to apply a somewhat higher voltage to the X-ray tube than in the earlier researches. The X-ray tubes we have used do not withstand a *constant* difference of potential greater than about 115,000 volts. To produce X-rays shorter than the K series of uranium, however, requires a voltage considerably in excess of this figure. By encasing the arms of the X-ray tube and also those of the kenotrons belonging to the high tension generating plant in oil baths we have been able to excite the tube with approximately constant voltages up to about 140,000 volts. We estimated these voltages with an electrostatic voltmeter, which we calibrated by measuring the current from the generating plant through a series of coils of manganin wire having a total resistance of 6.043,000 ohms. As a check on the voltage measurements and on the constancy of the difference of potential we determined the short wavelength limit of the general X-ray spectrum and calculated the voltage by means of the quantum equation $Ve = hv$. Duane and Hunt[7] showed by experiments that this law holds for the limit of the spectrum, and their results have been verified by more recent researches.

Results of the Measurements.—On platting the currents in the ionization chamber against the angular positions of the table on the spectrometer that supports the crystal we obtain curves, examples of which appear in figures 1 and 2. The sharp drops, *a*, in these curves, represent the critical absorption due to the chemical element under investigation. A layer of matter containing this element is placed in the path of the X-rays between the X-ray tube and the spectrometer. The angular distance between the mid points in corresponding drops on the two sides of the zero gives us twice the glancing angle, θ, which must be substituted in the usual formula,

$$\lambda = 2d \sin \theta$$

to calculate the critical absorption wave-length. A correction for excentricity, amounting to about 27″ of arc, has to be subtracted from the values of θ measured on the graphs.

One pair of curves in figure 1 represents the critical absorption of uranium. These curves are of special interest, for they correspond to the shortest characteristic X-ray (of any chemical element) that has been discovered up to the present time. The centres of the drops can be

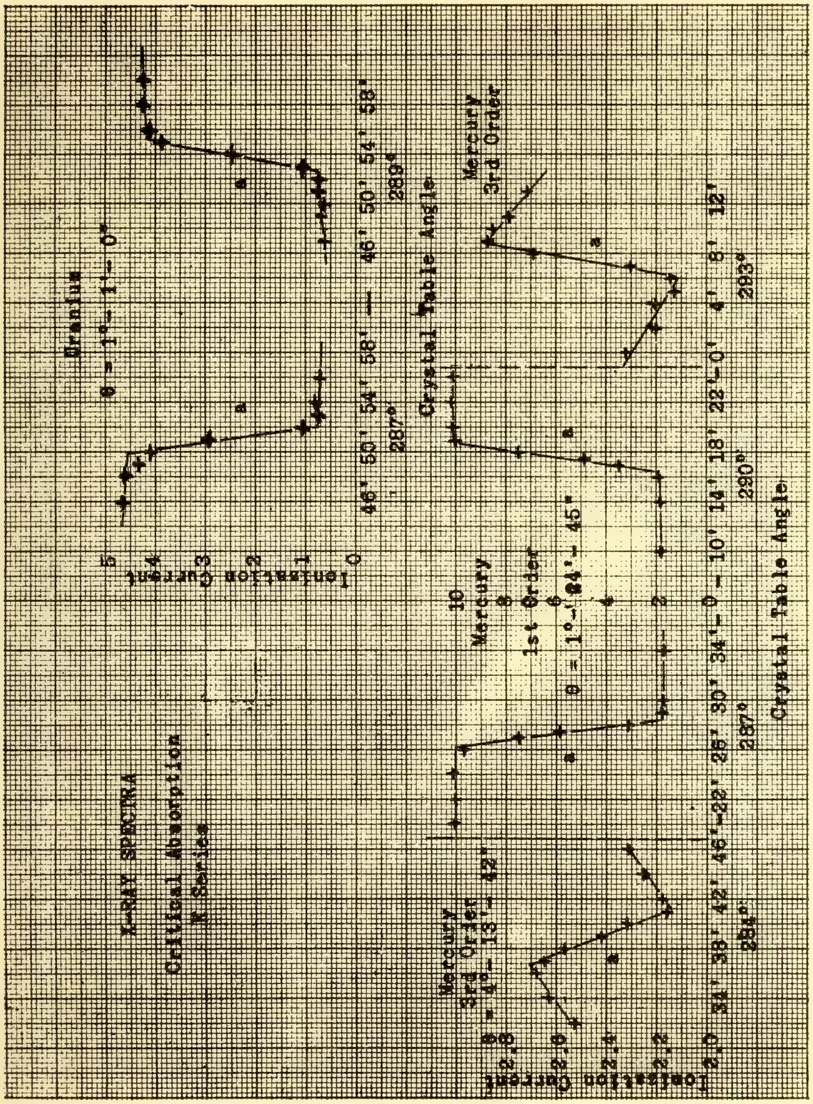

FIG. 1

estimated with reasonable certainty to within about 10″ of arc, and, as θ equals 61′ in this case, the error of precision amounts to less than one-third of one per cent.

The other curves in figure 1 and the curves in figure 2 belong to mercury and bismuth, respectively. One pair of curves represents the critical absorption of bismuth in the second order spectrum, and another pair, the critical absorption of mercury in the third order. The drops corresponding to the absorption in the second and third order spectra are very much smaller than in the first order, and for this reason it is

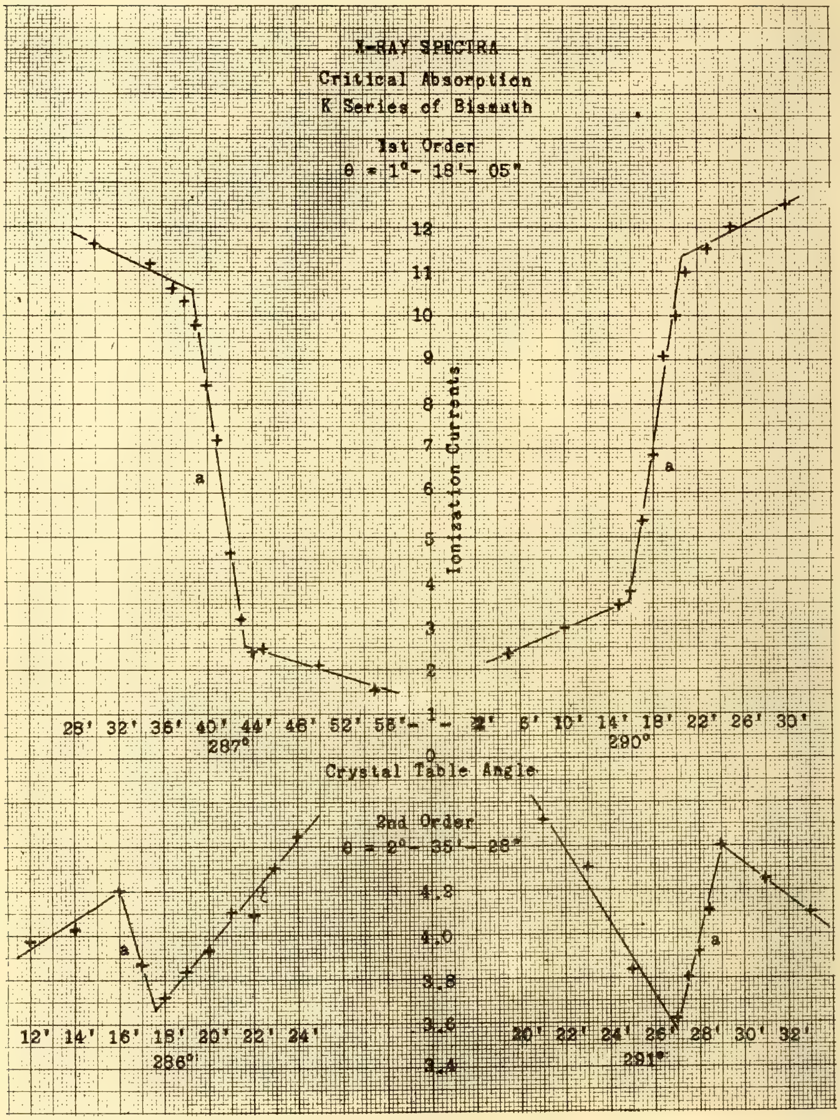

FIG. 2

more difficult to estimate their centers. The value of θ, however, is two or three times as large as in the first order.

Columns 5, 6 and 7 in table 1 contain the critical absorption wave-lengths estimated from the curves of figures 1 and 2 (and similar curves for the other chemical elements) in the spectra of the first, second and third orders respectively. Column 8 contains weighted mean values of the wave-lengths, column 9, the square roots of the wave-numbers, and

TABLE I.

K CRITICAL ABSORPTION WAVE-LENGTHS, $\lambda \times 10^8$ Cm.

For Calcite $\lambda = 6.056 \times s$ in $\theta \times 10^{-8}$Cm.

CHEMICAL ELEMENT	ATOMIC NUMBER	DE BROGLIE	SIEGBAHN JÖNSSON	AUTHORS' VALUES					
				1ST	2ND	3RD	AVERAGE	$\sqrt{1/\lambda}\times 10_{-4}$	DIF.
Tungsten*...	74	......	....	.1783	.1781	.1780	.1781	2.370	.036
Platinum.....	78	.152	.1578	.1582	....	.1581	.1581	2.515	.038
Gold........	79	.149	.1524	.1537	....	.1532	.1534	2.553	.037
Mercury.....	80	.146	.1479	.1493	....	.1488	.1491	2.590	.038
Thallium.....	81	.142	.1427	.1448	....	.1449	.1448	2.628	.035
Lead........	82	.138	.1385	.1412	.1409	....	.1410	2.663	.037
Bismuth.....	83	.133	.1346	.1375	.1369	....	.1372	2.700	.039
Thorium.....	90	......	.1127	.1139	.1124	....	.1131	2.974	.037
Uranium.....	92	......	.1048	.1075	....	....	.1075	3.051	

* The wave-lengths for tungsten were measured in coöperation with Dr. R. A. Patterson.

column 10, the difference, between these square roots calculated per unit increase in atomic number.

Discussion of the Results.—The differences tabulated in column 10 do not vary from their mean value more than one would expect from the experimental errors. This indicates that within the limits of error $\sqrt{1/\lambda}$ is a uniformly increasing function of the atomic number. The differences, however, are slightly larger than the corresponding differences between the square roots of the critical absorption wave-numbers measured in previous researches[3,4] for chemical elements of lower atomic numbers. This means that the square root of the critical absorption wave-number is not quite a linear function of the atomic number, which agrees with the results noted in the earlier papers.[3,4] The variation from the linear law, however, is too small to be clearly indicated in the range of values contained in table 1.

The wave-lengths measured in spectra of the second and third order (columns 6 and 7) appear to be, with one exception, smaller than the corresponding wave-lengths measured in the first order spectrum. The differences between the two sets of values about equal the errors of measurement. Similar differences between spectra of the various orders were observed first by Dr. Stenström[8] in his measurements of long X-rays by means of a photographic spectrometer. He attributed the effect to a slight refraction and dispersion of the X-rays by the reflecting crystal.

Our measurements in the second order spectrum for thorium were not as satisfactory as the others. The generating plant had to be pushed to the limit of its capacity, and the fluctuations in the X-ray output were unusually large.

Our values of the wave-lengths are uniformly larger than those obtained by photographic methods. On the average these differences amount to between one and two per cent. The measurements by Siegbahn and Jönsson were made with an interesting spectrometer specially designed to eliminate errors due to the penetration of the X-rays into the reflecting crystal, etc. In an ordinary spectronometer, if the grazing angle θ, is determined by the position of the reflected beam of X-rays these errors may be either positive or negative, according to the position of the axis of rotation of the crystal with reference to what we have called the *effective* reflecting plane. Further, they may increase or decrease with the wave-length of the rays, and in limiting cases the size of the crystal would have an effect on them. Our method of using the spectrometer eliminates these errors, for we determine the grazing angle, θ, from the angle through which the crystal turns and not by the position of the reflected beam. The differences, therefore, between our values and those given by Siegbahn and Jönsson cannot be ascribed to errors of this kind. Possibly they may be due to differences in the manner of interpreting the experimental observations. If, for instance, measurements on a photographic plate were made from the point where the plate *begins* to get dark, the corresponding value of the critical absorption wave-length would be shorter than that given by our method, for we measure between points half way up the steep drops. The point where the photographic plate begins to darken corresponds to the bottom of a drop on our curves.

The wave-lengths contained in table 1 complete the series of measurements of the K critical absorption of the chemical elements that we have been making in our X-ray laboratory during the past few years.[3,4] We now have values of the K critical absorption wave-lengths for most of the chemical elements from manganese (atomic number 25) to uranium (atomic number 92) both inclusive. These wave-lengths have been measured by means of the same ionization spectrometer and with the same calcite crystal, and are therefore comparable with each other.

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[1] Duane and Hu, *Physic. Rev., Ithaca, N. Y.*, Oct., 1918 (369).

[2] Duane and Stenström, *Washington, Nat. Acad. Proc.*, Aug., 1920.

[3] Duane and Shimizu, *Physic. Rev.*, Dec., 1919 (522).

[4] Blake and Duane, *Ibid.*, Dec., 1917, 697; and Duane and Hu, *Ibid.*, Dec., 1919 (516).

[5] De Broglie, *J. Physique, Paris*, May–June, 1916 (161).

[6] Siegbahn and Jönsson, *Physik. Zeits.*, 1919 (251).

[7] Duane and Hunt, *Physic. Rev.*, Aug., 1915 (166).

[8] Stenström, Doctor's *Dissertation*, Lund., 1919.

THE THOMSON EFFECT AND THERMAL CONDUCTION IN METALS

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Communicated July 14, 1920

The Thomson Effect.—In these PROCEEDINGS for March, 1920, is a paper in which I undertook to account for the Thomson effect values, as found by Bridgman for many metals, in accordance with the hypothesis of dual electric conduction.

Using data obtained by the methods of that paper, I was able, as I stated at the Washington meeting of the National Academy in April, 1920, to account quantitatively for the values of thermal conductivity found in a large number of metals at room temperature. I stated further, however, that the temperature-coefficient of thermal conductivity indicated by my calculations was far too large to accord with the obvious facts, and that a revision of my formulae was therefore necessary. This revision, which has now been made, enables me to deal pretty satisfactorily with thermal conduction, if my assumptions are granted. It involves important changes in most of the numerical tables in the paper referred to, but no great change in the method there followed.

The revision begins with equation (11), which is changed by the substitution of s for 2.5, so that it now stands

$$\lambda' = \lambda'_{\circ} + s\,R\,T, \tag{1}$$

where s is a constant, which varies from one metal to another but is always greater than 2.5. The λ' of this equation is the number of ergs required to separate one electron from its atomic union and leave it free, as a gas particle, within a metal; $\lambda'_{\circ}$ is a constant which may be different in different metals, and R is the gas constant for a single molecule. The assumption involved in this equation, like some others I have made, may seem improbable; but, as it does not, so far as I am aware, conflict with established facts or principles,[1] and as it serves my purpose fairly well, I have felt justified in adopting it provisionally.

As the total energy which a free electron possesses in virtue of its character as a monatomic gas molecule is $2.5\,RT$, the kinetic energy and the pv energy being taken together, and as I assume that this energy is acquired in the act of ionization, I have

$$\lambda' - 2.5\,RT, \text{ or } \lambda'_{\circ} + (s - 2.5)RT,$$

as the number of ergs required for overcoming the attractions or repulsions which are operative in the act of freeing a single electron from an atom, under the conditions which prevail within a solid metal. The supposed increase of this quantity with rise of temperature may be due to the expansion of the metal.

The meaning of P_a and P_f, symbols which I have used before and shall presently use again, is such that

$$P_f - P_a = \frac{1}{e}(\lambda'_0 + (s - 2.5)RT), \tag{2}$$

where e is the electron charge.

The revision leaves equations (12), (13), (17) of my previous paper unchanged, but in (14) it substitutes $(s + 1.5 - q)$ for $(4 - q)$ in the first term of the second member, and in (15) it puts $(2s + 1.5 - q)$ in place of $(6.5 - q)$.

Thermal Conduction.—In December, 1917, I proposed[2] the following theory: That thermal conduction in a metal is due to the convective action of a circulating electric current, free electrons moving down the temperature gradient and associated electrons moving up, with ionization at the hot part of the metal, involving absorption of heat, and reassociation at the cold end, involving the emission of heat. I now propose to go as far as I can at present in the way of submitting this theory to a quantitative test.

As in my previous papers, "hypothesis (A)" will mean the assumption that the purely mechanical tendency of the free-electron gas is toward equality of pressure throughout the metal. Under this hypothesis the condition of equilibrium in a detached metal bar having an established and permanent temperature gradient is expressed by the equation

$$k_f\left(\frac{1}{ne}\frac{dp}{dl} + \frac{dP}{dl} + \frac{dP_f}{dl}\right) = -k_a\left(\frac{dP}{dl} + \frac{dP_a}{dl}\right). \tag{3}$$

This comes from equation (1) of my paper in these Proceedings for April, 1918, by substitution for μ according to equation (2) of that paper, which *should be*

$$\mu = k_f \frac{I}{Gne^2}.$$

The first member of (3) as just given is the strength of the free-electron current, down the temperature gradient, and the second member is the strength of the associated-electron current, up the temperature gradient, per unit cross-section of the bar. We can rewrite (3) as follows, taking, for simplicity, the case in which $(dT \div dl) = 1$:

$$k_f\left(\frac{1}{ne}\frac{dp}{dT} + \frac{dP}{dT} + \frac{dP_f}{dT}\right) = -k_a\left(\frac{dP}{dT} + \frac{dP_a}{dT}\right). \tag{4}$$

The term $(dP_f \div dT)$ expresses a possible differential attraction of the unequally heated metal for the free electrons, tending to move them down the temperature gradient, something distinct from the force due to the gradient of potential $(dP \div dT)$, which the gas-pressure drift of the free electrons produces. The term $(dP_a \div dT)$ has a corresponding meaning with regard to the associated electrons.

The relation between P_f and P_a being expressed by equation (2), we must suppose at least one of the ratios $(dP_f \div dT)$ and $(dP_a \div dT)$ to have a finite value. I shall assume the value of $(dP_a \div dT)$ to be negligible, and therefore we shall have, from equation (2),

$$\frac{dP_f}{dT} = \frac{R}{e}.(s - 2.5). \tag{5}$$

Since

$$p = n\,RT, \tag{6}$$

and, according to a previous assumption,

$$n = zT^q, \tag{7}$$

where z and q are constants, we have

$$\frac{1}{n}\frac{dp}{dT} = R\left(1 + \frac{T}{dT}\frac{dn}{n}\right) = R\,(1 + q). \tag{8}$$

Substituting from (5) and (8) in (4) and taking $(dP_a \div dT)$ as zero, we get

$$k_f\left(\frac{R}{e}(s + q - 1.5) + \frac{dP}{dT}\right) = -k_a\frac{dP}{dT}. \tag{9}$$

According to my conception of thermal conduction, we have only to multiply either side of this equation by λ, the amount of energy required to free $(1 \div e)$ electrons within the metal, to get the value of θ, the thermal conductivity of the metal. That is, we can write

$$\theta = -\lambda k_a\frac{dP}{dT}.\ \text{ergs/cm., sec., deg. } C. \tag{10}$$

We must now deal with the individual factors in the second member of this equation. From (9), remembering that $k = k_a + k_f$, we get

$$\frac{dP}{dT} = -\frac{R}{e}\frac{k_f}{k}(s + q - 1.5), \tag{11}$$

and λ is $(1 \div e)$ times the λ' of equation (1).

Hence we have

$$\theta = \frac{R^2}{e^2}\left(\frac{\lambda'_0}{R} + sT\right)k_a\frac{k_f}{k}\,(s + q - 1.5). \tag{12}$$

Of course, since k is known, k_a is known if k_f is known. Substantially, my method of procedure with each metal is to find by trial values of λ_0', s, $(k_f \div k)$, and q, that will in combination account for Bridgman's value of σ, the Thomson effect, and then from the many combinations that will do this to find the smaller number that will account also for the known value of θ. Ultimately I must undertake to find from the combinations that meet these two tests the ones that will account for or be consistent with the Peltier effect, but I shall not do that in this paper.

In the following tables, as in the corresponding tables of my March paper, the metals are divided into four groups, according to their values of K_1 and K_2 in the equation $\sigma = (K_1 + K_2t)T$. Two alloys also, constantan and manganin, are dealt with according to the data given for them by Bridgman. In finding k_{100}, the electrical conductivity at 100° C., from k_o, the conductivity at 0° C., I have used Bridgman's temperature coefficients; but he, working with very fine wires, did not determine the absolute conductivity at any temperature. The values of θ_o and θ_{100} given in the first line beneath the name of any metal are derived from the work of Jaeger and Diessel-horst, except in those cases where a footnote is referred to. They are expressed in cal./cm., sec., deg. C. C, C_1 and C_2 are constants in the equation

$$(K_f \div K) = C + C_1t + C_2t^2.$$

The δ_c of these tables is the constant part of the "ionizing potential," expressed in volts; δ_o[3] is the total ionizing potential at 0° C., and δ_{100} is the total ionizing potential at 100° C.

The letter y indicates the estimated percentage of atoms which are ionized at 0° C. The method of estimation is given later.

First Group: Metals for which $K_1 > 0$ and $K_2 = 0$.

TABLE 1: COBALT

$k_o = 102 \times 10^{-6}; k_{100} = 74.7 \times 10^{-6}; \theta_o = 0.161;$[1] $\theta_{100} = 0.164;$[1] $(\theta_o \div \theta_{100}) = \lambda.098$

$\sigma = 7.8\ T$ ergs/deg. C.

If $q = 1.55$ and $s = 4$ and $\lambda'_o = 42\ R$, $C_1 = 230 \times 10^{-6}$ and $C = 0.256$

$\left(\frac{kf}{k}\right)_{100}$	γ	δ_c	δ_o	δ_{100}	$\left(\frac{dP}{dT}\right)_o$	$\left(\frac{dP}{dT}\right)_{100}$	$(ka)_o \times 10^6$	$(ka)_{100} \times 10^6$	θ_o	θ_{100}	$\frac{\theta_o}{\theta_{100}}$
0.279	6.7%	0.004	0.098	0.132	−8920	−9720	76.0	54.0	0.157	0.165	0.95

[1] Lacking observed values of θ_o and θ_{100} for cobalt, I have estimated the values here given by use of the Wiedemann-Franz ratio (average for 13 metals) applied to k_o and k_{100}.

TABLE 2: NICKEL

$k_o = 93 \times 10^{-6}; k_{100} = 62.5 \times 10^{-6}; \theta_o = 0.143; \theta_{100} = 0.138; (\theta_o \div \theta) = 1.04$

$\sigma = 3.56\ T$ ergs/deg. C.

If $q = 1.55$ and $s = 4$ and $\lambda'_o = 100\ R$, $C_1 = 105 \times 10^{-6}$ and $C = 0.239$

0.250	5.7%	0.009	0.103	0.137	−8330	−8710	70.8	46.9	0.144	0.133	1.08

TABLE 3: PALLADIUM

$k_o = 100 \times 10^{-6}; k_{100} = 75.9 \times 10^{-6}; \theta_o = 0.165; \theta_{100} = 0.182; (\theta_o \div \theta_{100}) = 0.91$

$\sigma = 3.52\ T$ ergs/deg. C.

If $q = 1.6$ and $s = 6$ and $\lambda'_o = 114\ R$, $C_1 = 69.8 \times 10^{-6}$ and $C = 0.0987$

0.106	3.2%	0.010	0.151	0.203	−5180	−5550	90.1	67.9	0.168	0.182	0.92

Table 4: Platinum

$k_o = 110 \times 10^{-6}$; $k_{100} = 79.3 \times 10^{-6}$; $\theta_o = 0.163$; $\theta_{100} = 0.173$; $(\theta_o \div \theta_{100}) = 0.95$

$\sigma = 2.67\ T$ ergs/deg. C.

If $q = 1.6$ and $s = 7$ and $\lambda'_o = 117\ R$, $C_1 = 45.2 \times 10^{-6}$ and $C = 0.0652$

$\left(\frac{k_f}{k}\right)_{100}$	γ	δ_C	δ_o	δ_{100}	$\left(\frac{dP}{dT}\right)_o$	$\left(\frac{dP}{dT}\right)_{100}$	$(ka)_o \times 10^6$	$(ka)_{100} \times 10^6$	θ_o	θ_{100}	$\frac{\theta_o}{\theta_{100}}$
0.070	2.3%	0.01	0.174	0.235	−3980	−4260	103	74	0.170	0.175	0.97

Table 5: Tin

$k_o = 105 \times 10^{-6}$; $k_{100} = 72.6 \times 10^{-6}$; $\theta_o = 0.157$; $\theta_{100} = 0.145$; $(\theta_o \div \theta_{100}) = 1.08$

$\sigma = 0.134\ T$ ergs/deg. C.

If $q = 1.51$ and $s = 8$ and $\lambda'_o = 234\ R$, $C_1 = 1.97 \times 10^{-6}$ and $C = 0.0466$

0.047	2.3%	0.02	0.208	0.277	−3210	−3220	100	69.2	0.159	0.147	1.08

Table 6: Zinc

$k_o = 180 \times 10^{-6}$; $k_{100} = 127 \times 10^{-6}$; $\theta_o = 0.266$; $\theta_{100} = 0.262$; $(\theta_o \div \theta_{100}) = 1.02$

$\sigma = 0.99\ T$ ergs/deg. C.

If $q = 1.55$ and $s = 10$ and $\lambda'_o = 117\ R$, $C_1 = 11.64 \times 10^{-6}$ and $C = 0.0304$

0.032	1.6%	0.01	0.245	0.331	−2630	−2730	175	123	0.268	0.265	1.01

Second Group: Metals for which $K_1 < 0$ and $K_2 = 0$.

Table 7: Bismuth

$k_o = 9.3 \times 10^{-6}$; $k_{100} = 646 \times 10^{-6}$; $\theta_o = 0.0201$; $\theta_{100} = 0.0161$; $(\theta_o \div \theta_{100}) = 1.25$

$\sigma = -3.2\ T$ ergs/deg. C.

If $q = 1.25$ and $s = 2.8$ and $\lambda'_o = 1170\ R$, $C_1 = -122.6 \times 10^{-6}$ and $C = 0.540$

0.528	2.9%	0.10	0.166	0.190	−11800	−11600	4.28	3.05	0.0200	0.0160	1.25

Cadmium

$k_o = 160 \times 10^{-6}$; $k_{100} = 112.4 \times 10^{-6}$; $\theta_o = 0.223$; $\theta_{100} = 0.216$; $(\theta_o \div \theta_{100}) = 1.03$

$\sigma = -32.4\ T$ ergs/deg. C.

It is easy to find values of q, s and λ'_o that will give the right value for θ_o; but the value of θ_{100} that results is much too small. I cannot deal satisfactorily with cadmium at present, owing to the very large negative value that Bridgman finds for its σ, a value which is about ten times as great as those deducible from the work of various other investigators who have studied the thermo-electric behavior of this metal. See, for example, a paper by Dewar and Fleming, *Phil. Mag.*, Vol. 40, 5th Series, 1895. It is possible that Bridgman has made some mistake in this case.

TABLE 8: COPPER

$k_\circ = 650 \times 10^{-6}$; $k_{100} = 455 \times 10^{-6}$; $\theta_\circ = 0.920$; $\theta_{100} = 0.908$; $(\theta_\circ \div \theta_{100}) = 1.01$

$\sigma = -0.966\ T$ ergs/deg. C.

If $q = 1.49$ and $s = 9$ and $\lambda'_\circ = 3.24\ R$, $C_1 = -12.54 \times 10^{-6}$ and $C = 0.0372$

$\left(\frac{kf}{k}\right)_{100}$	γ	δ_C	$\delta_\circ$	δ_{100}	$\left(\frac{dP}{dT}\right)_\circ$	$\left(\frac{dP}{dT}\right)_{100}$	$(ka)_\circ \times 10^6$	$(ka)_{100} \times 10^6$	$\theta_\circ$	θ_{100}	$\frac{\theta_\circ}{\theta_{100}}$
0.036	6.4%	0.003	0.214	0.292	−2880	−2780	626	439	0.918	0.850	1.08

TABLE 9: MAGNESIUM

$k_\circ = 240 \times 10^{-6}$; $k_{100} = ?$; mean θ between 0° and 100° $= 0.376$

$\sigma = -0.008\ T$ ergs./deg. C.

If $q = 1.499$ and $s = 6.5$ and $\lambda'_\circ = 460\ R$, $C_1 = -14.4 \times 10^{-8}$ and $C = 0.066$

0.066	12%	0.039	0.192	0.248	−3700	−3700	224	?	0.379	?	?

TABLE 10: SILVER

$k_\circ = 670 \times 10^{-6}$; $k_{100} = 476 \times 10^{-6}$; $\theta_\circ = 1.01$; $\theta_{100} = 0.99$; $(\theta_\circ \div \theta_{100}) = 1.02$

$\sigma = -0.864\ T$ ergs/deg. C.

If $q = 1.49$ and $s = 11.5$ and $\lambda'_\circ = 30\ R$, $C_1 = -8.8 \times 10^{-6}$ and $C = 0.0240$

0.023	5.5%	0.003	0.272	0.370	−2380	−2280	655	466	1.01	0.94	1.08

TABLE 11: TUNGSTEN

$k_\circ = 140 \times 10^{-6}$; $k_{100} = 106 \times 10^{-6}$; $\theta_{18} = 0.35$;[1] $\theta_{100} = ?$

$\sigma = -3.41\ T$ ergs/deg. C.

If $q = 1.49$ and $s = 9$ and $\lambda'_\circ = 18.4\ R$, $C_1 = -44.4 \times 10^{-6}$ and $C = 0.0696$

0.065	3.2%	0.002	0.213	0.290	−5380	−5030	130	99	0.355	0.344	1.03

[1] Value found by Coolidge.

Third Group: Metals for which $K_1 > 0$ and $K_2 > 0$.

TABLE 12: IRON

$k_\circ = 78 \times 10^{-6}$; $k_{100} = 48 \times 10^{-6}$; $\theta_\circ = 0.163$; $\theta_{100} = 0.151$; $(\theta_\circ \div \theta_{100}) = 1.08$

$\sigma = (1.78\ T + 6.04\ tT)$ ergs/deg. C.

If $q = 1.205$ and $s = 11$ and $\lambda'_\circ = 117\ R$, $C_2 = 26.9 \times 10^{-8}$, $C_1 = 14.8 \times 10^{-6}$, $C = 0.0364$

0.041	1.2%	0.01	0.268	0.363	−3350	−3740	75.2	46.2	0.161	0.149	1.08

TABLE 13: THALLIUM

$k_\circ = 57 \times 10^{-6}$; $k_{100} = 43.9 \times 10^{-6}$; $\theta_\circ = 0.090(?)$;[1] $\theta_{100} = 0.096(?)$;[1] $(\theta_\circ \div \theta_{100}) = 0.94(?)$

$\sigma = (0.268\ T \times 0.00336\ tT)$ ergs/deg. C.

If $q = 1.48$ and $s = 7$ and $\lambda'_\circ = 117\ R$, $C_2 = 2.80 \times 10^{-4}$, $C_1 = 3.56 \times 12^{-6}$, $C = 0.0676$

0.068	1.9%	0.01	0.174	0.236	−4060	−4100	53.1	41.0	0.089	0.094	0.95

[1] See footnote under table 1, cobalt

TABLE 14: ALUMINIUM

Fourth Group: Metals for which $K_1 < 0$ and $K_2 > 0$

$k_o = 380 \times 10^{-6}$; $k_{100} = 265 \times 10^{-6}$; $\theta^\circ = 0.477$; $\theta_{100} = 0.492$; $(\theta^\circ \div \theta_{100}) = 0.97$[1]
$\sigma = (-0.016\ T + 0.006\ tT)$ ergs/deg. C.
If $q = 1.45$ and $s = 5.5$ and $\lambda'_o = 125\ R$, $C_2 = 6.35 \times 10^{-8}$, $C_1 = -3.04 \times 10^{-4}$,
$C = 0.0886$

$\left(\frac{kf}{k}\right)_{100}$	γ	δ_c	δ_o	δ_{100}	$\left(\frac{dP}{dT}\right)_o$	$\left(\frac{dP}{dT}\right)_{100}$	$(ka)_o \times 10^6$	$(ka)_{100} \times 10^6$	θ_o	θ_{100}	$\frac{\theta_o}{\theta_{100}}$
0.089	11%	0.011	0.140	0.187	−4150	−4170	346	241	0.479	0.449	1.07

[1] Lees found $\theta_{18} = 0.524$ in this metal; but apparently he did not determine θ_{100}.

TABLE 15: GOLD

$k_o = 490 \times 10^{-6}$; $k_{100} = 351 \times 10^{-6}$; $\theta_o = 0.699$; $\theta_{100} = 0.703$; $(\theta_o \div \theta_{100}) = 0.994$
$\sigma = (-0.934\ T + 0.001\ tT)$
If $q = 1.495$ and $s = 8$ and $\lambda'_o = 15\ R$, $C_2 = 0.73 \times 10^{-8}$, $C_1 = -13.7 \times 10^{-6}$, $C = 0.0486$

0.047	8%	0.002	0.190	0.259	−3340	−3290	466	335	0.705	0.672	1.05

TABLE 16: MOLYBDENUM

$k_o = 274 \times 10^{-6}$; $k_{100} = 191 \times 10^{-6}$, $\theta_o = 0.43(?)$;[1] $\theta_{100} = 0.42(?)$;[1] $(\theta_o \div \theta_{100}) = 1.02(?)$
$\sigma = (-4.33\ T + 0.015\ tT)$ ergs/deg. C.
If $q = 1.3$ and $s = 10$ and $\lambda'_o = 110\ R$, $C_2 = 8.7 \times 10^{-8}$, $C_1 = -51 \times 10^{-6}$, $C = 0.0338$

0.0296	2.8%	0.009	0.244	0.330	−2850	−2490	265	185	0.439	0.363	1.21

[1] See footnote under table 1, cobalt.

ALLOYS

TABLE 17: CONSTANTAN

$k_o = 20.4 \times 10^{-6}$; $k_{100} = 20.4 \times 10^{-6}$, $\theta_o = 0.052$; $\theta_{100} = 0.064$; $(\theta_o \div \theta_{100}) = 0.81$
$\sigma = 7.94\ T$ ergs/deg. C.
If $q = 1.55$ and $s = 5$ and $\lambda'_o = 90\ R$, $C_1 = 188 \times 10^{-6}$ and $C = 0.288$

0.306[1]	1%	0.008	0.126	0.169	−12500	−13300	14.5	14.2	0.054	0.076	0.71

[1] Very roughly estimated from the values of γ given for copper and nickel.

TABLE 18: MANGANIN

$k_o = 23.8 \times 10^{-6}$; $k_{100} = 23.8 \times 10^{-6}$; $\theta_o = 0.051$; $\theta_{100} = 0.063$; $(\theta_o \div \theta_{100}) = 0.81$
$\sigma = (-0.828\ T - 0.00672\ tT)$ ergs/deg. C.
If $q = 1.51$ and $s = 4.5$ and $\lambda'_o = 90\ R$, $C_2 = -8.74 \times 10^{-8}$, $C_1 = -18.0 \times 10^{-6}$
$C = 0.27$

0.267	1%[1]	0.008	0.113	0.152	−10500	−10400	17.4	17.5	0.049	0.066	0.75

[1] See footnote under table 17.

Comments.—There is a curious tenacity in the value of $(\theta_o \div \theta_{100})$ for any metal, as found by the method of this paper. Thus in no case except that of Co, where a great range of $(k_f \div k)$ was tried, did the value of $(\theta_o \div \theta_{100})$ change from >1 to <1 with any variations of q, s and λ'_o. Out of seventeen cases, including those of constantin and manganin, the value of $(\theta_o \div \theta_{100})$, as found by experiment or calculated by the use of the Wiedemann-Franz ratio, is in nine greater than 1 and in eight less than 1. In only two of these cases, those of aluminium and gold, does the method of this paper fail to give a value of $(\theta_o \div \theta_{100})$ on the right side of 1.

In nine of these seventeen cases, Bi, Co, Fe, Ni, Pd, Pt, Sn, Tl, Zn, it has been found practicable to get a combination of q, s and λ'_o that will account for Bridgman's σ and give the desired values of both θ_o and θ_{100} to any reasonable degree of accuracy. In all of these nine cases, except that of Bi, the value of K_1 in the equation $\sigma = K_1 T + K_2 tT$ is positive.

In six of the other eight cases the value of $(\theta_o \div \theta_{100})$ here found is larger than the one found by experiment or indicated by the *W-F* rule. In four cases, Ag, Al, Au, Cu, the discrepancy is about 6–10%; in one, that of Mo, it may be considerably greater, but this is a *W-F* case; in only one, that of Cd, is the disagreement hopelessly great. In constantin and in manganin the calculated $(\theta_o \div \theta_{100})$ is less than the observed.

The value of y, the percentage of ionized atoms within a metal, has been estimated by means of the formula[1]

$$k_f = \frac{n\,c\,l\,e^2}{4\,R\,T}.$$

When T is taken as 273, we have, approximately,

$$k_f = 1.91 \times 10^{-20}\; n\,l = 1.91 \times 10^{-20}\gamma\nu l,$$

or

$$\gamma = 5.24 \times 10^{-19}\,(k_f \div \nu l),$$

where ν is the number of atoms of the metal per cu. cm. and l is the length of the "mean free path" of the electrons among the atoms.

Concerning the value of l, I have been and am still much in doubt. Adams and Chapman, in a study of the Corbino effect,[4] estimated it to be in copper not far from 3×10^{-7} cm., which is about 13 times the distance from centre to centre of adjacent atoms, if a cubical arrangement of the atoms is assumed. Compton, K. T., and Ross[5] concluded that a photo-electrically excited electron may move about 2.67×10^{-7} cm. in platinum, about 11 times the centre to centre distance of the atoms, and approximately 5×10^{-7} cm. in gold, nearly 20 times the atomic distance. I have taken l as 10 times the atomic distance, thus putting $l = (10 \div \nu^{\frac{1}{3}})$ and making

$$y = 0.52 \times 10^{19} \times k_f \div \nu^{\frac{2}{3}}.$$

A much smaller estimate of l than the one I have used would be disastrous for my speculations, as it would give large values of γ and thus introduce an insuperable difficulty regarding specific heat. Even with the assumption that I have made as to the length of l, the influence of the free electrons, and still more the influence of ionization with increase of temperature, on the specific heat, presents a rather serious question. Thus, to take what is probably the most unfavorable case set forth in the preceding tables, if in Mg $q = 1.5$ and $s = 6.5$ and $\lambda'_0 = 460\ R$ and $\gamma = 12\%$, we have, as the heat absorbed by the free electrons in the rise of 1 gm. of Mg from 0 °C. to 1° C., 0.025 cal., while the heat required by the accompanying ionization is 0.124 cal., a total of 0.149 cal., which is rather more than half the total specific heat of Mg at 0° C. It would be easy, however, to choose values of q, s and λ'_0 for Mg which would serve the purpose of this paper while affecting the specific heat less. Moreover, it is possible that l may be greater than I have taken it to be.

Table 17 of my paper in these *Proceedings* for March, 1920, now requires revision, Al, Fe, Mo and Tl going into Section (A); but the general testimony of the table remains unchanged, the *mean* of the last column being now -1.50% for Section (A) and $+2.47\%$ for Section (B).

It remains to be seen whether the theory I am developing can deal successfully with the Peltier effect. A preliminary examination, already made, of this matter is encouraging.

[1] Equipartition of energy, as holding for electrons *within* an atom, cannot be regarded as an established principle.

[2] *Physic. Rev., Ithaca,* April, 1918 (329).

[3] In my previous paper δ_0 had a different meaning.

[4] *London, Phil. Mag.,* **28,** 1914 (692–702).

[5] *Physic Rev., Ithaca,* May 1919 (374–391).

MOTION ON A SURFACE FOR ANY POSITIONAL FIELD OF FORCE

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1. The first part of the paper presents a study of the geometric properties of the system of trajectories generated by the motion of a particle on any constraining surface (spread of two dimensions) under any positional field of force. The complete characteristic properties are derived.[1] Starting at any point on the surface in a given direction, and with a given speed, a unique trajectory is generated. The complete system of trajectories forms a triply infinite system of curves corresponding uniquely to a given field of force. The five geometric properties stated in section 2 are characteristic of the system of trajectories, and any triply infinite system of curves on a surface possessing these five properties may be considered as generated by the motion of a particle in a unique field of force.

An additional property serves to characterize the motion when the field of force is conservative.

Another part of the paper presents briefly an analogous study for certain other classes of triply infinite systems of curves on a surface, in particular, brachistochrones and catenaries in a conservative field of force. For all such systems five characteristic properties (sec. 3) differing but slightly from those for trajectories are derived.

2. The motion of a particle on a surface

$$x = x(u,v), \; y = y(u,v), \; z = z(u,v)$$

may be most simply expressed by the Lagrangian equations

$$\frac{d}{dt}\left(\frac{\partial T}{\partial \dot{u}}\right) - \frac{\partial T}{\partial u} = \phi, \; \frac{d}{dt}\left(\frac{\partial T}{\partial \dot{v}}\right) - \frac{\partial T}{\partial v} = \psi,$$

where T is the kinetic energy, and ϕ, ψ are the components of the force given as functions of the coördinates[2] u,v. The differential equation of such a system found by eliminating the time has the form

$$v''' = A + Bv'' + Cv''^2,$$

where A,B,C are special functions of u,v,v'.

The following five characteristic properties of dynamical trajectories under any positional field of force are derived by studying the differential equation of such a system.

I. The ∞^1 curves passing through a given point O in a given direction ξ have associated with them their orthogonal projections in the tangent plane to the surface at O. The locus of the foci of the osculating parabolas of the associate system is a bicircular quartic with O as node, and the straight line in the direction ξ both as tangent line to the quartic and also as one of the asymptotes to the hyperbola which is the inverse of the quartic with respect to the given point O.

II. The focal locus or bicircular quartic associated by Property I with each direction ξ through a point O, is such that the tangent line to the quartic in the direction ξ bisects the angle between the force vector through O and the second tangent to the quartic.

III. Through every point O on the surface and in every direction ξ through that point, there passes *one* curve of the system which hyperosculates its corresponding geodesic circle of curvature. The locus of the centers of geodesic curvature of the ∞^1 hyperosculating trajectories which pass through O is a conic passing through O in the direction of the force vector.

IV. The points of the surface and the directions of the force vectors through these form a set of differential elements defining a simple system of ∞^1 curves on the surface, viz., the lines of force. At any point O, the geodesic curvature of the line of force through O is equal to three times the geodesic curvature of the hyperosculating trajectory of Property III which passes through O in the same direction.

V. Construct any isothermal net on the surface. At any point O, this net determines two orthogonal directions in which there pass two isothermal curves of the net and two hyperosculating curves of Property III. If ρ_1, ρ_2, R_1, R_2 are the radii of geodesic curvature of these four curves, s_1, s_2, the arc lengths along the isothermal curves, and ω, the tangent of the angle between the force vector and the isothermal curve with arc s_2, then, as we move along the surface from O, these quantities vary so as to satisfy the relation

$$\frac{\partial}{\partial s_2}\left(\frac{1}{\kappa_1}\right) - \frac{\partial}{\partial s_1}\left(\frac{1}{\kappa_2}\right) - \frac{1}{\rho_1\kappa_1} + \frac{1}{\rho_2\kappa_2} - \frac{\partial^2(\log \omega)}{\partial s_1 \partial s_2} = 0,$$

where

$$\frac{1}{\kappa_1} = \omega\left(\frac{1}{\rho_1} - \frac{3}{R_1}\right), \frac{1}{\kappa_2} = \frac{1}{\omega}\left(\frac{1}{\rho_2} - \frac{3}{R_2}\right).$$

If, in these five properties, we replace the force vector through a point by the tangent line to the conic of property III, we may state the theorem:

In order that a triply infinite system of curves (∞^1 *in each direction through each point) on a surface may be identified with a system of dynamical trajectories under any positional field of force, the given system must possess properties I, II, III, IV, V.*

When the surface is a developable surface, the bicircular quartic of property I reduces to a circle passing through the given point.

If the field of force is conservative, the conic of property III reduces to a rectangular hyperbola.

3. If we consider the motion of a particle on a surface in a conservative field of force from one position P_0 to another P_1 with the sum of the kinetic and potential energies equal to a given constant, h, certain systems of ∞^2 curves are defined by

$$\int_{(P_0)}^{(P_1)} \sqrt[n]{W + h}\, ds = \text{minimum},$$

where W is the work function (negative potential) and ds is the element of arc length. Among such systems, called "n" systems, we may mention *dynamical trajectories* ($n = 2$), *brachistochrones* ($n = -2$), and *catenaries* ($n = 1$). We may find the differential equation of the ∞^2 curves of an "n" system by the methods of the calculus of variation.[3] If, by differentiation, we eliminate the constant of energy, h, from this equation, we shall get the differential equation of the ∞^3 curves of a complete "n" system. This equation has the form

$$v''' = A + Bv'' + Cv''^2,$$

where A, B, C are special function of u, v, v', and involve n as a parameter.

The following five characteristic properties of an "n" system on a surface are derived by studying the differential equation of such a system.

I'. The "n" system possesses property I of section 2.

II′. For an "n" system, the focal locus or bicircular quartic associated by property I′ with each direction ξ through a point O, is such that the tangent of the angle which the tangent line to the quartic in the direction ξ makes with the force vector is to the tangent of the angle which the second tangent line to the quartic makes with the first tangent line as $n + 1$ is to 3.

III′. The "n" system possesses property III of section 2. The conic described in this property is a rectangular hyperbola.

IV′. For an "n" system, at any point O on the surface, the geodesic curvature of the line of force through O is equal to $(n + 1)$ times the geodesic curvature of the hyperosculating curve of property III′ which passes through O in the same direction.

V′. For an "n" system, as we move along the surface from a point O, the quantities ρ_1, ρ_2, R_1, R_2, ω, s_1, s_2, defined in property V of section 2, vary so as to satisfy the relation

$$\frac{\partial}{\partial s_2}\left(\frac{1}{\kappa_1}\right) - \frac{\partial}{\partial s_1}\left(\frac{1}{\kappa_2}\right) - \frac{1}{\rho_1\kappa_1} + \frac{1}{\rho_2\kappa_2} - \frac{\partial^2(\log \omega)}{\partial s_1\, \partial s_2} = 0,$$

where

$$\frac{1}{\kappa_1} = \omega\left(\frac{1}{\rho_1} - \frac{n+1}{R_1}\right), \quad \frac{1}{\kappa_2} = \frac{1}{\omega}\left(\frac{1}{\rho_2} - \frac{n+1}{R_2}\right).$$

If, in these five properties, we replace the direction of the force vector through a point by the direction of the tangent line to the rectangular hyperbola of property III′, we may state the theorem:

In order that a triply infinite system of curves on a surface may be identified with an "n" system, the given system must possess properties I′, II′, III′, IV′, V′.

---

[1] This is a summary of a long memoir offered for publication to the *Ann. Math., Princeton, N. J.* For a study of the corresponding problem in a plane or in ordinary 3-space, see Kasner, E., *Trans. Amer. Math. Soc., New York,* **7,** 1906 (401–424); also *Ibid.,* **8,** 1907 (135–158). For a summary of these two papers, see *Princeton Colloquium Lectures,* Chap. I.

[2] Dots refer to total derivatives with respect to the time t, and primes refer to total derivatives with respect to the parameter u.

[3] For a characterization of the ∞^2 curves of a natural system on a surface, defined by $\int F\, ds = minimum$, where F is a function of the coördinates only, see Lipka, J., *Ann. Math., Princeton, N. J.,* **15,** 1913 (71–77).

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CONTENTS

Page

GEOPHYSICS.—PRESENT STATUS OF GEODESY AND SOME OF THE PROBLEMS OF THIS BRANCH OF GEOPHYSICS . . . . *By William Bowie* . 545
——— THE PROBLEMS OF SEISMOLOGY . . . . . *By Harry Fielding Reid* 555
——— THE STATUS AND PROBLEMS OF METEOROLOGY . . *By C. F. Marvin* 561
——— SOME OF THE CHIEF PROBLEMS IN TERRESTRIAL MAGNETISM AND ELECTRICITY . . . . *By Louis A. Bauer* 572
——— THE PROBLEMS AND FUNCTIONS OF THE SECTION OF PHYSICAL OCEANOGRAPHY OF THE AMERICAN GEOPHYSICAL UNION . . *By G. W. Littlehales* 580
——— THE PROBLEMS OF VOLCANOLOGY . . . *By Henry S. Washington* 583
——— AN OUTLINE OF GEOPHYSICAL-CHEMICAL PROBLEMS . . . . . *By Robert B. Sosman* 592
CHEMISTRY.—THE IONIZATION OF STRONG ELECTROLYTES . . . . . *By William D. Harkins* 601
PHYSICS.—ON A CONDITION FOR HELMHOLTZ'S EQUATION SIMILAR TO LAMÉ'S *By Arthur Gordon Webster* 605
——— THE ABSORPTION OF X-RAYS BY CHEMICAL ELEMENTS OF HIGH ATOMIC NUMBERS . . . *By William Duane, Hugo Fricke and Wilhelm Stenström* 607
——— THE THOMSON EFFECT AND THERMAL CONDUCTION IN METALS . . . *By Edwin H. Hall* 613
MATHEMATICS.—MOTION ON A SURFACE FOR ANY POSITIONAL FIELD OF FORCE . *By Joseph Lipka* 621

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HOMOLOGOUS GENES AND LINEAR LINKAGE IN DROSOPHILA VIRILIS

By Alexander Weinstein

Garrison-on-Hudson, New York

Communicated by T. H. Morgan, September 30, 1920

Various species of Drosophila have produced mutations resembling in somatic appearance and in genetic behavior mutations in D. melanogaster.[1] In Sturtevant's case, involving notch in D. funebris, the resemblances to the notch of D. melanogaster are so numerous that it seems extremely probable that the two factors are similar genes in homologous loci. In the other cases, however, the similarities are not sufficient to substantiate such a conclusion.

Unfortunately, in all these instances, proof that the factors would act as allelomorphs if brought together is unavailable because the species studied do not hybridize with each other. However, even without data from hybridization, the homology of factors or of chromosomes in different species might still be detected if it were possible to demonstrate series of linked genes of which the similarly placed members have similar somatic effects. Metz has suggested that the yellow and forked genes in D. virilis are homologous with the similarly named genes in D. melanogaster. This identification, necessarily doubtful because only two factors were involved in each series, was rendered still more questionable by the discovery in D. melanogaster of singed (by Mohr) and inflated (by Weinstein[2]). The location of these factors is shown in figure 1*C*. Inflated resembles vesiculated; and if the two factors were considered homologous, it would be possible (by turning the virilis chromosome round) to homologize forked in virilis with singed in melanogaster. Singed resembles forked in its bristles, but in addition in singed flies the hairs are affected and the females are sterile. The resemblance, therefore, is not so good as that between the two forked characters; still the differences between forked in virilis and singed in melanogaster might be due to modifying factors, or the two genes might be multiple allelomorphs. While, therefore, the identification of forked and vesiculated with singed and inflated is not so plausible as the identification of the yellow forked series in the two species, it would be impossible in the absence of further evidence to decide which interpretation, if either, is correct.

The balance of proof has swung in favor of the identification of the two yellow forked series by the discovery of the mutant crossveinless in both D. melanogaster (by Bridges) and in D. virilis (by Weinstein). In crossveinless flies of virilis (fig. 2*c*) the posterior crossvein is entirely absent, except that very rarely a small segment in the middle of the crossvein persists; the anterior crossvein is very faint and may be partly missing.

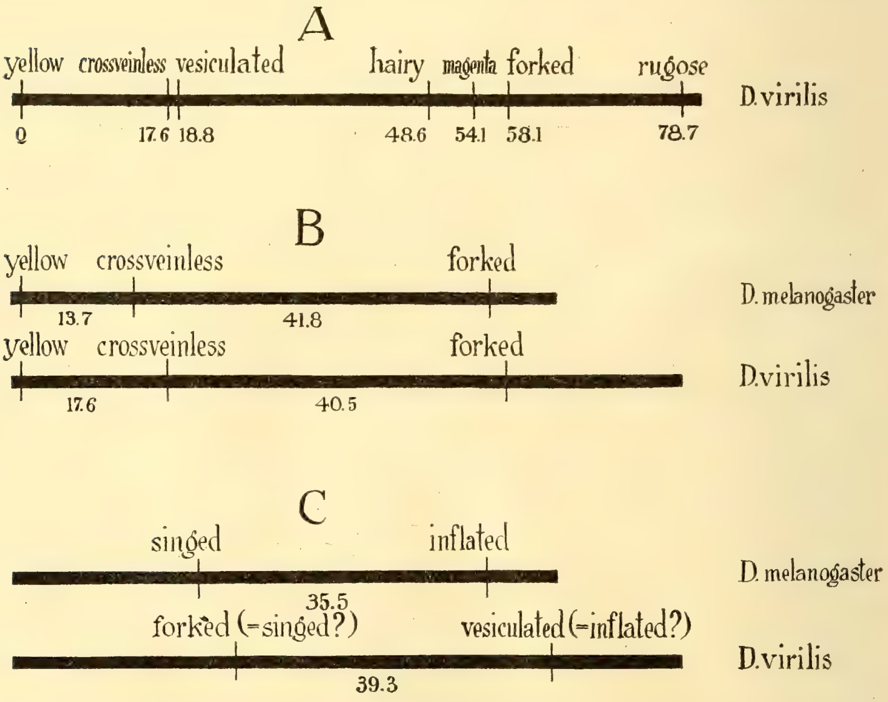
A
yellow
crossveinless
vesiculated
hairy
magenta
forked
rugose
D.virilis
0
17.6
18.8
48.6
54.1
58.1
78.7
B
yellow
crossveinless
forked
D. melanogaster
13.7
41.8
yellow
crossveinless
forked
D.virilis
17.6
40.5
C
singed
inflated
D. melanogaster
35.5
forked (=singed?)
vesiculated (=inflated?)
D.virilis
39.3

FIG. 1

In crossveinless mutants of melanogaster (fig. 2*b*), the posterior crossvein is missing, the anterior crossvein is very faint and almost entirely absent, and in addition the second longitudinal vein is slightly thickened at its distal end (see Bridges' paper elsewhere in this issue of the PROCEEDINGS, p. 660). Both factors are recessive and sex-linked, and occupy similar positions in the X chromosome with respect to yellow and forked (fig. 1*B*).

A map of the sex-linked factors of D. virilis is given in figure 1*A*.[3]

Crossveinless in Drosophila virilis.—Crossveinless in Drosophila virilis was discovered (November, 1917) in the sons of a female containing the factor yellow in one X chromosome and the factors vesiculated, hairy magenta, and forked in the other. In the offspring of this female (number 1309, table 4*D*), which had been mated by yellow males, it was noted that all the non-vesiculated sons were crossveinless. This indicated that the locus of the new factor was in the X chromosome very near the vesiculated locus, and that the mutation had occurred in the yellow-bearing chromosome or in one of its ancestors. This chromosome had come from yellow stock, and an examination of the stock showed that a large proportion of the yellow flies were crossveinless. The success of the mutation in establishing itself in the crowded conditions of a stock bottle indicated a good viability, and subsequent experiments have shown that the viability is comparable to that of wild-type flies. The excellent viability and the clear-cut nature of the crossveinless character will make it very useful in determining linkage relations in D. virilis.

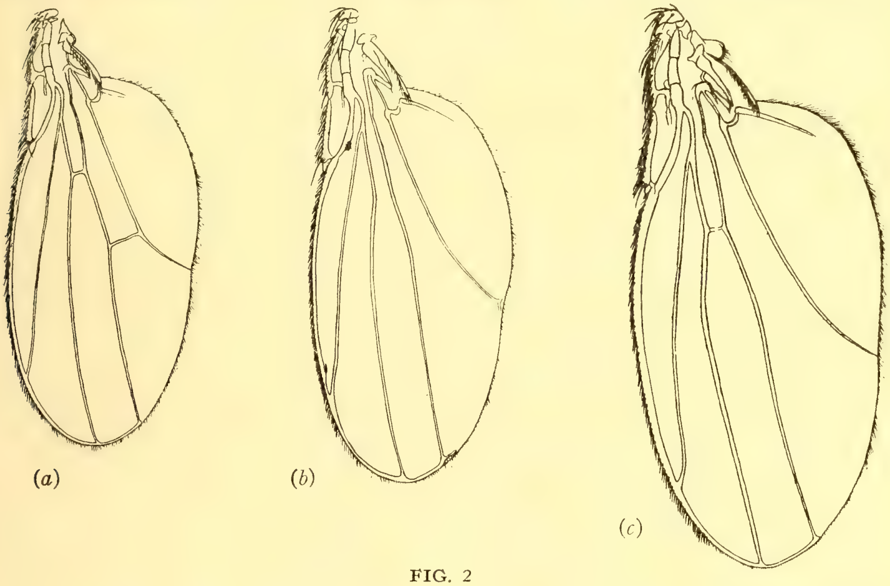

FIG. 2

(*a*) Wild-type wing of D. melanogaster (the wild-type venation of D. virilis is identical with this); (*b*) crossveinless wing of melanogaster; (*c*) crossveinless wing of virilis.

Crosses involving yellow, crossveinless, and vesiculated (table 1) show that the crossveinless locus lies between the other two, very near the vesiculated locus. The yellow crossveinless distance in these experiments was 19.23 units and the crossveinless vesiculated distance 1.15 units. The inclusion of other data (tables 2 and 3) lowers the yellow crossveinless distance to 17.6 units.

The determination of the crossveinless vesiculated distance may be subject to an error due to the fact that vesiculated is not a perfect diagnostic character. Vesiculated flies show the character sometimes in both wings, more often in only one, and occasionally (as we should therefore expect) in neither wing. This last point is proved by the fact that matings of vesiculated by vesiculated sometimes produce apparently normal flies which, however, behave genetically like vesiculated. Thus an apparently normal female from vesiculated stock mated by a vesiculated male produced 35 vesiculated and 3 normal females, and 48 vesiculated and 1 normal male. It is, therefore, possible that in the present experiment some non-crossover vesiculated flies appeared normal winged and were classed as crossovers between crossveinless and vesiculated. Similarly, some flies that were genetically yellow crossveinless vesiculated may have appeared and been classed as yellow crossveinless. This second error would tend to counterbalance the first, but only slightly, because the much larger size of the non-crossover class would make the number of vesiculated flies simulating normal much greater than the number of yellow crossveinless vesiculated flies simulating yellow crossveinless. Consequently, the crossveinless vesiculated distance, if in error, should be shorter than the calculated value. It seems, however, unlikely that in this experiment any large error was introduced in this way, because

TABLE 1

OFFSPRING OF $\frac{y \quad cv}{vs}$ FEMALES

CULTURE NO.	FEMALES	MALES						
		0		1		2		
		y cv	*vs*	*y vs*	*cv*	*y cv vs*	+	Totals
1591	62	36	29	4	4	1	3	77
1592	90	40	38	9	13		1	101
1593	69	24	29	5	5	1		64
1703	121	37	35	12	7	1		92
1704	113	59	40	9	14		1	123
1705	106	42	34	10	14		1	101
1706	120	42	43	11	9	1	1	107
1707	116	45	46	8	11		1	111
1708	52	24	21	2	1			48
1709	84	19	31	8	6	2		66
1710	88	34	43	7	6			90
1711	93	22	43	12	5			82
1712	91	38	32	11	14		1	96
1713	65	24	23	5	12		1	65
1714	59	12	18	8	1			39
1715	76	40	29	9	5	1		84
1716	88	32	23	7	6			68
1717	74	20	31	4	7			62
1718	91	31	33	7	12	1		84
Totals	1658	621	621	148	152	8	10	1560

TABLE 2

A. OFFSPRING OF $\frac{y \quad cv}{m \quad f}$ FEMALES

CULTURE NO.	FEMALES	MALES										
		0		1		2		3		1,2		1,3
		y cv	*m f*	*y m f*	*cv*	*y cv m f*	+	*y cv f*	*m*	*y*	*cv m f*	*y m*
1607	69	21	13	1	3	8	7					
1608	34	7	6	2		7	2		1	3		
1611	45	2	10		1	4	5		1		1	
1612	53	11	14			8	10	1				
1613	45	14	13	2	1	6	7			1	2	1
Totals...	246	55	56	5	5	33	31	1	2	4	3	1

B. OFFSPRING OF $\frac{y \quad cv \quad m \quad f}{r}$ FEMALES.

CULTURE NO.	FEMALES	MALES																		
		0		1		2		3	4		1,2		1,3	1,4		2,4		3,4	1,2,3	1,2,4
		y cv m f	*r*	*y r*	*cv m f*	*y cv r*	*m f*	*f*	*y cv m f r*	+	*y m f*	*cv r*	*cv m r*	*y*	*cv m f r*	*y cv*	*m f r*	*y cv m*	*y m r*	*cv*
1658	110	13	14	3	7	10	16		3	8	1	3				4	1	1		
1662	69	4	9	3	1	8	5			5		1		2		2		1		
1663	110	6	21	3	2	10	9			5	1		2	2		2	1	1		1
1670	118	16	20	8	1	8	5		3	4	1	2		3		7	1		1	1
1675	57	11	13	1	4	7	3	1	1	2	2				1	6				
Totals..	464	50	77	18	15	43	38	1	7	24	5	6	2	7	1	21	3	3	1	2

C. OFFSPRING OF $\frac{y \quad cv}{m \quad f \quad r}$ FEMALE

CULTURE NO.	FEMALES	MALES								
		0		1	2	3	4	1,4	2,3	2,4
		y cv	*m f r*	*cv*	+	*m*	*y cv r*	*cv r*	*y cv m*	*r*
1559	36	12	3	2	8	3	3	2	1	1

the complementary classes are so nearly equal. Moreover, if there was overlapping between vesiculated and normal, we should expect some genetically yellow vesiculated flies to appear merely yellow (thus simulating double crossovers); but no merely yellow flies were observed in this cross. Whatever error there may be does not influence the determination of the yellow crossveinless distance; furthermore, the order of the factors is not obscured.

Linkage of Crossveinless with Magenta and Forked.—Crosses of crossveinless with magenta and forked indicate that the crossveinless magenta distance is 36.0 and the magenta forked distance 2.7 units. This would place forked 38.7 units from crossveinless and 56.3 units from yellow. However, the crossveinless magenta distance is so long that double crossing over occurs within it (see tables 3B and 4C); and the discovery of other mutations within this region will probably move magenta and forked farther to the right. If the ratio of the crossveinless forked distances in D. virilis and D. melanogaster is the same as that of the yellow crossveinless regions (17.6 to 13.7), forked in D. virilis should be about 54 units from crossveinless and about 71 units from yellow.

The Locus of Hairy.—Crosses involving crossveinless and hairy (table 3) indicate that the two loci are about 31 units apart.

TABLE 3

A. SONS OF $\frac{y \quad cv}{\qquad h}$ FEMALES

CULTURE NO.	0		1		2		1,2	
	y cv	*h*	*y h*	*cv*	*y cv h*	+	*y*	*cv h*
1563	15	13	3	2	6	9	1	
1564	6	3				4		1
1567	5	7	1	2		4		
Totals...	26	23	4	4	6	17	1	1

B. SONS OF $\frac{y \quad cv \quad h \quad m}{\qquad}$ FEMALES

CULTURE NO.	0		1		2		3		1,2		1,3		2,3	
	y cv h m	+	*y*	*cv h m*	*y cv*	*h m*	*y cv h*	*m*	*y h m*	*cv*	*y m*	*cv h*	*y cv m*	*h*
1422	15	15	5	3	11	4			2	1		1		
1424	19	37	5	2	17	8	5	1		5	1	1	1	1
Totals......	34	52	10	5	28	12	5	1	2	6	1	2	1	1

The second part of this table suggests that hairy lies to the left of magenta, because when these two factors separate, yellow and crossveinless generally go with hairy. This is contrary to the conclusion of Metz (1918)[1] who provisionally located hairy between magenta and forked on the basis of crosses not involving hairy and magenta simultaneously. The following crosses (table 4), in which all three factors were present at the same time, show clearly that the actual order is hairy, magenta, forked.

TABLE 4

A. Offspring of $\frac{\quad m \quad f}{h}$ Females

CULTURE NO.	FEMALES	MALES					
		0		1		2	
		h	*m f*	*h m f*	+	*h f*	*m*
1531	60	52	7		2		
1539	60	16	17	1	2	2	3
Totals........	120	68	24	1	4	2	3

B. Offspring of $\frac{y \quad m \quad f}{h}$ Female

CULTURE NO.	FEMALES	MALES									
		0		1		2		3	1,2	1,3	
		y m f	*h*	*y h*	*m f*	*y*	*h m f*	*y m*	+	*y h f*	*m*
1352	83	13	24	16	9	1	4	1	2	1	1

C. Offspring of $\frac{y \qquad m \quad f}{vs \quad h}$ Females

CULTURE NO.	FEMALES	MALES											
		0		1		2		3	4	1,2		1,4	2,3
		y m f	*vs h*	*y vs h*	*m f*	*y h*	*vs m f*	*y*	*vs h f*	*y vs m f*	*h*	*m*	*vs*
1351	33	6	6	3	2	14	2	1			2		
1353	88	8	16	1	2	5		2	1	1	1	2	1
Totals...	121	14	22	4	4	19	2	3	1	1	3	2	1

TABLE 4—(*Continued*)

D. OFFSPRING OF $\frac{y}{vs\ \ h\ \ m\ \ f}$ FEMALE

CULTURE NO.	♀ ♀	MALES																						
		0		1		2		3		4		1,2		1,3		1,4		2,3		2,4		1,2,3	1,2,4	
		y	*vs h m f*	*y vs h m f*	+	*y h m f*	*vs*	*y m f*	*vs h*	*y f*	*vs h m*	*y vs*	*h m f*	*y vs h*	*m f*	*y vs h m*	*f*	*y h*	*vs m f*	*y h m*	*vs f*	*y vs m f*	*y vs f*	*h m*
1305	102	32	4	1	3	6	10					3	1		1		1	1						
1306	59	12	1	3	4	3	6				2	2	1	1		1								
1309	104	29	14		7	11	15	2	1			2	4			1			1					
1335	147	35	9	6	9	8	16	2	3		1	9	3	1			4	1	1					1
1338	98	23	2	1	8	4	5		1	1	1						2							
1340	87	12	1		3	4	7	2									3	1						
1342	64	18	3	1	5	8	14	2		4		1												
1343	84	36	6		6	8	17	4	1	1		1	2				2				1			
1344	76	27	9	1	3	7	14			1	1	2	2				1							
1359	68	24	6	1	4	3	6		1	1			2					2		1				
1360	84	17	6	1	4	5	11	2	2	1		1		3						1				
1361	54	16	3	4	2	4	3					2	1	1						1				
1362	83	13	10	4	4	5	11		1	2	1	3					1			1			1	
1363	53	16	5	4	4	10	9	1				1			1				1			1		
Totals...	1163	310	79	27	66	86	144	15	10	11	6	27	16	6	2	2	14	5	3	4	1	1	1	1

These results demonstrate, despite the great differential mortality in many broods, that the hairy locus is to the left of magenta; because whenever there is crossing over between hairy and magenta, forked goes with the linked gene in the magenta locus, and vesiculated (except for occasional double crossing over) goes with the linked gene in the hairy locus. If we consider only those broods in which there is no marked differential mortality (A1539 and B), we obtain the same result.

Linear Linkage in Drosophila virilis.—The data on the linkage of yellow, crossveinless, and vesiculated, and of hairy, magenta, and forked, are of interest because they demonstrate that in each case the three genes are in strictly linear order, the longest distance being the exact sum of the two shorter distances. Hitherto the only species for which such data have been presented has been D. melanogaster, in which, as Sturtevant, Bridges, and Morgan,[4] and Muller[5] have shown by reference to the results of critical tests, the arrangement of the genes is exactly linear. As a result of criticisms by these authors, Castle[6] has withdrawn his objections to the theory of linear linkage. In D. virilis, however, there have been hitherto no decisive data published. Metz's results, while they agree so far as they go with those in melanogaster, do not go far enough to give a decisive proof of linear arrangement. In the only case (that of hairy, magenta, and forked) in which the factors with which he worked were sufficiently close together to make a crucial test possible, he failed to make the necessary cross.

TABLE 5

A. OFFSPRING OF $\frac{y}{h \quad m}$ FEMALE

CULTURE NO.	FEMALES	MALES							TOTALS
		0		1		2		1,2	
		y	*h m*	*y h m*	+	*y m*	*h*	*m*	
1369	89	35	29	23	18	1	4	1	111

B. OFFSPRING OF $\frac{h}{m}$ FEMALE

CULTURE NO.	FEMALES	MALES			
		h	*m*	*h m*	TOTAL
1529	71	22	7	1	30

Castle, on the basis of his three dimensional theory and of Metz's data, arranged hairy, magenta, and forked, at the apices of a triangle, and predicted that the hairy magenta distance (which Metz had not determined) would be "about 4 or 5." The data now presented (tables 3 B, 4, and 5) give the hairy magenta distance as 5.5 units; or—if only those cultures are used in which the viability is good (tables 3 B; 4 A 1539, B; 5A)—the hairy magenta distance is 7.03 units.

Of course, because of the poor viability in the one case and the small numbers in the other, little reliance can be placed on these determinations as exact values. However (and this is the point at issue) the uncertainty does not affect the arrangement of the loci, which is clearly a linear one; for in all the experiments where the three loci were followed simultaneously, the frequency of separation of hairy and forked (0.1003) was the exact sum of the hairy magenta (0.0534) and the magenta forked (0.0469) separation frequencies.

Now the linear order, besides disproving Castle's triangular arrangement, shows that the values he used were not consistent with each other; for the hairy forked distance, being necessarily longer than the magenta forked distance, cannot be 3.1 if magenta forked is 3.7. The two distances, not being strictly comparable, should not be used to predict the hairy magenta distance definitely. Moreover, if the hairy magenta distance is (as Castle predicts) 4 or 5, the hairy forked distance must be longer than 4 or 5 and cannot be 3.1; that is, if Castle's predicted value is correct it follows that the value on which the prediction was based is incorrect. Thus the fulfilment of the prediction would in itself invalidate the grounds on which the prediction was based. It is therefore of no consequence whether or not the hairy magenta value recorded in this paper (5.5 for the total broods or 7.03 for those with good viability) be considered in agreement with Castle's "about 4 or 5 units"; for any agreement must be accidental and the fulfilment or non-fulfilment of Castle's prediction can have only a dramatic and not a scientific value.[7]

The loss of the hairy stock prevents any further determinations; but the data already obtained are sufficient to demonstrate that the hairy, magenta, and forked loci are arranged in strictly linear order. It has already been noted that in the cross involving simultaneously yellow, crossveinless and vesiculated, the yellow vesiculated separation frequency (20.38) is the exact sum of the yellow crossveinless (19.23) and the crossveinless vesiculated (1.15) separation frequencies. Hence, in the two only cases in D. virilis in which (because of the absence of double crossing over) the linear theory can be subjected to a decisive test, the arrangement of the genes in the chromosome turns out to be precisely linear.[8]

Castle also predicted, on the basis of his theory, that rugose and glazed would give a crossing over value of about 4 or 5 percent.; but this prediction cannot be tested directly because rugose-glazed hybrids are sterile. It is, of course, possible to determine the crossing over value of each gene with the nearest other factor and to compare the distances thus obtained. But such an experiment is necessarily inconclusive because variations in linkage caused by other genetic factors are difficult or impossible to eliminate unless the loci to be compared are followed in the same cross.

Castle's prediction can, however, be disproved in another way, because there is evidence that rugose and glazed are allelomorphs. This is indicated by the similarity of their linkage relations (so far as tested) and of their somatic effects. Rugose dominates glazed, but this cannot be considered evidence of allelomorphism because rugose females, and hence rugose-glazed hybrid females, are wild-type in appearance. (As the factors are sex-linked there are, of course, no males carrying both together.) However, I have found that glazed females are almost invariably sterile. (Out of 146 females tested in my experiments, either singly or in mass cultures, only two produced offspring.) The sterility of glazed, while it is recessive to wild-type fertility, is dominant to the fertility of rugose, so that rugose-glazed hybrid females are sterile. This suggests strongly that the rugose and glazed genes are allelomorphic; and the evidence is strengthened by the behavior of a third mutant gene, wax, which in its somatic effects resembles rugose and glazed (though it is more extreme) and is, like them, sex-limited. Wax females, I have found, are also sterile. (Of 89 tested in my experiments, all but two failed to give offspring.) Wax eye is recessive to wild-type and to rugose, and the sterility of wax is recessive to the fertility of wild-type, but dominant to the fertility of rugose. (None of the 84 rugose-wax females tested in my experiments produced offspring.) Wax also, therefore, seems to be allelomorphic to rugose, and this is confirmed by the fact that glazed and wax behave as allelomorphs when crossed: the hybrid is intermediate and, as would be expected, sterile. (None of the 106 tested produced offspring.)[9]

Since the evidence indicates that rugose and glazed are allelomorphs, the amount of crossing over between them, if it could be directly determined, would be not about 4 or 5 per cent (as Castle predicted), but 0.

Coincidence in Drosophila virilis.—While the data in Drosophila virilis are not sufficiently extensive for any detailed considerations of coincidence, they indicate, as far as they go, that the coincidence of various regions resembles that of the corresponding regions of the X chromosome in D. melanogaster.

The values of table 6, because of the smallness of the figures and the uncertainty of vesiculated as a diagnostic character, are subject to large error; but they all agree in indicating that coincidence first

TABLE 6

COINCIDENCE IN D. VIRILIS

REGIONS	COINCIDENCE	SOURCE OF DATA
Yellow crossveinless and crossveinless vesiculated	0	This paper, table 1
Yellow crossveinless and crossveinless magenta	0.848	This paper, tables 2, 3B
Yellow vesiculated and vesiculated magenta	0.977	Metz, 1918, tables 11, 19, 22
Yellow crossveinless and magenta forked	1.4	This paper, table 2
Yellow vesiculated and magenta forked	0.989	Metz, 1918 table 19
Yellow crossveinless and forked rugose	0.795	This paper, tables 2B, C
Yellow vesiculated and forked rugose or forked glazed	0.590	Metz, 1918 tables 20, 21

increases and then decreases as the regions considered become separated farther. This agrees very closely with the conditions shown to exist in the X chromosome of D. melanogaster by Muller (1916)[10] and Weinstein (1918).[2] A similar rise and fall of coincidence have been demonstrated for the second chromosome of D. melanogaster by Bridges[2] and for the third chromosome by Gowen (1919).[11]

The Homology of Apparently Similar Factors.—Although the evidence so far obtained favors the homology of the yellow crossveinless forked series in D. virilis and D. melanogaster, it should be kept in mind that the data are by no means sufficient to make a homology certain for it is known that similar somatic effects may be produced by non-homologous genes. Thus in melanogaster there are mutant characters that resemble each other in appearance but are due to mutant genes in different loci; for example, the various eye colors resembling pink (ruby, garnet, purple, maroon, claret, etc.) and the various body colors resembling black (sable, ebony). In some cases two such "mimic" genes have been found to be very close together in the same chromosome, as in the case of miniature and dusky, which are not more than 1.8 units apart. It is obvious that a miniature mutation in virilis, even if it showed closely similar linkage relations, could not be definitely homologized with either of these two genes. A yellow virilis male with miniature wings did, indeed, occur in a cross of a yellow female by a wild-type male; and this miniature variation, if a mutation and recessive, must have been sex-linked. Unfortunately it proved sterile and its linkage relations could not be tested. Since mutations that have similar somatic effects may occur in homologous chromosomes of the same species and occupy non-homologous loci, it is certainly hazardous to take it for granted that mutations of similar effect occurring in homologous chromosomes of different species must lie in homologous loci.

A case particularly in point in the present connection is that of the body color chlorotic, resembling yellow; its gene is close to the yellow gene in the melanogaster X chromosome. Another case is that of magenta in virilis. If forked were not known in this species, magenta might have been homologized with garnet, which is twelve units to the left of forked in melanogaster. While this identification is not entirely impossible, it is rendered rather improbable by the difference in distance from forked and by the large number of magenta-like mutants in melanogaster. The cases of singed and inflated have already been considered.

Not only may similar effects be caused by non-homologous genes, but two originally identical genes may, because of the mutation of one, or the mutation of both in different directions, or because of differences in modifying factors, come to have different somatic effects. Modifying factors may also affect the strength of linkage. The differences between crossveinless in melanogaster and virilis, and the differences between the lengths of the yellow crossveinless and the crossveinless forked distances in the two species, might be thus explained.

The order of the genes would not be affected by modifying factors (though if crossing over were entirely prevented, the order would be impossible to determine, at least in the ordinary way). But Bridges's cases of duplication and transposition[12] show that genes may be shifted to another region of the chromosome, or even to a different chromosome, without being otherwise affected; so that homologous (and indeed apparently identical) genes may come to have different linkage relations. By a combination of mutation and transposition, homologous factors might come to differ in both chemical composition and in linkage relations.

Further complications might be introduced because of duplication and of deficiency. In the vermilion duplication stock of melanogaster, each X chromosome carries two vermilion genes. Though one vermilion gene is recessive to wild-type, two vermilion genes dominate one not-vermilion. If a low cross-over factor were introduced, the two vermilion genes would remain together and would simulate the behavior of a single dominant gene. If vermilion-deficiency were substituted for one vermilion gene, the vermilion vermilion-deficiency combination would simulate a dominant vermilion with a recessive lethal effect.[13]

A similar reversal of dominance might occur because of the presence of several recessive genes in tetraploidy. Such a case could be distinguished by the frequencies of the different classes, for each factor would act as an allelomorph to each of the others.[14] Finally, there might also be reversal of dominance because of modifying factors.[15]

It must therefore be understood that while the discovery of crossveinless in D. virilis and D. melanogaster increases the probability that the X chromosomes of the two species are composed of homologous genes, a demonstration of such homology in the absence of hybridizing experiments requires a greater number of similar genes similarly placed than have yet appeared.

Are Genes with Similar Somatic Effects Chemically Similar?—It might be supposed that factors with similar somatic effects are chemically similar even if they occupy different loci. But this supposition can be disproved by a consideration of two such factors as ruby and garnet in D. melanogaster. Each of these produces a pink eye, but the genes lie in different parts of the X chromosome. A ruby female has the composition rG/rG, a garnet female the composition Rg/Rg. Now, if $r = g$ and $R = G$, either of the above genetic compositions should produce the same effect as the composition rg/RG or rG/Rg; for in each of these cases there are two (supposedly similar) genes for pink eye color (r and g) and two (supposedly similar) allelomorphs of these (R and G). But the composition rg/RG or rG/Rg produces not a pink but a red (wild-type) eye. Hence either r differs from g, or R differ from G, or both.

This proof depends on the assumption that the position of a gene does not influence its somatic expression. The correctness of this assumption is demonstrated by Bridges'[11] cases of duplication and transposition already referred to, in which the shifting of a factor from one part of a chromosome to another or even to a different chromosome does not alter its somatic effect.

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[1] Hyde, R. R., *Amer. Nat.*, **49**, 1915 (183–85, 185–187); Metz, C. W., *Genetics*, **1**, 1916 (591–607); *Ibid.*, **3**, 1918 (107–34); Sturtevant, A. H., *Science*, **48**, 1918 (72–3).

[2] Weinstein, A., *Genetics*, **3**, 1918 (135–73).

[3] D. virilis itself has been described by Sturtevant, *Ann. Ent. Soc. Amer.*, **9**, 1916 (323–43). Descriptions of the sex-linked mutants other than crossveinless will be found in Metz, *l. c.* Dscriptions of the sex-linked characters in D. melanogaster will be found in Morgan and Bridges, *Carnegie Institution Washington, Publ.*, No. 237, 1916 (1–87).

[4] Sturtevant, A. H., Bridges, C. B., Morgan, T. H., These PROCEEDINGS, **5**, 1919 (168–173); Morgan, T. H., Sturtevant, A. H., and Bridges, C. B., *Ibid.*, **6**, 1920 (162–164).

[5] Muller, H. J., *Amer. Nat.*, **54**, 1920 (97–121).

[6] Castle, W. E., These PROCEEDINGS, **5**, 1919 (25–32, 32–36); *Ibid.*, **5**, 1919 (500–506); *Ibid.*, **6**, 1920 (73–77).

[7] It was pointed out in Muller's article that the value which I had recently obtained for the hairy magenta distance was (on the data then available) 6.6, contrary to Castle's prediction; and that this, if combined according to Castle's own method with the values previously obtained by Metz in separate crosses for the hairy forked and forked magenta distances, would place the three factors in a nearly straight line in the order hairy forked magenta. This situation was used by Muller to show that, even by using Castle's method of combining separate experiments, linear results might be arrived at that were quite inconsistent with the prediction obtained by the same method. The new value, moreover, was based on more flies (total 364) than Metz's hairy forked value used by Castle (162). Muller made no claim, however, that these relationships were really correct, as it was evident that linear results arrived at by such a method

might be accidental, just as Castle's non-linear results so obtained were accidental. The fact that the order really turns out to be hairy magenta forked illustrates the latter point to a nicety and falls in line with the other results to show the fallacy of the method of combining separate experiments for a determination of the nature of linkage.

[8] It is of course possible that larger counts would give double crossovers in these cases. But this is very improbable in the case of hairy, magenta, and forked, because of the shortness of the distance involved; and even in the case of yellow, crossveinless, and vesiculated, where the distance is longer, there could scarcely be enough double crossing over to throw the factors much out of line.

[9] The sterility of rugose-glazed hybrids was first observed by Metz, who attributed it to "incompatibility," because he thought that glazed females were fertile. He had noticed their sterility when mated to rugose males, but he termed this also "incompatibility," as he believed that glazed females were not sterile when mated to glazed or to wild-type males. Now that it is known that glazed females are almost always sterile, it is obvious that the failure of the hybrids to produce offspring is due to the dominance of the sterility of glazed. In my work, each class of females (glazed, wax, and hybrids) was tested with four kinds of males (wild-type, rugose, glazed, and wax); and in all cases, with the exceptions already noted, the matings produced no offspring, though all four types of males are otherwise fertile. Metz has, consequently, withdrawn his "incompatibility" theory and accepted the interpretation given above.

[10] Muller, H. J., *Amer. Nat.*, **50,** 1916 (193–221, 284–305, 350-366, 421–434).

[11] Gowen, J. W., *Genetics,* **4,** 1919 (205–250).

[12] Bridges, C. B., *Genetics,* **2,** 1917 (445–465); *J. Gen. Physiol.*, **1,** 1919 (645–656).

[13] The yellow genes in mice and rats suggest themselves in this connection; but there is no proof that the yellow mouse case is actually like the hypothetical vermilion vermilion-deficiency combination described above, or that yellow in mice is homologous with either of the yellow factors in rats.

[14] Muller, H. J., *Amer. Nat.*, **48,** 1914 (508–512).

[15] Altenburg, E., and Muller, H. J., *Genetics,* **3,** 1920 (1–59); Bridges, C. B., and Mohr, O. L., *Genetics,* **4,** 1919 (283–306).

THE INTENSITIES OF X-RAYS OF THE L-SERIES, III. CRITICAL POTENTIALS OF THE PLATINUM AND TUNGSTEN LINES

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This work is a continuation of that of Webster and Clark[1] and Webster.[2] The methods used by them are applied to the classification of some lines that were doubtful, and the general validity of the intensity laws is tested by the use of a different metal as anticathode (tungsten).

Apparatus.—The high tension outfit constructed at the Massachusetts Institute of Technology by Prof. D. L. Webster and described in these PROCEEDINGS[3] was used without essential modification. The Chaffee voltmeter was calibrated by measuring the current through a megohm resistance in parallel with it, and also checked by measurement of the high frequency limit of the spectrum.

The spectroscopic arrangement consisted of a large calcite crystal with a face $5^1/_2 \times 8$ cm., with a wide slit (1 cm.) placed close to the crystal and the plate about 70 cm. from the crystal. The anticathode was about 20 cm. from the crystal. With this simple setup a greater resolving power and dispersion was obtained than with the mica spectrograph used by Prof. Webster. Irregularities in the crystal gave little trouble. To exclude stray radiation the plate holder was placed in a lead box extending nearly to the crystal. In this way a considerable region of the spectrum can be mapped at once without moving the crystal; the range from 0.900 Å to 1.24 Å in the platinum spectrum, for instance, being spread over about 4 cm.

No attempt was made to measure wave-lengths directly, but for new lines the values could be found to about 0.3% by intperolation on the plate. Tubes with platinum and tungsten anticathodes and thin glass windows were kindly furnished by the General Electric Co. The former contained no iridium, but high voltage pictures showed a faint tungsten spectrum.

L-Series of Platinum and Tungsten.—The platinum and tungsten L-series, containing all lines observed with certainty here, is shown in Fig. 1. Reproductions of photographs of the region from γ_4 to β_4 are shown in Fig. 2, *a* and *b*. The tungsten spectrum has received special attention, and Dershem,[4] Overn,[5] de Broglie[6] and Siegbahn[7] have reported a large number of faint lines. All of those recorded by Siegbahn have been found, but two of the faint ones recorded by Overn only have not been observed.

In platinum, besides the 13 lines that have been followed through from element to element, we find two more lines. β_6 and γ_5, of which the wave-lengths as interpolated are 1.080 and 0.988, respectively. β_6 has been observed by Webster, while γ_5 has apparently escaped attention so far. That these two lines are really part of the platinum L-series is supported by the fact that lines occur in corresponding positions in tungsten. Thus the wave-length interval from γ_5 to γ_1, in tungsten is approximately the same as in platinum, and the same is true of the interval from β_6 to β_5.

There seems to be a real difference between the tungsten and platinum spectra as observed here. With pictures taken under similar conditions in the two elements, the relative intensity of the lines is not always the same, and some lines occur in tungsten which are apparently absent in platinum. Thus β_7, β_8, and β_9 and γ_7 which appear in tungsten are not found at all in platinum. For intensity differences, β_5 is much stronger than β_6 in platinum, but they are of about the same intensity in tungsten. There is also a change with γ_2, which is stronger than γ_3 in platinum, but weaker than γ_3 in tungsten. In the case of both β_5 and γ_2 we are dealing with lines which lie near the absorption limits, and as there is some evidence to show a progressive shift from element to element of the absorption limits relative to β_5 and γ_2 which would bring the absorption limits on opposite sides of these two lines in platinum and tungsten, this may account for the difference in intensity here.

Another difference in intensity occurs in the case of β_4, which is stronger in platinum than it is in tungsten, but we can ascribe it to a different cause. Noticing that β_7 which is between β_1 and β_4 in tungsten is apparently absent in platinum, and that if β_4 and β_7 in tungsten were to stay at a constant wave-length interval from β_3 and β_2 respectively, they would nearly coincide in platinum, we can think of β_4 as in reality composed of these two lines. Since β_3 and β_4 belong together in L_3 and β_2 and β_7 in L_1, we would expect this wave-length interval to remain constant.

Wave-lengths of the tungsten and platinum lines in the L-series are given in table I. Siegbahn calls the line at 1.2031 β_5 but this puts it out of line with β_5 in other elements. The wave-length interval from β_2 to β_5 is quite constant from element to element, and if it is not to be far off here, we would have to take the line at 1.2118 Å as β_5.

TABLE I

LINE	PLATINUM	TUNGSTEN	LINE	PLATINUM	TUNGSTEN
l	1.499	1.67505	η	1.242	1.4177
α_1	1.313	1.47348	γ_1	0.958	1.09553
α_2	1.323	1.48452	γ_2	0.933	1.06584
β_1	1.120	1.27917	γ_3	0.929	1.05965
β_2	1.101	1.24191	γ_4	0.900	1.02647
β_3	1.098	1.26000	γ_5	0.988	1.1284
β_4	1.142	1.29874	γ_6	.....	1.079*
β_5	1.072	1.2031	γ_7	.....	1.072
β_6	1.080	1.2118	γ_8	.....	1.044*
β_7	.....	1.2871			
β_8	.....	1.2205			
β_9	.....	1.202			

*Observed by Overn only.

Assignment of the Lines to Sub-Series.—The existence in platinum of the three series L_1, L_2, and L_3 with critical potentials near the quantum potentials of β_5, γ_2 and γ_4 respectively was shown by Webster and Clark[1] and Webster.[2] The stronger lines were measured directly by ionization methods, and the weaker ones were classified by means of constant voltage photographs. The following lines have been definitely assigned, by measurements on platinum alone:

L_1 l, α_2, α_1, β_2, β_6, β_5
L_2 η, β_2, γ_1
L_3 γ_4

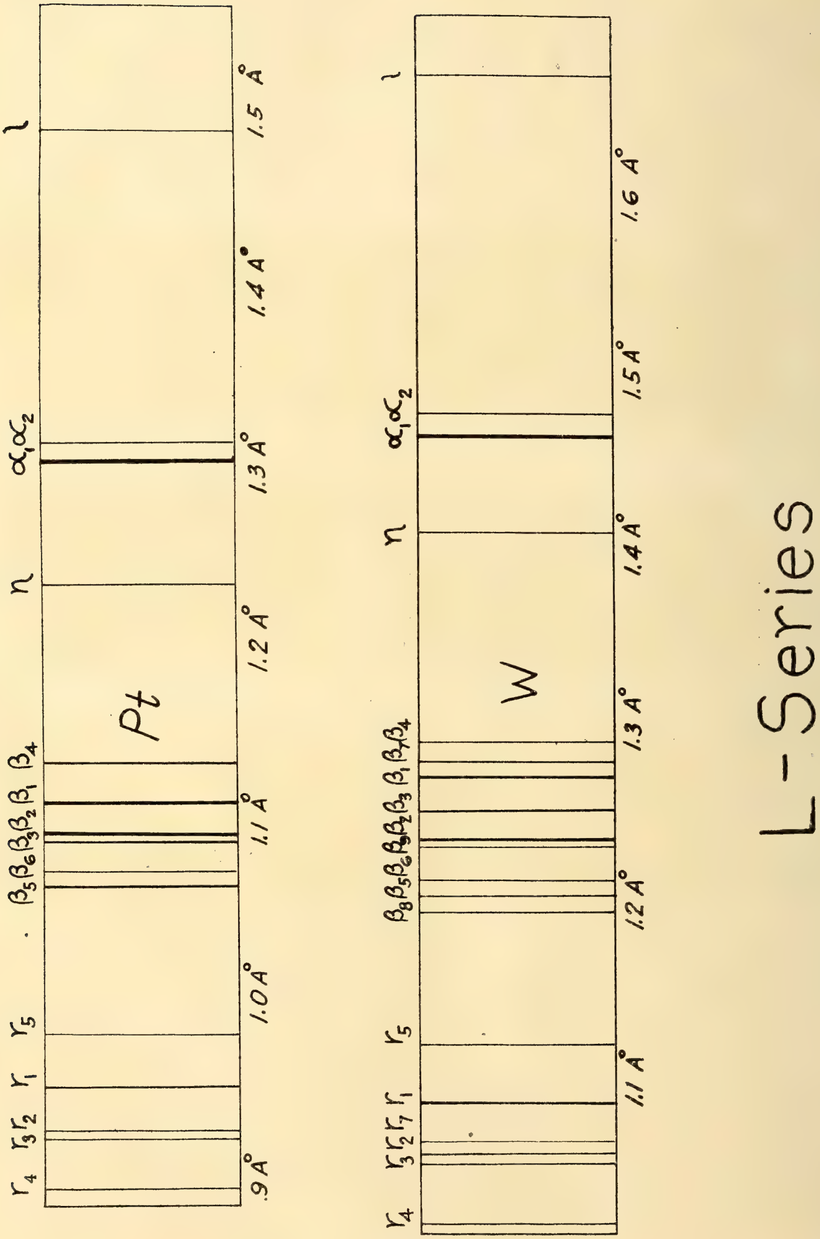
Pt
l
$\alpha_1\alpha_2$
η
$\beta_5\beta_6\beta_3\beta_2\beta_1\beta_4$
γ_5
γ_1
$\gamma_3\gamma_2$
γ_4
1.5 Å
1.4 Å
1.3 Å
1.2 Å
1.1 Å
1.0 Å
.9 Å
W
l
$\alpha_1\alpha_2$
η
$\beta_8\beta_5\beta_6\beta_9\beta_2\beta_3\beta_1\beta_7\beta_4$
γ_5
$\gamma_3\gamma_2\gamma_7\gamma_1$
γ_4
1.6 Å
1.5 Å
1.4 Å
1.3 Å
1.2 Å
1.1 Å
L-Series

FIG. 1

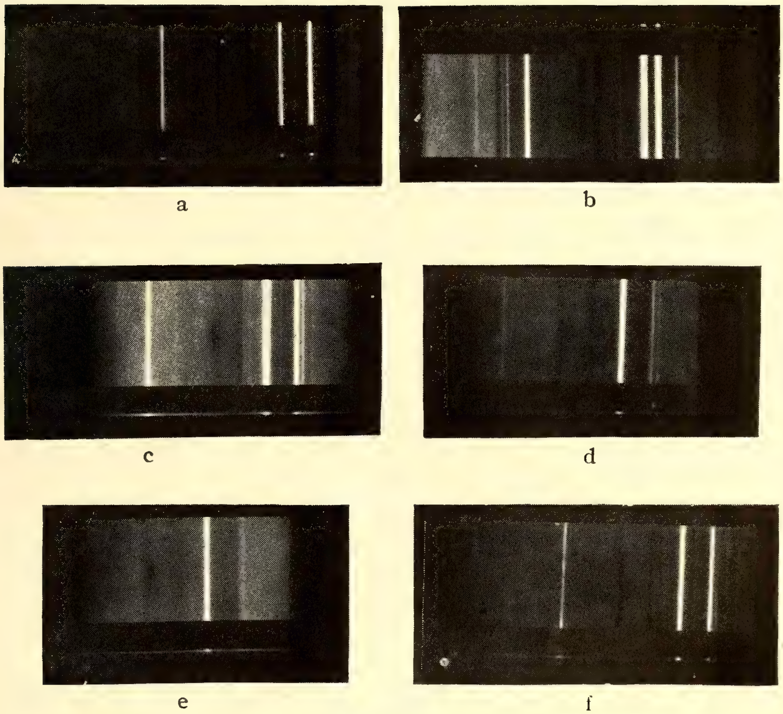

FIG. 2

In the present work these results have been confirmed. This leaves as still doubtful in platinum γ_2, γ_3, γ_5, β_4 and β_3. Of these still unclassified lines, γ_2 and γ_3 are so faint that although well resolved in all pictures, the results are still uncertain. The possibility of a fourth critical potential, as might be indicated by Sommerfeld's theory, makes these lines particularly interesting. γ_5 is too weak to show except at high voltages, with any degree of certainty. For all of these very weak lines it will be necessary to cut down the general radiation by using greater dispersion (plate farther from crystal) than is at present possible without unduly weakening the lines themselves by absorption in the air. For this purpose a vacuum spectrograph is now under construction.

β_3 is so close to β_2 in platinum as to make work on this line very difficult, but in tungsten it lies midway between β_1 and β_2. With the platinum anticathode used, where the tungsten lines also showed, the coincidence of W γ_1 with Pt β_3 was another complication, so that results were obtained on this line only in tungsten.

Pt β_4 has given rather anomalous results. Webster and Clark's[1] ionization curves showed a critical potential near V_2, but the uncertainty here was greater than with the stronger lines. In Webster's[2] photographs, however, it showed well below V_2, but the presence of a line here could be ascribed to the near coincidence of Ir β_2 with Pt β_4. With the present anticathode there were no iridium lines, and yet β_4 showed below V_2. We can account for this if we regard it as the combined line β_4 and β_7. The work with tungsten showed β_7 to be L_1 and β_4 to be L_3, so that the part showing below V_2 is simply β_7. That the ionization work indicated a critical potential the same as that of β_1 can only be ascribed to the uncertainty of the measurements on a line as weak as β_4 and to the disturbing effect of β_7, which would tend to decrease the apparent critical potential.

The results on tungsten are well illustrated by the reproductions in Fig. 2, *c*, *d*, *e*, and *f*. In *c*, *d*, and *e*, the product of current through the tube, time of exposure, and square of the voltage was made the same in all three. In *f* the time of exposure was shorter. The critical potentials calculated for tungsten would be:

V_1 10.2 kilovolts [Quantum potential of β_5]
V_2 11.6 kilovolts [Quantum potential of γ_2]
V_3 12.0 kilovolts [Quantum potential of γ_4]

These constant voltage pictures show us how β_4 and β_7 are to be classified. In *f* at 13 kv. we have them showing at nearly equal intensities. At 12.5 kv. β_4 is noticeably weaker, and at 12 kv. (V_3) β_4 is out completely, assigning it to L_3, while β_7 is still strong. At 11.5 kv., below V_2, β_7 still shows, although faint, assigning it to L_1.

That β_5 and β_6 belong together in L_1 is shown by the fact that they show with constant intensity ratio down to $11^1/_2$ kv.

β_3 disappears at 12 kv. placing it in L_3.

The evidence in these pictures would seem to put γ_2, γ_3 and γ_5 in L_3, as they show well at 12.5 kv., but are out at 12. They are still weaker, however, than β_7 which is very faint near its critical potential, so it is possible they would not show at 12 kv. even if belonging to L_2.

β_8, β_9 and γ_7 are too weak to show except at high voltages.

The behavior of the other lines that have already been classified is what would be expected. The change in intensity ratio of β_1 and β_2 is particularly noticeable. They are nearly the same at 13 kv., but quite different at 12. γ_1 behaves as would be predicted.

In some of the pictures the absorption limit of tungsten, due to absorption in the anticathode, shows near β_5, and there is also one near β_2. This latter has exactly the wave-length of Zinc $K\gamma$ and is probably due to zinc in the glass of the tube. The light spot showing in the region between γ_1 and β_5 is an imperfection in the crystal.

The classification of the lines, to date, is as given below. Those for which further work is necessary are marked with a question mark.

L_1	l, α_2, α_1, β_2, β_5, β_7
L_2	η, β_1, γ_1
L_3	β_3, β_4, γ_2(?), γ_3(?), γ_5(?), γ_4

It is to be noted that the photographic method as here employed does not enable us to determine the critical potential of each line independently, as does the ionization method; but only serves to classify lines if we assume they must all fall into three series, whose critical potentials are known. It is hoped to develop a method in which quantitative intensity measurements on the plate can be made. In this way curves similar to ionization-voltage curves can be obtained.

This work has been carried out under the direct supervision of Prof. D. L. Webster, without whose very material aid it would have been impossible.

[1] Webster, D. L., and Clark, H., these PROCEEDINGS, **3,** 1917 (181–5).

[2] Webster, D. L., *Ibid.*, **6,** 1920 (26).

[3] Webster, D. L., *Ibid.*, **6,** 1920 (269–72).

[4] Dershem, E., *Physic. Rev.*, Ithaca, **11,** 1918 (461–76).

[5] Overn, O. W., *Ibid.*, **13,** 1919 (137–42).

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[7] Siegbahn, M., *Phil. Mag., London,* **38,** 1919 (639).

SEMINVARIANTS OF A GENERAL SYSTEM OF LINEAR HOMOGENEOUS DIFFERENTIAL EQUATIONS

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Communicated by E. J. Wilczynski, September 27, 1920

The system of linear homogeneous differential equations

$$y_i^{(m)} + \sum_{l=0}^{m-1} \sum_{k=1}^{n} \binom{m}{l} p_{ikl} y_k^{(l)} = 0, \; (i = 1,2,\ldots,n), \qquad (A)$$

where

$$y_k^{(l)} = \frac{d^l y_k}{dx^l}$$

and where p_{ikl} are functions of the independent variable x, is evidently converted into another system of the same form by the transformation

$$y_k = \sum_{\lambda=1}^{n} \alpha_{k\lambda} \bar{y}_\lambda, \qquad (k = 1,2,\ldots,n), \qquad (1)$$

where $\alpha_{k\lambda}$ are arbitrary functions of x, and where the determinant $|\alpha_{k\lambda}| = \Delta$ does not vanish identically. Furthermore, it is known[1] that (1) is the most general transformation of the dependent variables which leaves (A) unchanged in form.

A function of the coefficients of (A) and their derivatives which has the same value for (A) as for every system derived from (A) by a transformation of the form (1) is called a *seminvariant*. The seminvariants of (A) have been calculated for the case[2] $n = 1$, $m =$ any positive integer, and for the case[3] $m = 2$, $n =$ any positive integer. It is the purpose of the present paper to obtain them for the general case of (A).

The calculations are considerably simplified by first obtaining the seminvariants in a so-called semi-canonical form and then expressing them in terms of the coefficients of (A). The possibility of a simplification of this general nature was first suggested by Green.[4]

The transformation (1) converts (A) into a new system (B) in which the coefficients of the derivatives of order $m - 1$ of the dependent variables are zero provided that α_{ij} is so selected that

$$\alpha'_{ij} = -\sum_{k=1}^{n} p_{i,k,m-1}\alpha_{kj} = \sum_{k=1}^{n} q_{ik1}\alpha_{kj}, \; (i,j = 1,2,\ldots,n). \quad (2)$$

Such a selection is always possible. The system (B) is the *semicanonical* form of (A).

When the coefficients of (1) are subject to the conditions (2) it is easily proved by induction that

$$\alpha_{ij}^{(\tau)} = \sum_{k=1}^{n} q_{ik\tau}\alpha_{kj}, \tag{3}$$

where

$$q_{ik\tau} = q'_{i,k,\tau-1} + \sum_{\lambda=1}^{n} q_{i,\lambda,\tau-1}q_{\lambda k1}.$$

If π_{ikl} denotes the coefficient of (B) corresponding to p_{ikl} of (A), we find by straight substitution

$$\Delta\pi_{ikl} = \sum_{\lambda=1}^{n} A_{\lambda i}\Bigg[\alpha_{\lambda k}^{(m-l)} + \sum_{\mu=1}^{n}\sum_{\nu=0}^{m-\lambda-l}\binom{m-l}{\nu} p_{\lambda,\mu,l+\nu}\alpha_{\mu k}^{(\nu)}\Bigg], \; (l = 0,1,\ldots,m-2), \tag{4}$$

where $A_{\lambda i}$ is the algebraic minor of $\alpha_{\lambda i}$ in Δ. By the use of (3) the expression (4) for π_{ikl} may be put into the form

$$\Delta\pi_{ikl} = \sum_{\lambda=1}^{n}\sum_{\mu=1}^{n} A_{\lambda i}\alpha_{\mu k}u_{\lambda\mu l}, \tag{5}$$

where

$$u_{\lambda\mu l} = q_{\lambda,\mu,m-l} - \sum_{\nu=1}^{n}\sum_{\tau=0}^{m-\lambda-l}\binom{m-l}{\tau} q_{\lambda,\nu,m-l-\tau}q_{\nu\mu\tau}.$$

Again, we find by differentiation

$$\Delta\pi'_{ikl} = \sum_{\lambda=1}^{n}\sum_{\mu=1}^{n} A_{\lambda i}\alpha_{\mu k}v_{\lambda\mu l}, \tag{6}$$

where

$$v_{\lambda\mu l} = u'_{\lambda\mu l} + \sum_{\nu=1}^{n}(q_{\nu\mu 1}u_{\lambda\nu l} - q_{\lambda\nu 1}u_{\nu\mu l}).$$

Similarly

$$\Delta\pi''_{ikl} = \sum_{\lambda=1}^{n}\sum_{\mu=1}^{n} A_{\lambda i}\alpha_{\mu k}w_{\lambda\mu l}, \tag{7}$$

where

$$w_{\lambda\mu l} = v'_{\lambda\mu l} + \sum_{\nu=1}^{n}(q_{\nu\mu 1}v_{\lambda\nu l} - q_{\lambda\nu 1}v_{\nu\mu l}).$$

From (2) it follows that the most general transformation of form (1) which leaves (B) in the semi-canonical form is given by

$$y_k = \sum_{\lambda=1}^{n} a_{k\lambda}\bar{y}_\lambda, \quad D = |a_{k\lambda}| \pm 0, \quad (k = 1,2,\ldots.,n) \qquad (8)$$

where $a_{k\lambda}$ are arbitrary constants. Equation (4) shows that such a transformation converts (B) into a new system whose coefficients $\bar{\pi}_{ikl}$ and their derivatives $\bar{\pi}_{ikl}^{(\tau)}$ are given by the equations

$$D\bar{\pi}_{ikl}^{(\tau)} = \sum_{\lambda=1}^{n}\sum_{\mu=1}^{n} A_{\lambda i}\pi_{\lambda\mu l}^{(\tau)}a_{\mu k}, \qquad (l = 0,1,\ldots.,m-2), \qquad (9)$$

where $A_{\lambda i}$ is the algebraic minor of $a_{\lambda i}$ in D.

If the transformation (8) is made infinitesimal, it is found that all seminvariants in their semi-canonical form must satisfy the system of partial differential equations

$$\sum_{\tau=0}^{\infty}\sum_{k=1}^{n}\left(\pi_{krl}^{(\tau)}\frac{\partial f}{\partial \pi_{ksl}^{(\tau)}} - \pi_{skl}^{(\tau)}\frac{\partial f}{\partial \pi_{rkl}^{(\tau)}}\right) = 0,$$

$$(r,s = 1,2\ldots n;\, l = 0,1,2\ldots,m-2). \qquad (10)$$

For $\tau = 0$, $l = m - 2$, there are n solutions given by[3]

$$I^{(roo)} = \frac{1}{r!}\sum_{i=1}^{n}\left(\frac{\partial}{\partial \pi_{i,i,m-2}}\right)^r \begin{vmatrix} \pi_{1,1,m-2} & \pi_{1,2,m-2}\cdots & \pi_{1,n,m-2} \\ \pi_{2,1,m-2} & \pi_{2,2,m-2}\cdots & \pi_{2,n,m-2} \\ \cdots & \cdots & \cdots \\ \pi_{n,1,m-2} & \pi_{n,2,m-2}\cdots & \pi_{n,n,m-2} \end{vmatrix}$$

$$(r = 0,1,\ldots,n-1)$$

For $\tau = 1, 2$, $l = m - 2$, solutions are given by[5]

$$I^{(rso)} = \frac{1}{s!}\sum_{i=1}^{n}\sum_{j=1}^{n}\left(\pi'_{i,j,m-2}\frac{\partial}{\partial \pi_{i,j,m-2}}\right)^s I^{(roo)}, \; (r + s \leq n),$$

$$I^{(rst)} = \frac{1}{t!}\sum_{i=1}^{n}\sum_{j=1}^{n}\left(\pi''_{i,j,m-2}\frac{\partial}{\partial \pi'_{i,j,m-2}}\right)^t I^{(rso)},$$

$$(r + s \leq n;\, t = 1,2,3;\, t \leq s).$$

For $\tau = 0$, $l = 0, 1, \ldots.,m - 3$, n^2 solutions for each value of l are given by

$$I_l^{(rst)} = \frac{1}{t!}\sum_{i=1}^{n}\sum_{j=1}^{n}\left(\pi_{ijl}\frac{\partial}{\partial \pi'_{i,j,m-2}}\right)^t I^{(rso)}, \; \left(\begin{matrix} r + s \leq n;\, t = 1,2;\, t \leq s; \\ l = 0,1,\ldots.,m-3 \end{matrix}\right).$$

The above seminvariants may be converted into seminvariants of (A) by means of equations (5), (6), (7). A comparison of these equations with (9) shows that the desired seminvariants of (A) are obtained simply by replacing in the above semi-canonical forms π_{ikl} *by* u_{ikl}, π'_{ikl} *by* v_{ikl} and π''_{ikl} by w_{ikl}.

A comparison of the seminvariants $I^{(rst)}$ of (A) with the corresponding seminvariants[5] for the case $n = 2$ shows the former to be independent. Moreover, the functional determinant of $I_l^{(rst)}$ with respect to u_{ikl} for each value of $l = 0, 1, 2 \ldots, m - 3$ shows that $I_l^{(rst)}$ are independent among themselves and of the seminvariants $I^{(rst)}$. Equations (10) show that we have the proper number of solutions for the variables involved and that all other seminvariants of the complete system can be obtained by the differentiation of $I^{(rst)}$ and $I_l^{(rst)}$. We have therefore the following theorem:

All seminvariants of (A) *are functions of* $I^{(rst)}$ $(r = 0, 1 \ldots, n - 1;\ r + s \leq n;\ t = 1, 2, 3;\ t \leq s)$, $I_l^{(rst)}$ $(r = 0,\ 1 \ldots, n - 1;\ r + s \leq n;\ t = 1, 2;\ t \leq s;\ l = 0, 1, \ldots m - 3)$, *and of the derivatives of* $I^{(rst)}$ $(t = 1, 2)$ *and* $I_l^{(rst)}$.

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[1] Wilczynski, E. J., *Projective Differential Geometry of Curves and Ruled Surfaces,* Teubner, Leipzig, Chap. I.

[2] Wilczynski, E. J., *Ibid.*, Chap. II.

[3] Stouffer, E. B., *London, Proc. Math. Soc.* (Ser. 2), **15,** 1916 (217–226).

[4] Green, G. M., *Trans. Amer. Math, Soc., New York,* **16,** 1915 (1–12).

[5] Stouffer, E. B., *London, Proc. Math. Soc.* (Ser. 2), **17,** 1919 (337–352).

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*SOME NEW METHODS IN INTERIOR BALLISTICS**

By Arthur Gordon Webster

Clark University, Worcester, Mass.

Read before the Academy, April 26, 1920.

The principal problem of interior ballistics is, given a particular gun, a particular shot, and a particular kind of powder, to find, for a given load, the position and velocity of the shot, the mean pressure (and incidentally temperature) of the gases in the gun, and the fraction of the powder burned, all as functions of the time or of each other until the exit of the shot from the muzzle of the gun. In particular, we wish to know the muzzle-velocity of the shot, the maximum pressure to which the gun will be exposed, and the portion of the bore which will be exposed to it. It is then the duty of the mechanical engineer to design a gun to safely resist the pressure that may be expected. Or it is the inverse problem of the ballistician, by experiments on the action of the powder in the gun, to find its properties and those of the gun. It is true that the properties of the powder are more conveniently studied by means of explosions in the bomb at constant volume, but we shall here show how all the properties may be determined by experiments in the gun alone.

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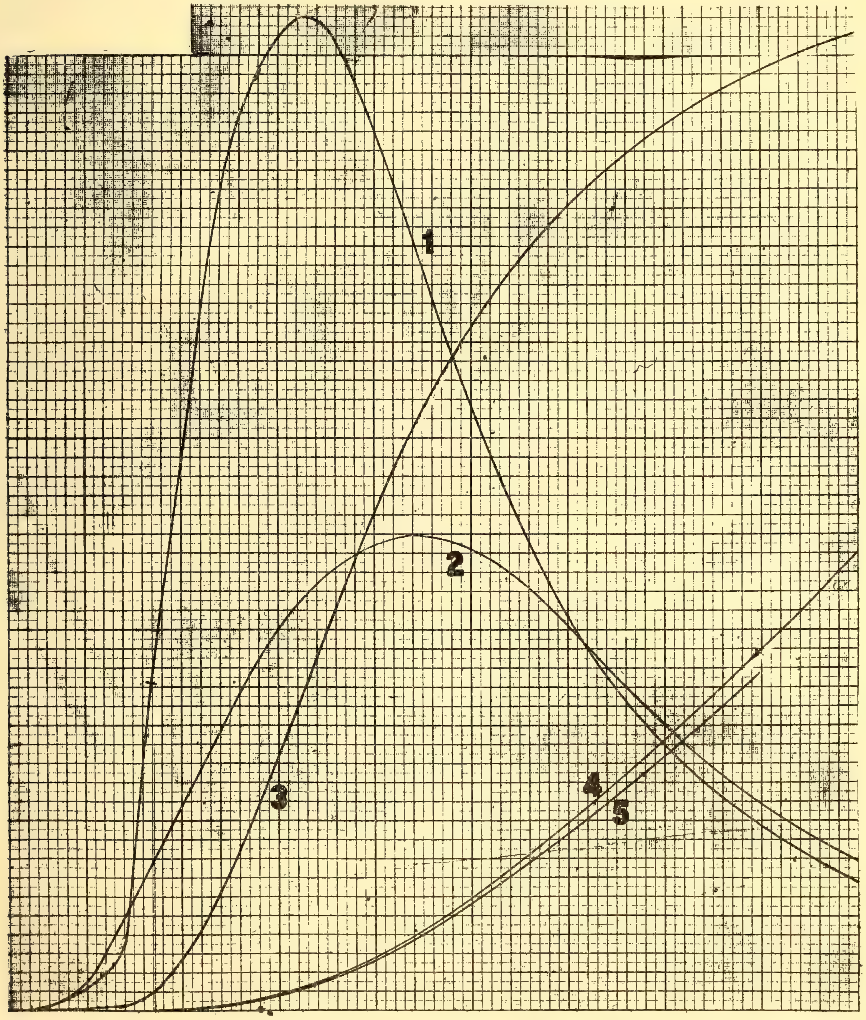
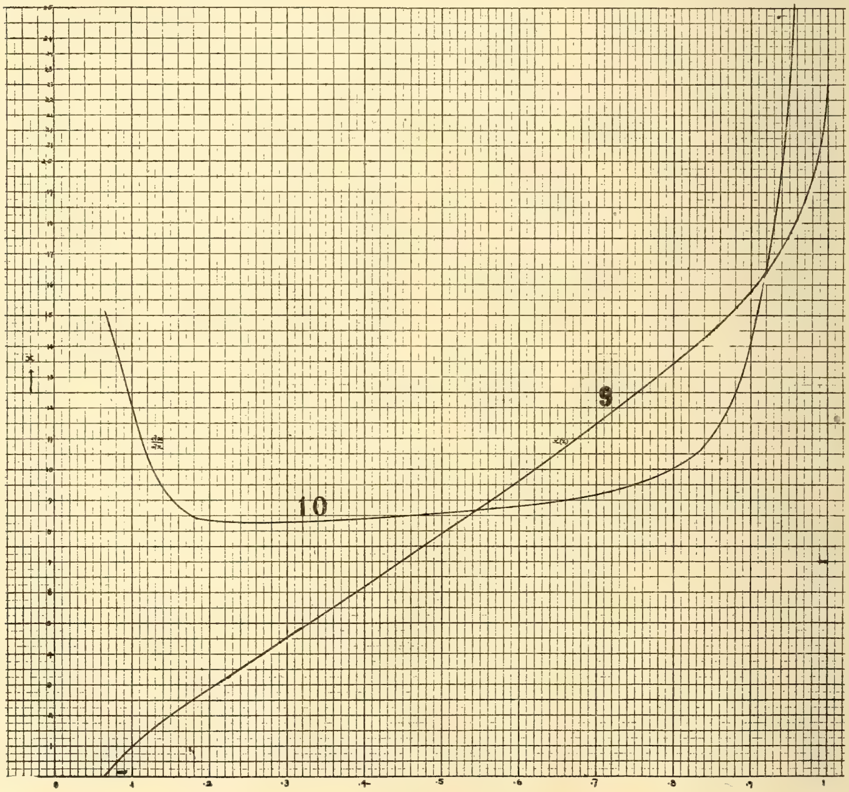
*Communication from the Ballistic Institute, Clark University, No. 10.

It is customary among ballisticians to make many more or less crude hypotheses, and it is even denied that it is possible to apply the strict principles of thermodynamics to the gun as a heat-engine. It shall be our endeavor to show that this is not the case, and to relieve the subject from some of the approximations that have usually been made. The latest and most distinguished writers upon this subject are General Charbonnier and his pupil Sugot, both members of the celebrated Commission de Gâvre (the latter its chief engineer) from which emanated all the knowledge of ballistics that was used during the war, not only for the French army but for ours.



The new methods here described consist, first, in the use of new experimental methods for obtaining the pressure in the gun, the position of the shot, and the muzzle velocity; second, in making the theory depend upon a new differential equation, and third, in the use of the graphic methods of calculation and integration that are common in engineering problems. The experimental methods hitherto have depended on the crusher gauge of Sir Andrew Noble and the Le Boulengé chronograph, both almost unchanged since their invention fifty years ago, and both giving but a single value of the pressure or the velocity. To be sure the Sébert velocimeter gives a continuous record of the position of the shot as a function of the time. It will readily be seen how a continuous record, with an infinity of values of the pressure, as well as a more direct measure of the positions of the shot, will increase our knowledge of the whole process.



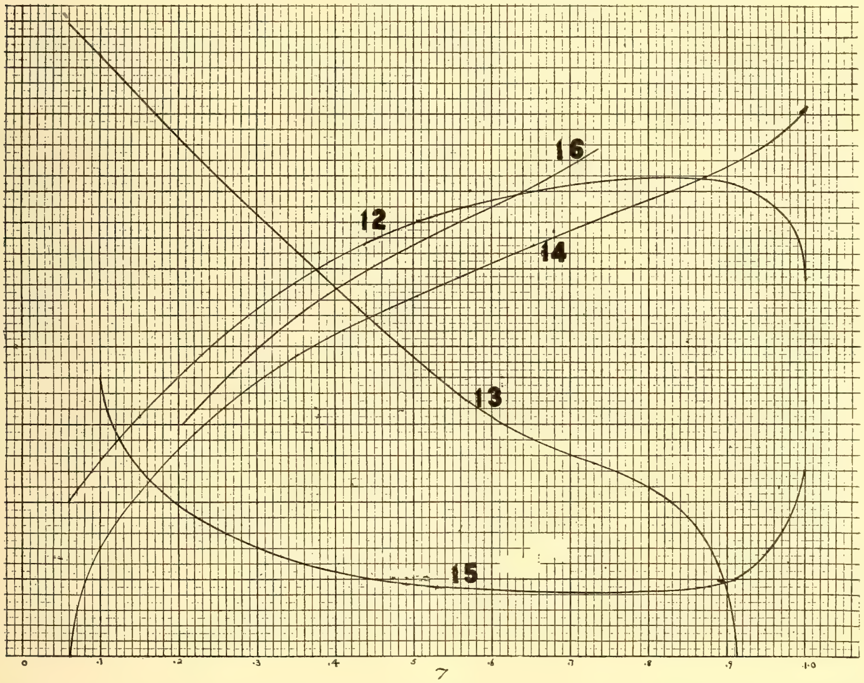
We must first decide upon the equation of state that is to be used for the gases generated by the explosion. It has been shown in a previous paper in these PROCEEDINGS (Vol. 5, pp. 286–288, July 1919) what equations of state are compatible with the constancy of the difference of specific heats at constant pressure and constant volume, and we shall use the equation of Clausius,

$$\left\{p + \frac{a}{T(v + \beta)^2}\right\}(v - \alpha) = RT, \qquad (1)$$

in the simplified form, suitable for the high temperatures concerned (2000°–3000° C)

$$p(v - \eta) = RT \qquad (2)$$

We shall use nearly the notation of Charbonnier and Sugot (*Cours de Balistique*, Imprimerie Nationale), but after having striven desperately with the multitude of engineers' units, pounds per square inch, kilograms per square centimeter or decimeter, inches or centimeters for bore and feet or meters for travel, and unknown units for velocity, acceleration and mass, we shall go back to first principle and express everything in C. G. S. absolute units. It should be an axiom that no formula should contain any (arabic) numeral quantities except natural constants of the universe such as π, and that every formula should be correct in whatever system of units it is interpreted. It is hard to get engineers to agree to this. Consequently our unit of pressure shall be the dyne per square centimeter, with the convenience that a megadyne is between, and nearly equal to, the standard atmosphere and the kilogram per square centimeter at a place usually not specified, that v is the specific volume, in our units in cubic centimeters per gram or liters per kilogram, while T is the temperature on the absolute scale, in our units centigrade degrees. If we consider a load of powder of mass ω contained in a volume V, we define



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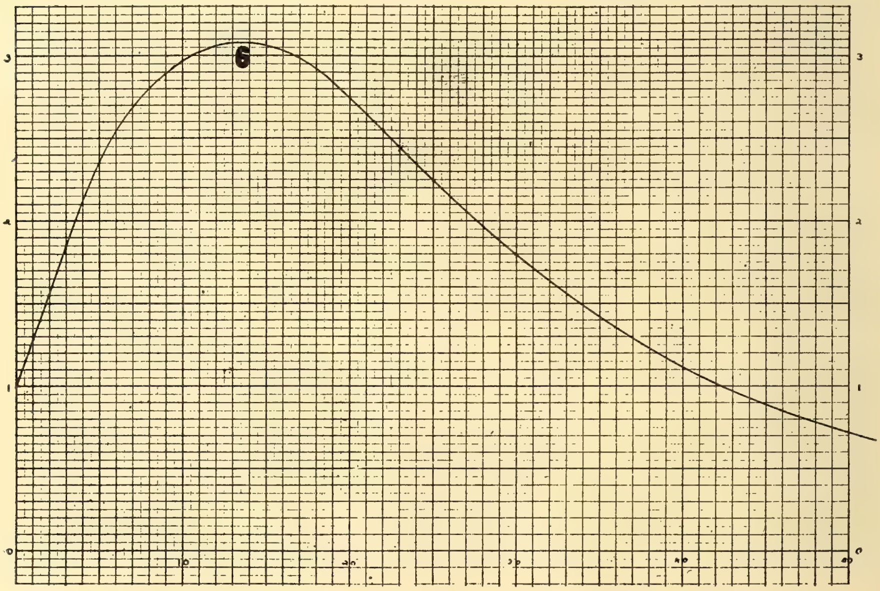
the density of loading as $\Delta = \omega/V = 1/v$, while if the fraction of the load z has been converted into gas we have $v = V/\omega z$. Accordingly

$$p = \frac{RT}{v - \eta} = \frac{RT}{V/z - \eta} = \frac{RTz}{V - z\eta} \tag{3}$$

It is frequently assumed that during the burning the temperature of the gas remains constant, although this can hardly be the case during the expansion in the gun. Although the specific heats of the gases vary largely with the temperature, as shown by the classical experiments of Mallard and Le Chatelier (who found them to be linear functions of the temperature) we shall here consider them as constant, taking mean values. This hypothesis can be corrected later.



If the powder is contained in a variable volume c and ωz has been burned, while $\omega(1 - z)$ remains in solid form, of density δ, the gases will have the space $V = \{c - \omega(1 - z)/\delta\}$ to occupy, so that the specific volume will be $v = \left\{c - \frac{\omega}{\delta}(1 - z)\right\}/\omega z$ and we have

$$p = \frac{RT\omega z}{c - \omega(1 - z)/\delta - \omega z\eta} \tag{4}$$

as the relation between p, z, c, and T. Let S be the area of the bore, c' the volume of the chamber, and x the position or "travel" of the shot, so that

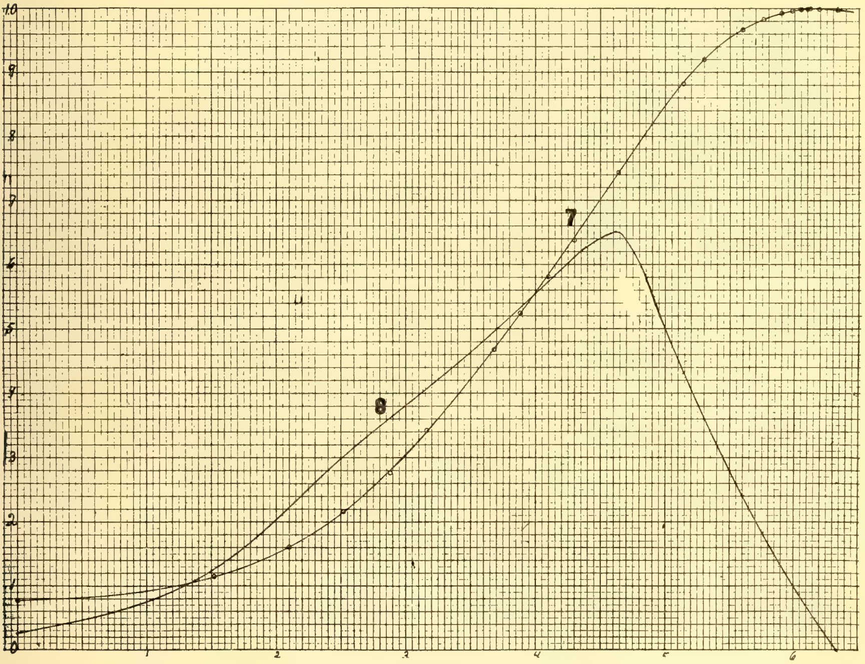
$$c = c' + Sx. \tag{5}$$

Then we have

$$p = \frac{RT\omega z}{c' + Sx - \omega(1-z)/\delta - \omega z\eta}. \qquad (6)$$

In order to write the dynamical equation of motion of the shot we shall assume that the resistance of the rifling (sometimes known as the passive resistance) which is due to the friction of the shot against the lands, and would naturally contain a term proportional to the pressure, is a linear function of the pressure

$$R = R_\circ + bp. \qquad (7)$$



If then m is the mass of the shot, u its velocity, t the time, and if we take account of the inertia of the gas already formed, which is really ωz, by means of a factor λ, we have

$$\frac{dx}{dt} = u, \quad (8) \qquad (m + \lambda\omega)\frac{du}{dt} = S(p - p_\circ) - R_\circ - bp, \quad (9)$$

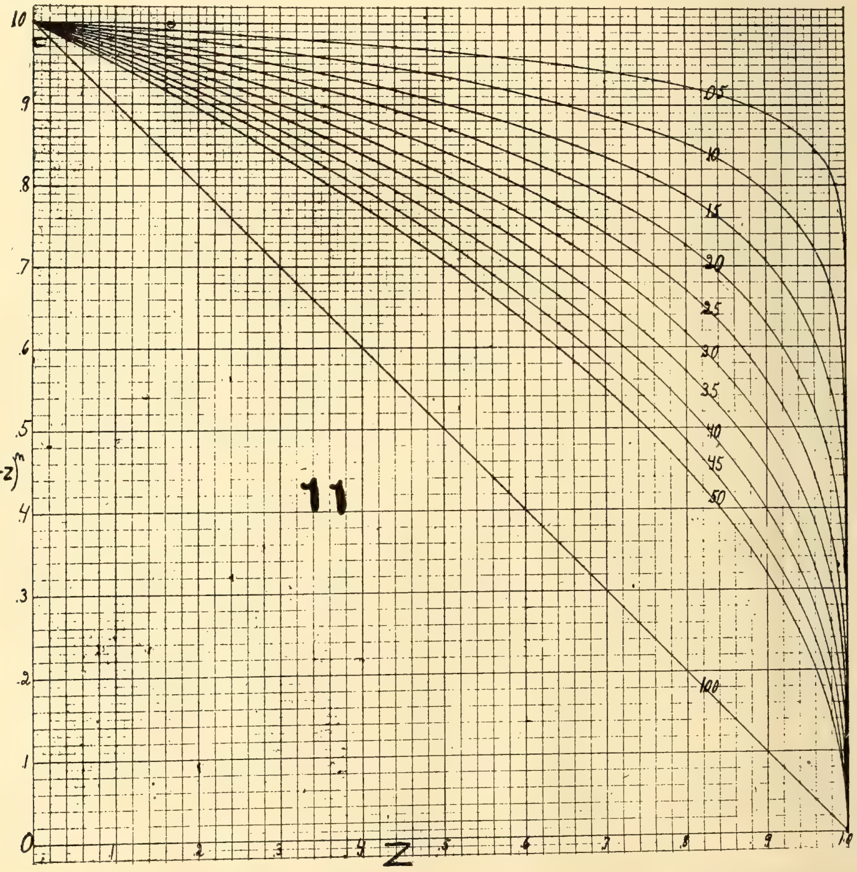
where $p_\circ$ is the atmospheric pressure, which is so small that it may generally be neglected.

By integration of (9) with respect to the time,

$$(m + \lambda\omega)u = (S - b)\int_0^t pdt - (Sp_\circ + R_\circ)t, \qquad (10)$$

$$(m + \lambda\omega)x = (S - b)\int_0^t dt \int_0^t p\,dt - (Sp_\circ + R_\circ)t^2/2. \qquad (11)$$

If p is given in the form of a curve by the indicator, and the two integrals are evaluated for the whole time the shot is in the barrel, the determination of the muzzle velocity and of the whole length of the barrel, everything else being known, will furnish two linear equations for the determination of $R_\circ$ and b. On the other hand if the times at which the shot passes a number of points are known, or if x is experimentally given as a function of t either of the equations (10) or (11) gives a means of verifying the assumption (7) and as many points as may be for determining the constants by least squares.



It will now be necessary to take account of the first law of thermodynamics. If U is the energy of the hot gas per unit of mass, dQ the amount of heat taken in, and Rdx the work done against friction, which is converted into heat, which stays in the gas, we have

$$dQ + Rdx = d(\omega z U) + pdV. \qquad (12)$$

If we now add (9) multiplied by (8) to (12) we have

$$dQ = (m + \lambda\omega)udu + d(\omega zU) + \frac{\omega}{\delta}pdz + Sp_{\circ}dx \qquad (13)$$

and if there are H units of heat evolved for each unit of mass of powder burned, and dq lost to the gun by conduction, we obtain the equation of conservation of energy,

$$H\omega dz - dq = (m + \lambda\omega)udu + d(\omega zU) + \frac{\omega}{\delta}pdz + Sp_{\circ}dx \quad (14)$$

Now if the specific heats are constant, we have

$$U = C_vT = \frac{RT}{\kappa - 1} = \frac{p(v - \eta)}{\kappa - 1} \qquad (15)$$

where κ is the ratio of the specific heats. The quantity of heat $Hdz - pdz/\delta$ may be determined by calorimetric experiments in a bomb at constant volume, as is seen by putting dx and du equal to zero. Putting this equal to $fdz/(\kappa - 1)$ as is customary, neglecting q and $p_{\circ}$, which can be done, and integrating (14) we have

$$\frac{f\omega z}{\kappa - 1} = (m + \lambda\omega)\frac{u^2}{2} + \frac{\omega zRT}{\kappa - 1} \qquad (16)$$

which is Résal's equation, given in 1864. Combining this with (6) we eliminate the unknown temperature, which has never been experimentally determined, and obtain

$$p = \frac{f\omega z - \frac{1}{2}(\kappa - 1)(m + \lambda\omega)u^2.}{c' + Sx - \omega(1 - z)/\delta - \omega\eta z} \qquad (17)$$

From this, if p, x, and u are experimentally determined, we may calculate z for each position of the shot.

If we could neglect u^2, or if it were proportional to z and if x, p, z, were taken as coördinates, this would be the equation of an hyperbolic paraboloid, of which the sections $z = const.$ are called *isopyric* lines by Lieut. Col. Hadcock in his paper on Internal Ballistics (*Proceedings Royal Society*, July 1, 1918 (479–509)). Col. Hadcock states that the expansion is neither adiabatic nor isothermal, but something between the two. This is true enough, as will be shown later, but it can hardly be justifiable to put, with any value of ϵ, Col. Hadcock's equation (5) $p(v - \alpha)^\epsilon/z^\epsilon = K$, for this makes the pressure proportional to z^ϵ, instead of a linear fractional function of z, The assumption that u^2/z is constant is not exactly true, but might be adopted without great error, as we shall see.

If we now put for brevity,

$$\frac{c'}{\omega} - \frac{1}{\delta} = B,\ \frac{S}{\omega} = C,\ \eta - \frac{1}{\delta} = D,\ \frac{S-b}{m+\lambda\omega} = E, \tag{18}$$

$$\frac{R_\circ + Sp_\circ}{S-b} = F,\ \left(\frac{m}{\omega} + \lambda\right)\left(\frac{\kappa - 1}{2}\right) = G$$

where B and C are constants depending on the load, D a constant of the powder, E a constant of the gun and shot, and G a mixed mass and specific heat constant, or $g = f - Gu^2/z$ an approximate constant, we may write

$$p = \frac{fz - Gx^2}{B + Cx - Dz} \tag{19}$$

$$p = \frac{gz}{B + Cx - Dz} \tag{20}$$

We have heretofore said nothing about the rate of burning of the powder. After the powder is all consumed, $z = 1$, and the expansion is adiabatic. We are interested in the period of combustion. It is now customary to assume, after Charbonnier, that the rapidity of burning of the powder is a function of the amount already burned, the function $\varphi(z)$ being known as the form function of combustion, while a factor A is called the vivacity, being, for a given powder, inversely proportional to the linear dimensions of the grain of given form. The rate of burning is also proportional to some increasing function of the presssure, let us say $P(p)$. The simplest functions, except linear functions, are some powers of the variables, not in this case integral powers. If the power is progressive we take $\varphi(z) = z^\beta$, if degressive (as in our powders), $\varphi(z) = (1 - z)^\beta$. We will also put $P = p^\alpha$, where α is positive, as the burning is faster the greater the pressure. We accordingly put

$$\frac{dz}{dt} = A\varphi(z)P(p) = A(1 - z)^\beta p^\alpha. \tag{21}$$

We thus have the three simultaneous equations,

$$dt = \frac{dz}{A\varphi(z)P(p)} = \frac{dx}{u} = \frac{du}{E(p - F)}. \tag{22}$$

It is obvious that the simplest choice for independent variable is z, accordingly we take

$$\frac{dx}{dz} = \frac{u}{A\varphi(z)P(p)}, \tag{23}$$ $$\frac{du}{dz} = \frac{E\,(p - F)}{A\varphi(z)P(p)}, \tag{24}$$

and eliminating u by differentiation,

$$\frac{d^2x}{dz^2} = \frac{1}{A\varphi P}\frac{du}{dz} - \frac{u}{A\varphi^2P^2}\left(P\varphi' + \varphi P'\frac{dp}{dx}\right) = \frac{E(p - F)}{A^2\varphi^2P^2} - \frac{dx}{dz}\left(\frac{P'}{P}\frac{dp}{dz} + \frac{\varphi'}{\varphi}\right). \qquad (25)$$

Now by logarithmic differentiation of (19) we obtain

$$\frac{1}{p}\frac{dp}{dz} = \frac{f - 2Gu\dfrac{du}{dz}}{fz - Gu^2} - \frac{C\dfrac{dx}{dz} - D}{B + Cx - Dz} \qquad (26)$$

or by means of (23), (24)

$$\frac{dp}{dz} = p\left\{\frac{f - 2GE(p - F)\dfrac{dx}{dz}}{fz - GA^2\varphi^2P^2\left(\dfrac{dx}{dz}\right)^2} - \frac{C\dfrac{dx}{dz} - D}{B + Cx - Dz}\right\}, \qquad (27)$$

so that we have finally

$$\frac{d^2x}{dz^2} = \frac{E(p - F)}{A^2\varphi^2P^2} - \frac{dx}{dz}\left[\frac{\varphi'}{\varphi} + p\frac{P'}{P}\left\{\frac{f - 2GE(p - E)\dfrac{dx}{dz}}{fz - GA^2\varphi^2P^2\left(\dfrac{dx}{dz}\right)^2} - \frac{C\dfrac{dx}{dz} - D}{B + Cx - Dz}\right\}\right] \qquad (28)$$

This is a sufficiently complicated equation of the second order and of the third degree in dx/dz, but it is exact, and no simplifying assumptions have been made. If we know φ, P, p being obtained from equation (19), it gives x, the travel of the shot, in terms of z, the fraction of the powder burned. The equation (28) may be integrated graphically. When x is known as a function of z, p may be so determined, and then t and u from equations (22). Thus x, p, u, and z may all be found in terms of t, and the direct problem is solved. The equation (28) will be considerably simplified if we neglect G, which is equivalent to assuming the temperature constant, which is usually done.

We shall however use the differential equation (28) or rather (25) to solve the inverse problem. If x, p, and u are experimentally given, from which z is calculated by (19), everything in (28) is known except P and φ. If we begin with an approximate value of φ, say $(1 - z)^\beta$ and an assumed value of p^α, equation (28) will be a linear equation for α and β, from which by a few trials, the exact values may be obtained.

We now come to the experimental portion of the work. The pressures in the rifle were observed by means of the gauge described in these PROCEEDINGS for July, 1919, the film being afterwards placed in the lantern and an enlarged tracing being made with a pencil on squared paper. From this the calculations were made by my assistant, Dr. E. A. Harrington, who has carried out all the experimental work except the determination of the travel of the shot. In the figures (p. 649), curve 1 represents p,t for the shot used, while 2 is a curve of a reduced charge of 7/10 normal. The curves were then graphically integrated twice by counting the squares. Curves 3 and 4 show the first and second integrals $\int p dt$ and $\int dt \int p dt$, respectively. The travel of the shot was determined by observing the time of contact of the bullet with the end of a coil of fine wire forced down the barrel to a measured point, an oscillograph of high frequency being used, and the time being observed on one of the revolving drums previously described. This was done about a year ago by my then assistant, Mr. H. C. Parker, and the result, shown in curve 5, is the average of a good many shots. The exact agreement of the two curves 4, 5 shows the propriety of the assumption about the resistance, and determines b as $(S-b)/S = 0.894$. In this case $R_o = 0$. We can also neglect p_o so that $F = 0$.

The muzzle velocity and the time of the shot leaving the barrel were determined by the method described in these PROCEEDINGS, April, 1920, developed in this laboratory by Thompson, Hickman, and Riffolt. The constants of the gun were as follows:

Volume of chamber, $c = 4.18$ cm.
Area of bore, $S = .456$ cm.

As for the constants of the powder, the density was determined by Dr. Harrington in the pyknometer as 1.58 gm./cm. No attempt was made to determine η experimentally, that being best done in the bomb, but the value $\eta = 0.95$ was assumed. The load was $\omega = 3.100$ gm. To determine f, the "force" of the powder (which has the dimensions of energy/mass) two methods were used, which agreed very well. If it is assumed that the point of inflexion on the observed pressure curve marks the end of combustion, $z = 1$, f is determined from equation (19). If, on the other hand, equation (19) is used with observed values of p, x, and u, it will give values of z increasing to a maximum, and then diminishing. This maximum should be equal to unity, and after a few trials a value of f can be found that makes it so. As stated, this method agrees with the other. We obtain $f = 1.087 \times 10^{10}$ cm.²/sec.², corresponding very well with the values for the French powders and cordite. (The values given by Charbonnier and Sugot involve gravitation units.) We thus obtain

$$B = 0.716 \text{ cm.}^3/\text{gm.}, \qquad C = 0.147 \text{ cm.}^2/\text{gm.},$$
$$D = 0.32 \text{ cm.}^3/\text{gm.}, \qquad E = 0.394 \text{ cm.}^2/\text{gm.}$$

If we take $\kappa = 1.2$, and $\lambda = 0.2$, we have $G = 0.333$. Also we find that g (eq. 20) varies by about 16%, nearly as a linear function of x or z.

In order to show the functional relation (19) between the five quantities p, x, u, z, G, an "*abaque à glissement*" was constructed, so that by placing a transparent index on four of the variables the fifth could be found. This will be described elsewhere, but inasmuch as it was found to take longer to set the index than to set up the numbers on the arithmometer, of which we have several models, it was not used. After z was calculated, curves were drawn: $6(p,x)$, the indicator diagram (p. 652) for the gun; $7(z,t)$, $8(dz/dt,t)$, $9(x,z)$, $10(dx/dz,z)$, $11(1-z)^n$ for various values of n; $12(p,z)$, $13(dp/dz,z)$ (with a different zero of ordinates); $14(u,z)$; $15(du/dz,z)$; $16(dz/dt,z)$.

For graphical differentiation, a simple contrivance was used consisting of two metal rulers jointed together at one end. Near the other end of one was placed a plane mirror normal to the edge of the rule. By looking at the curve in this, and making its reflection join it without a corner, the direction of tangency could be found more accurately than by making the ruler tangent. The other ruler is then set parallel to the horizontal lines of the paper, and for a horizontal distance of ten or twenty centimeters the vertical distance between the rules read off, giving the trigonometrical tangent. Thus in a few minutes we may lay down the derivative curve with nearly three figure accuracy.

I have the following criticisms to make on the curves. After they had been given to the photographer it was discovered that the left-hand portions of (9) and (10) are incorrect, since dx/dz must vanish when x and u are zero. This is probably due to an error in choosing this point, which should be at the inflexion of the p curve, which should have been determined with the mirror directly on the film, instead of on the enlargement. When this is done, the pressure to start the shot is found to be the same as on curve 2, which (like 1) is so good that films of two different shots may be laid one over the other without being distinguishable. Making this correction will slightly displace curves (3) and (4). I realize that I have given hardly enough here to verify the theory, but as this paper is too long already, and as I am very shorthanded for assistance, I consider it best to publish it for the method, and to leave the verification, which should be by the construction of the observed curves from calculation, for another paper. By the courtesy of Rear-Admiral Ralph Earle, we have a Naval one-pounder gun, to which we hope to apply the methods, on which we hope to improve by experience. For a larger gun, where the wires might be blown out of the gun by the blast, we have another method of finding the velocity in the bore.

It should have been stated in our former paper, and is now stated here, that these experiments have been made possible by means of a generous grant from the Rumford Fund of the American Academy of Arts and Sciences, and to my colleagues on the Rumford Committee I hereby express my hearty thanks.

THE MUTANT CROSSVEINLESS IN DROSOPHILA MELANOGASTER*

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Communicated by T. H. Morgan, September 30, 1920

The Origin of Crossveinless.—A recessive sex-linked mutant called small-eye appeared, the locus of which was found to be very close to that of fused (locus of *fu*, 59.5). To determine definitely the relation of the two loci, a forked (locus of *f*, 56.5) small-eye (symbol, *sy*) female was crossed to a fused male, and five of the F_1 wild-type females were mated singly to forked small-eye brothers. In F_2, no crossovers between small-eye and fused were obtained. While this result confirms the closeness of the loci, it was decided to raise more F_2 cultures until one or more cross-overs appeared which would show the position of the new locus with reference to the known loci forked and fused. Accordingly, from one of the five F_2 cultures (No. 10,846) four wild-type females, of the same constitution as the original F_1 female, were mated to forked small-eye brothers. There appeared two cross-overs (wild-type) that showed that the locus of small-eye is to the left of that of fused (and about 0.2 unit distant, i.e., at 59.3).

In one of these four cultures it was noticed (No. 10,988, Dec. 21, 1919) that about half the males did not have either anterior or posterior cross-veins. All females had normal crossveins. Evidently crossveinless (*cv*) is a sex-linked recessive. The other three sister cultures were then examined, and no crossveinless flies were present.

The males that hatched after the mutant character was noticed were: *cv f sy* 12, *fu* 11, *cv fu* 3, *f sy* 9. Since approximately half the males showed the crossveinless character, the mother is known to have been heterozygous for the mutant gene. The crossveinless character emerged in association with forked more often than with fused, which means that the mutant gene was in the forked small-eye chromosome of the mother. She had received this chromosome from her father—the F_1 forked small-eye male used in raising culture 10,846. But three of her sisters likewise received forked small-eye sperm from this same male, and, as we have seen, none of these three sisters received the crossveinless gene. It follows that the mutation occurred relatively late in the ontogeny of the testis of the male in question, perhaps as late as the maturation stage.

The Locus of Crossveinless.—Of the thirty-five males in which the linkage relations of crossveinless to forked, small-eye, and fused were noted, 12, or 34.3%, were crossovers between crossveinless and forked. In view of the small numbers, this value indicated only that the locus of crossveinless is to the left of that of forked and probably at least as distant as vermilion (*v* locus, 33.0). The next step was to mate a crossveinless forked small-eye male to a vermilion female and to raise several F_2 cultures (table 1).

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* Contribution from the Carnegie Institution of Washington.

TABLE 1

P_1, CROSSVEINLESS FORKED SMALL-EYE ♂ × VERMILION ♀; F_1 WILD-TYPE ♀, $\frac{cv \quad f \quad sy}{v}$ × F_1 VERMILION ♂ (TABLE INCLUDES F_2 ♂ ♂ ONLY)

JAN. 20, 1920	0		1		2		3		1,2		1,3	
	cv f sy	*v*	*cv v*	*f sy*	*cv*	*v f sy*	*cv f*	*v sy*	*cv v f sy*	+	*cv v sy*	*f*
11223	64	60	19	15	22	17	3	1	1	1	–	–
11225	41	63	16	19	18	10	1	–	1	2	–	–
11226	42	52	16	14	25	14	2	2	1	2	1	–
11227	63	62	18	10	19	16	1	–	–	2	1	–
11268	102	84	29	24	39	31	1	1	4	4	–	–
11275	89	110	31	27	27	33	1	3	2	3	–	–
11276	10	22	3	4	5	8	–	1	–	2	–	1
Total.....	411	453	132	113	155	129	9	8	9	16	2	1

The locus of crossveinless, on the basis of the F_2 males, proved to be about 19.0 units to the left of vermilion, that is, about 14 units from the zero end of the chromosome. This location is regarded as especially valuable since the interval from ruby (7.5) to cut (20.0) did not contain any serviceable mutant character. The rather poor mutants club (16.7), lemon (17.5), shifted (17.8), and depressed (18.0) had all been lost or discarded. It was important to have a mutant of first rank in this region in order to investigate the linkage behavior of lethal-2, which is probably a "deficiency." A multiple stock, ruby cut tan (*t*, 27.5) vermilion had been made up for this analysis, and it was decided to incorporate the crossveinless character in the multiple. In the process of incorporating crossveinless, accurate knowledge of the location of crossveinless was obtained from the F_2 results of the cross between crossveinless male and females of the original multiple (table 2).

There was 8.5% of crossing over between ruby and crossveinless, and 8.3% between crossveinless and cut. The locus of crossveinless is thus almost exactly in the middle of the gap between ruby and cut. The total amount of crossing over between ruby and cut was 16.8 which is slightly higher than the expected value 12.5.

This position of crossveinless makes it possible to study linkage relations in any part of, or throughout the *X*-chromosome, by means of mutants so close together than no unobserved double crossing over occurs. Furthermore, these mutants are perfectly classifiable in combination, and, with the exception of cleft at the extreme right end of the chromo-

TABLE 2

P_1, CROSSVEINLESS ♂ × RUBY CUT TAN VERMILION ♀; F_1 WILD-TYPE ♀, $\frac{rb \quad ct\ t\ v}{cv}$, × F_1 *rb ct t v* ♂ (TABLE INCLUDES F_2 ♂ ♂ ONLY)

FEB. 14, 1920	0		1		2		3		4		1,3		1,4		2,3	2,4
	rb ct t v	*cv*	*rb cv*	*ct t v*	*rb*	*cv ct t v*	*rb ct*	*cv t v*	*rb ct t*	*cv v*	*rb cv t v*	*ct*	*rb cv v*	*ct t*	*rb t v*	*rb v*
11328	63	87	6	15	17	14	10	7	7	7	–	1	–	–	–	–
11329	62	74	12	6	8	7	7	5	8	5	1	2	3	1	1	1
11330	75	77	6	5	7	4	10	9	6	6	–	–	–	–	–	1
11331	86	74	15	4	8	5	9	9	7	7	–	–	–	–	–	–
11332	72	87	5	8	6	12	10	6	2	17	1	–	–	–	–	–
11333	75	77	10	8	10	7	10	6	10	6	1	–	–	–	–	–
11400	47	44	4	8	4	4	5	2	5	5	–	–	–	–	–	–
11418	59	70	10	5	9	8	5	8	3	2	–	–	1	–	1	–
Total.....	539	590	68	59	69	61	66	52	48	55	3	3	4	1	2	2

some, are of such excellent viability that they can be used simultaneously with only negligible differential mortality. Crossveinless is one of the best of these characters, since the somatic effect is so slight that interference with the classification of other mutants is at a minimum. It is a matter of observation that mutants like crossveinless with slight somatic effects are generally those with least disturbance to viability. A striking example of this correlation is to be found in the cut mutations. On at least eleven occasions sex-linked mutations that were similar to the original cut have appeared. Of these, cut^3 was both the poorest in viability and the most extreme in extent and number of somatic changes, while cut^6 was the best in viability and least extreme in its single observed somatic change. Two of the other allelomorphs, were intermediates in

TABLE 3

P_1, CROSSVEINLESS ♀ × CUT^6 ♂; F_1 WILD-TYPE ♀, $\frac{cv}{ct^6}$, × F_1 *cv* ♂ (F_2 ♂ ♂ ONLY)

APR. 14, '20	*cv*	ct^6	$cv\ ct^6$	+	CONT.	*cv*	ct^6	$cv\ ct^6$	+
11700	88	95	9	8	11733	228	235	15	14
11701	81	75	7	7	11734	143	164	11	12
11702	108	106	7	10	11735	89	88	6	7
11703	22	26	2	2	11736	135	156	7	12
11704	91	88	4	5					
					Total.....	985	1033	68	77

both these regards, while differing from each other. Cut^6 is now the only cut mutant used for linkage determinations involving this locus.

Additional linkage data upon the crossveinless cut distance was obtained by raising nine F_2 cultures from the cross of crossveinless to cut^6. The crossover value was found to be 6.7 on the basis of the 2,163 flies (table 3).

The locus of crossveinless is at 13.7, as calculated on the basis of the data of the three preceeding tables, and on the assumptions that the locus of ruby is at 7.5 of cut at 20.0, and of vermilion at 33.0.

Description of the Crossveinless Character; Homology with Crossveinless in D. virilis.—The somatic changes produced by the crossveinless gene seem to be restricted to the entire absence of the posterior crossvein and the almost complete absence of the anterior crossvein. There is a slight trace only of the anterior crossvein, though on casual inspection it seems to extend outward from the III-longitudinal vein about half way to the IVth. However, what is seen is largely a sense-organ that is normally present near the mid-point of the anterior crossvein and that is not affected by the crossveinless mutation. Examination of crossveinless in D. virilis showed that the sense organ is unaffected there also, but that the crossvein is not reduced in length or thickness as much as in D. melanogaster. (See figures of the crossveinless mutant in Weinstein's paper preceding this.)

The similarity of the characters is parallelled by a similarity of position on the maps of the two X-chromosomes. It seems highly probable that the two mutants are homologous, though, as Weinstein's discussion brings out, this cannot yet be accepted without reservations.

IS CROSSING OVER A FUNCTION OF DISTANCE?[1]

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Communicated by C. B. Davenport, September 24, 1920

There is a well intrenched concept of recent genetics that hereditary factors or genes may be given fairly definite loci on chromosome maps and that these maps correspond to or represent, roughly perhaps, the actual conditions in the chromosome. The basis for this attractive and suggestive view is the premise that the distance between two genes is necessarily proportional to the percentage of crossing over which these two genes show—other things being equal. If the distance which gives one per cent of crossovers is used as an arbitrary unit of measurement, then it follows that distances on the chromosome may be calculated in terms of this unit. It has seemed to me for some time that the antecedent in this hypothetical proposition contains a more or less gratuitous assumption.

We do not know that the distance which gives 1% (or *n*%) of crossovers is a fixed unit. Stated differently, we do not know how constant the percentage of crossing over may be between two genes to which we give a fixed distance, i. e., our arbitrary unit of measurement may itself prove to be a variable. It may be possible for the distance which gives 1% of crossing over to differ in different females of the same population, or differ between stocks. In order to throw some light on these questions I began a set of experiemnts in 1916 in collaboration with my colleague, Dr. E. Roberts, and several students. Although a number of these experiments are still in progress, data involving the classification of over 400,000 individuals have been accumulated and some conclusions seem warranted. A more detailed account of these experiments will appear in the current numbers of the *Journal of Experimental Zoölogy.*

In observing a large number of females (Drosophila melanogaster) of the generalized zygotic formula $\frac{a\ \ b}{A\ \ B}$, it is common to find great differences between individual females with respect to the amount of crossing over. Some of the variability may be due to sheer fluctuations of sampling, to age, and to environmental conditions, but sometimes the deviations are so wide as to arouse a suspicion that hitherto unknown causes may be effective. If this variability is due at least in part to genetic modifiers then selection should be effective, particularly if environmental fluctuations do not mask or obliterate the effects of genetic modifiers. It was with this thought in mind that I began to select for high and low crossover value. Four selection experiments were undertaken. Series A, A' and B were low selection experiments and Series C was a high selection experiment. Each series began with a single white-eyed, miniature-winged female mated to a wild, red, long male. The F_1 females were red, long double heterogzyotes $\frac{w\ \ m}{W\ \ M}$ and the F_1 males were white, miniature double recessives $\underline{w\ m}$. These were mated in pairs, giving the parental classes (red long and white miniature) and the crossovers (red miniature and white long) with the usual ratio of approximately 33% crossovers—the same value used in plotting chromosome maps. The same mating was made in successive generations, always selecting as far as was possible the widest deviates to perpetuate the line of selection. At the same time, the closest possible inbreeding was maintained, the details of which are given in our longer papers. The results indicate that selection was effective in all series.

Series A was reduced to 0% in F_{10} and remained at about 0% for two more generations. Series A', derived from Series A as a side line of selection in the F_7, began with a female showing 1 : 91 = 1.10%.[2] This line was carried for 9 generations (F_7–F_{15}) and also bred true to about 0%. The grand total for this *entire series* gave 33 : 5156 = 0.64%—actually less than 1 %. There can be no doubt but that an original crossover value of about 33% has been changed by selection, at least a marked change has followed selection.

Series *B* was entirely independent in its origin from Series *A* and *A'*; and low selection was also effective, as in the case of the preceding series. Curiously enough we have not been able as yet to reduce the crossover value to zero, or approximately zero, as in the other series. Since experiments with this series form the basis of the present paper, table 1 is given to show in condensed form the progress during selection. After the F_{28}, selection was discontinued, and the stock has bred practically true to about 5 or 6% crossing over for 22 generations (i. e., through F_{50}). Table 1 gives the percentage of crossovers for every fifth generation, and also for the total offspring in each block of five successive generations.

TABLE 1

CROSSOVER VALUES FOR EVERY FIFTH GENERATION, AND FOR THE TOTALS IN EACH BLOCK OF FIVE SUCCESSIVE GENERATIONS, IN SERIES *B* (LOW SELECTION)

GENERATIONS	CROSSOVER VALUES	GENERATIONS INCLUDED	CROSSOVERS	TOTALS	CROSSOVER VALUES
1	28.60	1– 5	10517	40567	25.93
5	24.55	6–10	10344	47295	21.87
10	16.99	11–15	3687	25333	14.55
15	11.17	16–20	4869	48277	10.09
20	9.81	21–25	3386	36693	9.23
25	7.15	26–30	576	8007	7.19
30	5.62	31–35	121	2089	6.79
35	4.18	36–40	267	4571	5.84
40	6.70	41–45	750	12453	6.02
45	6.51	46–50	350	5203	6.73
50	6.98				

Series *C*, high selection, was carried for 8 generations, but we were unable to make progress in selecting upward. On the contrary, we were greatly astonished to find in the F_7, 9 out of 72 pairs, which gave almost no crossing over at all, although they produced a large number of offspring. The remaining 63 pairs gave about the usual crossover value. The totals for these 9 paired matings were:

26 crossovers : 1055 total = 2.46% crossing over.

These same genes should have given 33% crossing over if they agreed with the usual values used in plotting chromosome maps. The natural inference is that any attempt to increase crossing over leads to double crossing over and thus to very low crossover values (practically zero). The explanation implies that these 9 females showed a marked *decrease* in crossover values, despite high selection, because they gave almost nothing but double crossovers. Series *C* was dropped, but we hope to repeat the experiment and test out the region between white and miniature in such females.

The effects of selection on crossover values may be due to one or a number of causes, some of which suggest themselves almost immediately. The most promising and probable explanation seemed to me to be that crossing over is either due to or markedly modified by multiple factors. In order to test out this view, I crossed the low crossover stock of Series *B*, which shows 5–6% crossing over, to ordinary stock which shows 33% crossing over. Table 2 gives the results of this experiment. The first line of the table gives for comparison the frequency distribution of crossover values in an ordinary population. There were 90 females in this sample but I have eliminated two very wide deviates from the distribution, because the number of offspring on which their crossover values were based was extremely small. One of them showed 1 : 8 = 12.5%; and the other showed 6 : 8 = 75%. The population as a whole showed 30.68% crossing over and the mean female had a value of 30.55%. The average number of offspring per female and the totals show that the values for the females both individually and collectively are as reliable as can be reasonably expected. The second line of the table shows the first generation in Series *B*, which resembles closely the sample just described. After 28 generations, selection was discontinued. Thereafter the generations were perpetuated by en masse matings. In the F_{42}, I mated 50 red, long females heterozygous in white miniature $\frac{w\ m}{W\ M}$ to stock white miniature males $\underline{w\ m}$. All except one were fertile and the distribution of their crossover values is given as the low P_1 parent in table 2. Mating them to ordinary stock males would not change the crossover values which such low females show. We cannot know positively what the crossover value of each white miniature male parent was, but we have no reason to suppose it differs greatly from the values given for the first generation of Series *B* or the ordinary stock, both shown in table 2. We have used this same white miniature stock in class work and have always found it to give the regular "map value" of about 33%.

It was virtually impossible to breed all of the F_1 hybrid females from each pair separately. I decided to breed exhaustively 50 red long F_1 females $\frac{w\ m}{W\ M}$ to their F_1 white miniature brothers $\underline{w\ m}$ to obtain a sample frequency distribution of F_1 females coming from a single P_1 pair (pair No. 18). Forty-seven of these 50 females were fertile and gave an average of 465 offspring per female.[3] The range of F_1 crossover values shows quite clearly that they lie between the low and the high parents. The value of the mean female and the crossover value of this total F_1 population show the same thing. One F_1 female showed a ratio of 1 : 36 = 2.77%, but since the ratio is based upon such a small total we need not lay much stress on this wide variate.

TABLE 2

THE DISTRIBUTION OF CROSSOVER VALUES IN NORMAL POPULATION, IN LOW CROSSOVER STOCK, AND IN HYBRIDS BETWEEN THESE

GENERATIONS	NUMBER OF VARIATES	THE DISTRIBUTION OF CROSSOVER VALUES														MEAN VARIATE	σ	CROSS-OVERS	TOTAL	% OF CROSS-OVERS	AVERAGE NUMBER OF PROGENY PER VARIATE
		1.5	4.5	7.5	10.5	13.5	16.5	19.5	22.5	25.5	28.5	31.5	34.5	37.5	40.5						
Sample population	88								7	11	19	28	15	5	3	30.55	4.28	6465	21071	30.68	239.4
1st generation, Series *B*	34							3	5	3	10	6	3	2	2	28.85	5.58	2056	7189	28.60	211.4
P_1 low patent	49	7	26	15	1											5.11	2.10	425	7948	5.35	162.2
F_1 hybrid females	47	1		2	10	16	12	6								13.88	3.64	3216	21853	14.72	465.0
F_1 samples of each P_1	45			10	18	10	6	1								11.50	3.10	5213	39416	13.23	875.9
F_2 from P_1 No. 2	33	2	1	5	8	8	5	3	1							12.24	4.90	1060	7812	13.57	236.7
F_2 from P_1 No. 5	39				5	13	12	5	1	2			1			16.19	4.70	1477	9001	16.41	230.8
F_2 from P_1 No. 6	76		1	3	14	23	22	6	3	3	1					14.96	4.39	3161	21701	14.57	285.5
F_2 total	148	2	2	8	27	44	39	14	5	5	1		1			14.66	4.81	5698	38514	14.79	260.2

In addition to the F_1 distribution coming from a single P_1 pair, I also obtained a sample crossover value of the F_1 coming from each of 44 other P_1 females. I chose at random four F_1 red, long double heterozygotes from each P_1 pair. Each group of four F_1 females was mated to white miniature F_1 brothers. Thus we secured crossover values for 45 different F_1 groups, each group coming from a single P_1 pair. These F_1 crossover values are put in the form of a frequency distribution in the fifth row of table 2. Here again we find the F_1 values intermediate between the low stock and the original population. In no case was an F_1 value as low as the mean or mode of the low parent nor as high as the mean or mode of the original population.

Three distinct and separate F_2 distributions were reared, coming from P_1 pairs No. 2, 5 and 6. The value of each original P_1 female, together with the crossover ratio of its F_1 and F_2 progeny, is given in table 3.

TABLE 3

THE DATA ON P_1 PAIRS NO. 2, 5, AND 6, FROM WHICH THE F_2 DISTRIBUTIONS WERE OBTAINED

P_1 PAIR NO.	CROSSOVER VALUE	F_1 CROSSOVER VALUE	F_2 CROSSOVER VALUE
2	21:382 = 5.50	29: 464 = 6.25	1060: 7812 = 13.57
5	7:193 = 3.63	72: 530 = 13.59	1477: 9001 = 16.41
6	6:178 = 3.37	251:1730 = 14.51	3161:21701 = 14.57

The F_2 distributions of table 2 show a wider range than the low parent or the F_1. In fact, the total F_2 population with 148 pairs gives some females as low as the low parent and some as high as the original population. The mode is between the two stocks. It is clear that the results of these hybridization experiments bear the distinctive features of multiple factor inheritance with incomplete dominance; for the F_1 is intermediate and the F_2 is likewise intermediate in its average but the F_2 shows a conspicuous increase in range which easily overlaps both original P_1 distributions. The increase in the standard deviation of each F_2 population and of the total F_2 distribution over that of the F_1 or P_1 puts these facts in concrete terms. Therefore, we can hardly escape the conclusion that multiple factors have a striking influence upon crossover values. In the frequency distributions of table 2, some variates will necessarily have little meaning because their crossover values are based upon small totals. I have thought it desirable to include every variate and thus withhold no data rather than include only such females as produced more than a fixed minimum of offspring. However, in order to show that the extremes in the F_2 population are segregates, rather than fluctuations of sampling, I have given in table 4, detailed data on the highest and lowest 12 variates in the total F_2 frequency distribution of table 2. The lowest 12 variates have values from 0%–9% and cover about the same range as the low parent; while the highest 12 have values above 21% and cover about the same range as the high parent. The values for both low and high F_2 variates are based upon totals which are just as satisfactory as any in the population, where the average number of progeny per female was 260. The total F_1 included 61,000 offspring and the total F_2 distribution is based upon 38,500 offspring.

TABLE 4

THE HIGH AND LOW VARIATES OF THE TOTAL F_2 DISTRIBUTION IN TABLE 2

LOW VARIATES				HIGH VARIATES			
RECORD NUMBER	CROSS-OVERS	: TOTAL =	CROSSOVER VALUE	RECORD NUMBER	CROSS-OVERS	: TOTAL =	CROSSOVER VALUE
2- 2	1	35	2.86	2–32	66	287	23.00
2- 6	18	216	8.33	5–21	52	154	33.77
2–19	14	223	6.28	5–22	45	178	25.28
2–22	0	19	0.00	5–24	69	325	21.23
2–25	8	168	4.76	5–28	112	466	24.03
2–28	14	174	8.05	6–10	107	416	25.72
2–33	22	329	6.69	6–21	56	241	23.24
2–34	25	280	8.93	6–23	71	287	24.74
6- 8	11	281	3.91	6–24	61	251	24.30
6- 1*a*	32	394	8.12	6–11*a*	33	145	22.76
6- 2*a*	23	275	8.36	6–13*a*	26	89	29.21
6–35*a*	20	272	7.35	6–16*a*	62	295	21.02
Total......	188	2666	7.05	Total.......	760	3134	24.25

In obtaining a crossover value for any two genes like white and miniature we find much variability among the females which serve to make up the general population from which our map value is derived. This variability is due to numerous modifying factors. Selection has evidently sifted out certain relatively pure combinations of these modifiers, hence the low variability of our low crossover stock. The hybridization experiments indicate that the amount of crossing over is at least markedly influenced if indeed it is not actually determined by multiple factors. There are several ways in which multiple factors might possibly change the crossover value which two genes show. In modifying the crossover value of white and miniature from 33% to 6% or to 0% we might suppose that we had either moved the locus of genes or that we had eliminated the usual single chromosomal twist between these two genes. Since the allelomorphic relationships between red and white and between long and miniature have not been disturbed when we mate low crossover stock to the original population, the latter alternative explanation seems preferable. We can evidently change by selection the amount of twisting which members of an homologous pair of chromosomes show. Now, if the difference between practically no crossing over (Series A and A') or between 6% crossing over (Series B) and normal crossing over (33%) is due to multiple factors, we then naturally wonder just what part "distance between two genes" on a chromosome map may play in determining linkage values. Our current view is that "the percentage of cases in which two linked genes separate (amount of crossing over between them) is necessarily proportional to the distance between these genes,—other things being equal," i. e., under ordinary circumstances and in the absence of unusual factors or environmental conditions which geneticists recognize. But evidently under ordinary circumstances, the percentage of crossing over is a variable which is determined by the different possible combinations of multiple modifying factors; hence the percentage of crossing over cannot be proportional to the distance if the distance remain constant. For example in Series *B* we find 6% crossing over, and so we should conclude that the distance is less than one-fifth of what it originally was before we began selection. To maintain our original position, we must conclude that the percentage of crossing over and the distance are correlated variables, if the proportion is to remain reasonably constant. The dilemma will hardly aid us in determining what had happened to almost all of the distance and the genes between 0 and 33 in Series *A* and *A′*, where crossing over was practically eliminated. In view of these considerations it would perhaps be simpler to conclude that linkage is not a function of distance, i. e., crossing over is not necessarily proportional to distance. The distance between two genes may remain fairly constant, but the amount of crossing over depends upon numerous hereditary factors.

[1] Paper No. 14 from the Laboratory of Genetics, Illinois Agricultural Experiment Station.

[2] In giving crossover values, I shall put the data in the following form throughout this paper—crossovers: total = per cent of crossing over. Since the classes are always the same, repetition can be avoided.

[3] I am indebted to Dr. E. Roberts and Mr. P. M. Woodworth for carrying this generation in part.

INTERSPECIFIC HYBRIDS IN CREPIS

I. *Crepis capillaris* (L) Wallr. × *C. tectorum* L.

By E. B. Babcock and J. L. Collins

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Communicated by J. C. Merriam, September 22, 1920

Interspecific hybrids, particularly those having different specific chromosome numbers, have in the past figured conspicuously in establishing the now generally accepted principle of the individuality and continuity of the chromosomes. It is probable, therefore, that a study of the hereditary characters of species hybrids may give further information concerning the chromosomes as the carriers of the determiners of hereditary characters and add to our knowledge regarding the manner of the origin and evolution of specific chromosome numbers.

From a study of interspecific crosses between *Nicotiana sylvestris* and several varieties of *Nicotiana Tabacum,* Clausen and Goodspeed[1] have found that the Mendelian laws apply but apparently the chromosomes of the hybrid will function normally only when there is a union of a majority of the chromosomes of one species. When the chromosomes of one gamete are mostly of a different species from those of the other uniting gamete the zygote either does not live or it develops abnormally. This seems to indicate that although they are numerically the same, the chromosomes of the two species differ so much qualitatively that they cannot properly function together.

Eggs of the sea urchin have been fertilized artificially with the sperm of another species in which the chromosomes were different in number. In practically all such cases the zygote would start but fail to complete development. It appears that the chromosome complex sets up an incompatible combination of genes such that the embryo cannot pass a certain stage of differentiation.

The writers have found a somewhat similar case in plants in the hybrid between *Crepis capillaris* and *C. tectorum.* This genus belongs to that tribe of the Compositae having the heads composed entirely of ligulate flowers. *C. tectorum* is an annual and *C. capillaris* behaves either as an annual or as a biennial. The former has four pairs of chromosomes while the latter, *C. capillaris,* has but three pairs. Cytological studies have shown the hybrid to have seven chromosomes, the sum of the haploid numbers of the two species.

The differences between the two species are quite marked. The achenes of *tectorum* are about 4 mm. long, are dark brown in color and retain the pappus quite well. The cotyledons are narrowly linear with obtuse apex, the shape resembling that of a sunflower seed or the cotyledons of a very young cucumber seedling. The first plumule leaf appears very promptly soon after the cotyledons have expanded and reached their maximum size. The plants often reach maturity in ninety days. The achenes of *capillaris* are about 2.5 mm. long, are tan or straw color, and shed the pappus rather easily. The cotyledons vary from broadly ovate to the condition where the breadth is slightly greater than the length. As in *tectorum* the first plumule leaf appears very promptly after the cotyledons have expanded to their normal size.

In the hybrid, regardless of the way the cross is made, we find that the *tectorum* type of cotyledon is dominant but the seedlings are larger than the normal *tectorum* seedlings and may in that respect be said to show hybrid vigor. That the shape and size of the hybrid cotyledons growing on a *tectorum* mother plant would be comparable to the cotyledons from a pure *tectorum* achene of the same size would not be surprising but that the cotyledons coming from the much smaller *capillaris* achenes should be so much larger than those usually coming from ordinary *capillaris* achenes seems unusual and gives immediate evidence of the hybrid nature of the seedling.

The hybrid seedlings grew very vigorously through the cotyledon stage, then growth and development ceased although different plants remained alive from 30 to 81 days in this arrested condition. All hybrids died before passing beyond this stage of development. The attempt to grow the hybrid between these two species was made three separate times always with the same behavior and the same results.

TABULATION OF RESULTS OF RECIPROCAL CROSSES

CULTURE NO.	PARENTS	COTYLEDON CHARACTERS OF HYBRID	BEHAVIOR OF HYBRID	NUMBER OF SEEDLINGS
Z 3	*capillaris* × *tectorum*	6 *tectorum*, 5 *capillaris*, 1 intermediate	all failed to pass cotyledon stage	12
Z 5	*capillaris* × *tectorum*	all *tectorum;* showed hybrid vigor	all failed to pass cotyledon stage	6
Z 7	*capillaris* × *tectorum*	all *tectorum;* showed hybrid vigor	all failed to pass cotyledon stage	3
Z 8	*capillaris* × *tectorum*	intermediate	failed to pass cotyledon stage	1
Z 10	*tectorum* × *capillaris*	all *tectorum;* showed hybrid vigor	all failed to pass cotyledon stage	12
Z 12	*capillaris* × *tectorum*	small and distorted, abnormal	failed to pass cotyledon stage	1
Z 13	*capillaris* × *tectorum*	small and distorted, abnormal	all failed to pass cotyledon stage	5

Histological examination of a hybrid seedling revealed a most unusual teratological condition of the tissue systems and of the cells which, no doubt, was the immediate cause of the unusual behavior noted. These tissue systems were in a chaotic condition where order and continuity should be expected. Patches of embryonic tissue were scattered irregularly among the vegetative cells, patches of tracheary cells were likewise found here and there in the meristematic tissue of the seedling. In fact, tracheary cells were in one section found to be extending at right angles to the long axis of the seedling. Groups of vegetative cells were separated by streaks of disorganized and disintegrating tissue. Some of the vegetative cells were abnormally large.

In this interspecific hybrid it appears as if the force directing cell differentiation were lacking or non-functioning. We know that in all promeristematic cells there must be the potentiality for development of a complete individual but that during development something determines what, when and where tissues and organs shall be developed. In these hybrids that force is not acting, in a normal way at least, and hence the cells develop the various tissues in a haphazard way and otherwise misbehave.

The opinion has been expressed a number of times that the different species of a single genus having different chromosome numbers may have been derived, the larger from the smaller number, by fragmentation or by duplication resulting from non-disjunction during synopsis. Rosenberg[2] has recently worked out the specific chromosome number of 15 species of Crepis (published 19, 4 having been determined by other cytologists). He expressed the belief that the species having four and five pairs of chromosomes could have been formed by non-disjunction occurring during the reduction division. Bridges[3] has shown that such behavior is possible, at least for the sex chromosomes in Drosophila, for he has found flies with ten chromosomes, the typical number for the species being eight. The increased number came about through secondary non-disjunction of the sex chromosomes in both parents. This fly, a female, was able to live and reproduce. These two cases are not, however, equivalent since the ten-chromosome Drosophila had the regular female content plus two Y chromosomes which are not known to influence the production of characters except that absence of the Y chromosome causes sterility of the males.

Whether a zygote containing a duplicated pair of chromosomes can go through development may depend entirely upon which pair is duplicated. From our knowledge of the chromosome numerical variations in Oenothera and Primula certainly it is possible for a plant to develop to maturity with one extra chromosome or with an entire duplicate set.

The behavior of the seven-chromosome Crepis hybrid leads us to believe that there is not such a direct relationship between the two parent species as Rosenberg suggests. However, this in no way does violence to Rosenberg's hypothesis, for *C. tectorum* may behave in quite a different way when crossed with some other three paired chromosome species of Crepis.

It is possible that more light may soon be thrown upon this side of the problem inasmuch as there are now 36 species of Crepis with which trial crosses are being made to further test their chromosome relationships.

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[1] Clausen, R. E., and Goodspeed, T. H., *Proc. Nat. Acad. Sci.*, **2**, 1916 (240–44).

[2] Rosenberg, O., *Arkiv. för Botanik.*, **15**, 1918 (11).

[3] Bridges, C. B., *Genetics*, **1**, 1, 1916 (1–52; 107–63).

NEW OBSERVATIONS ON THE VARIABILITY OF THE SUN

By C. G. Abbot

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Read before the Academy, April 26, 1920

Photometric Observations of the Planets.—(*a*) Simultaneous spectrobolometric observations of the solar constant of radiation in California and Algeria in 1911 and 1912, and in California and Chile in 1918, (*b*) comparisons of the distribution of radiation over the sun's disk with simultaneous measurements of the intensity of total solar radiation, (*c*) comparisons of the temperature of the earth with the radiation of the sun, and (*d*) several other minor evidences, have all indicated a short irregular periodicity in the sun's emission. In other words, the sun appears to be a variable star ranging through about 0.10 stellar magnitude between extremes and often changing 0.03 magnitude within a few days.

If this is so it must follow that the planets are also variable because they reflect solar light. Guthnick and Prager have made accurate photoelectric photometric comparisons of Saturn and Jupiter with available stars. Their published results led them to state a conclusion adverse to the solar variability (*Veröffentlichungen K. Sternwarte Berlin-Babelsberg*, **2**, Heft III, 1918 (126)). I wrote to Dr. Guthnick pointing out that this conclusion was premature, because the number of published photometric observations was not large, and they might have fallen on dates when the sun's emission was nearly the same. No direct solar observations were available on those dates. Dr. Guthnick has kindly reopened his investigation and sent me results for January, February, March, April and May, 1920.

We have available for comparison on almost all these days observations of the sun by Smithsonian observers at Calama, Chile. But it is not obvious that a comparison between the brightness of the planets and that of the sun should involve identical dates. Two hypotheses of solar variation may be made. First, the sun's emission may vary in all directions proportionally and simultaneously. Second, the sun may be surrounded by a ragged absorbing or radiating envelope so that his emission is unequal in different directions. Under this second hypothesis the rotation of the sun would carry with it shafts of unequal radiation, which would encounter the planets successively according to their heliocentric longitudes. This second hypothesis is plausible in view of the dissymmetry of the solar corona. It has the advantage, too, of not requiring rapid changes of the sun's emission, which would be hard to account for in view of the immensity of the sun.

Dr. Guthnick states that Jupiter varied irregularly and widely in surface conditions during the period of observation so that its fluctuations are not available for solar constant comparisons. Various well-known causes of variation in addition to the supposed solar changes affect the brightness of Saturn. In the observations recently sent to me Dr. Guthnick has corrected for all of these influences except those associated with phase. These produce quite a large effect, so that, for instance, the comparisons between Saturn and α Leonis show gradually progressive differences ranging from 0.61 to 0.90 stellar magnitude. He states that for 1920 observations the influence of phase is approximately eliminated by subtracting from the given magnitude differences the product of the phase angle in degrees by a coefficient, which before opposition is 0.025, but after opposition is 0.039. These correcting coefficients are, however, inapplicable for observations made within less than 1° of opposition. Saturn then becomes considerably brighter than would be expected.

I have reduced the observations on Saturn to constant phase angle by applying the coefficients just mentioned. On three dates, namely, Feb. 25, Feb. 28 and March 1, the phase angle is less than 0.5°, so that the corrections are not applicable for these dates. The observation of March 13 is stated by Dr. Guthnick to be of small weight because of unfavorable sky conditions in Berlin. Those of April 23 and May 12 are not of very great weight because Saturn was too near the horizon.

In a first comparison I employed solar constant observations made in Chile on even dates with the observations made in Berlin. The result was so unsatisfactory as in my judgment to discredit the first hypothesis of a solar variation, appearing simultaneously in all directions. I then turned to the second hypothesis, making allowance for differences of heliocentric longitude of the earth and Saturn, and also for differences of terrestrial longitude and times of observing between Chile and Berlin. I thus arrived at the results given in the following table. In addition to the six days mentioned above as unsuitable, Feb. 7 is also unsatisfactory, owing to very cloudy conditions in Chile.

There remain nine days suitable for comparison. These nine days are on the whole favorable to the view that the sun and Saturn each varied in brightness by two per cent, and that their variations were synchronous and proportional. The largest discrepancy which occurs among these nine comparisons is for March 9.5/10 and it indicates an error of slightly more than 0.5 per cent, or 0.005 magnitude. This amount of error is surely to be expected occasionally. Indeed it is very remarkable how accurate the results appear to be both in Berlin and Chile.

Extraordinary March of Solar Radiation 1919–20.—For about six years the intensity of solar radiation has almost always exceeded the mean value, 1.933 calories per square centimeter per minute, which was found from the Washington, Mt. Wilson and Mt. Whitney observations of 1902–12, as published in Volume III of the Annals of the Astrophysical Observatory. This condition of affairs was expected to attend the return of increased solar activity, otherwise evidenced by numerous sunspots,

DATE	SATURN—α LEONIS	PHASE ANGLE	SATURN ⊙ LONGITUDE	EARTH ⊙ LONGITUDE	Δ	SOLAR LAG. $\frac{25.4\ \Delta}{360}$ DAYS	NEAREST CORRESPONDING DATE, CALAMA	SOLAR CONSTANT	GRADE	CORRECTION FOR PHASE* M.	CORRECTED SATURN—α LEONIS. M.
Jan. 29.6	+0.743	3.18°	157° 13′	128° 55′	28° 18′	2.0	Jan. 27	1.975	S	—0.079	+0.664
Feb. 8.6	0.715	2.16	36	139 3	18 33	1.3	Feb. 7	1.946	U+	0.054	0.661
14.5	0.704	1.53	49	140 59	16 50	1.2	13	1.962	U+	0.038	0.666
16.5	0.701	1.30	53	147 5	10 48	0.8	15	1.951	S—	0.032	0.669
17.5	0.690	1.18	55	148 6	9 49	0.7	17	...	...	...	...
18.6	0.687	1.07	58	149 7	8 51	0.6	18	1.971	S—	0.027	0.660
19.5	0.681	0.96	158 0	150 6	7 54	0.6	19	1.983	S—	0.024	0.657
23.5	0.637	0.53	8	154 7	4 1	0.3	23	...	...	...	...
25.5	0.633	0.34	12	156 7	2 5	0.1	25	1.987	E—, S	0.008	0.625
28.5	0.614	0.25	18	159 8	—0 50	—0.1	28	1.953	S—	0.010	0.604
Mar. 1.5	0.653	0.40	22	161 8	—2 45	—0.2	Mar. 1	1.980	S—	0.016	0.637
9.5	0.711	1.26	40	169 8	—10 28	—0.7	10	1.959	S	0.049	0.662
13.5	0.708	1.70	48	173 6	—14 18	—1.0	14	1.962	VG, S	0.066	0.642
24.4	0.783	2.85	159 11	183 58	—24 8	—1.7	26	1.953	S	0.111	0.672
Apr. 25.5	0.784	2.96	13	185 03	—25 8	—1.8	27	1.966	E, S	0.115	0.669
12.4	0.854	4.54	51	202 41	—42 8	—3.0	Apr. 15	1.944	S—	0.177	0.677
23.4	0.860	5.27	160 14	213 26	—53 2	—3.7	26	1.960	VG+, S	0.205	0.655
May 11.4	0.899	6.04	52	230 52	—70 0	—4.9	May 16	1.957	S	0.235	0.664

* 0.025 and 0.039 phase coefficients before and after opposition.

prominences, faculae. We have now, however, long passed the period of maximum sunspots, so that we should naturally expect the sun's radiation to be falling below the mean value of 1902–12. The results obtained by the Smithsonian observers at Calama, Chile, indicate quite otherwise.

I have computed solar radiation values for each five days interval from July 1, 1919 to March 25, 1920. The mean value is never based on less than two observations, and this minimum occurs only in two instances. All other values depend on three days of observation, more often four, and very often five.

One is immediately struck by the wide fluctuation of the mean values shown. The fluctuation of individual days naturally had a still wider range, reaching in fact to 8%. The mean values cover a range of 5%. With gradually diminishing swings, up and down, the radiation fell from June 1919 to early in October, then suddenly leaped up to a high mean value which it maintained until early in December, and then again suddenly leaped much further and remained from the end of December to the middle of March 1920 at a mean value far in excess of anything which we have any record of, continued for so long a time as three months during the whole fifteen years in which solar constant observations have been carried on with anything like regularity. Towards the end of March an extremely rapid fall of radiation occurred, so that individual values have run as low as 1.86 calories.

1919	A	B	C	D	E	F
June	46	84	37	39	71	53
July	36	54	47	63	57	31
Aug.	53	54	38	36	51	45
Sept.	28	33	30	42	31	30
Oct.	18	57	49	46	59	62
Nov.	60	51	60	43	47	54
Dec.	55	48	54	60	67	81
1920						
Jan.	69	102(?)	74	78	81	70
Feb.		87	60	78	77	68
Mar.	77	65	70	50	10	

In view of this extraordinary march of solar radiation values, it may be recalled that we have been passing through an exceptionally cold and cloudy winter from about the first of December. The cloudiness has prevailed in South America as well as here, so that if it had not been for the introduction of the new method of observing, of which notice was given to the Academy at its last meeting, the observers would not have been able to give us this very continuous record.

At first sight it looks paradoxical that a cold winter could accompany extraordinarily high values of solar radiation, but it has been not only a cold winter but a cloudy winter. Hence it may have been that the direct effect of the outburst of solar activity was to produce excessive cloudiness which by high reflection diminished the radiation available to warm the earth.

In the preceding table I give the mean values of the solar radiation above mentioned. In each month I have indicated the successive five day periods by the capital letters A, B, C, D, E, and F. The values given are the number of thousandths of a calorie by which the solar radiation of a given time interval exceeds 1.900. Thus, for the first period of June the mean value is 1.946.

March Values.—On or about March 22, great sunspot activity was reported. On March 22 and 23 there were intense magnetic disturbances affecting all observations of terrestrial magnetism and the operation of telegraphs and cables. Remarkable auroral displays followed. In connection with these conditions it is interesting to note the very unusual progress of the solar constant of radiation during the month of March. This is given in the following table

Date	Mean, 11 to 17	18	19	20	21	22	23	24
Value	1.968	1.954	1.940	1.931	1.941	1.927	1.866	1.905

It is highly probable that the results just given will have a special significance in connection with the remarkable outbreak of solar activity to which attention has been drawn.

THE PERMANENT GRAVITATIONAL FIELD IN THE EINSTEIN THEORY

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Communicated by E. H. Moore, June 7, 1920

1. In accordance with the theory of Einstein a permanent gravitational field is defined by a quadratic differential form

$$ds^2 = \sum_{i,k}^{1,..4} g_{ik}dx_idx_k, \quad (g_{ik} = g_{ki}), \qquad (1)$$

where the g's, called the potentials of the field, are determined by the condition of satisfying ten partial differential equations of the second order, $G_{ik} = 0$. When the four coördinates x_i are functions of a single parameter, the locus of the point with these coördinates is a curve in four-space. If these functions are of such a character that the integral

$$\int \sqrt{\Sigma g_{ik}dx_idx_k} \qquad (2)$$

is stationary along the curve, the curve is called a "world-line," or a geodesic, in the four-space.

Einstein[1] considered the case when x_1, x_2, x_3, are rectangular coördinates and x_4 represents the time, and assumed that the field was produced by a mass at the origin which did not vary with the time. In order to obtain the equations of the world-lines in the form which enabled him to establish his well-known expression for the precession of the perihelion of Mercury, Einstein made also the following assumptions:

A. The quantities g_{ik} are independent of t.

B. The equations $g_{i4} = 0$ for $i = 1, 2, 3$.

C. The solution is spacially symmetric with respect to the origin of coördinates in the sense that the solution is unaltered by an orthogonal transformation of x_1, x_2, x_3.

D. The quantities $g_{ik} = 0$ for $i \pm k$ at infinity and also $g_{44} = -g_{11} = -g_{22} = -g_{33} = 1$. Schwarzschild,[2] using the first three of these assumptions and certain others integrated the equations $G_{ik} = 0$, and obtained (1) in the form

$$ds^2 = \left(1 - \frac{\alpha}{R}\right) dt^2 - \frac{dR^2}{1 - \frac{\alpha}{R}} - R^2(d\theta^2 + \sin^2 \theta d\varphi^2), \tag{3}$$

where α is a constant depending on the mass at the origin. Levi-Civita[3] has given three solutions of the equations $G_{ik} = 0$, one of which includes the above, and Weyl[4] has given still another solution. Later, Kottler[5] obtained the form (3) not by the solution of the equations $G_{ik} = 0$ but as a consequence of certain postulates. It is the purpose of this paper to accomplish the same result by the following set of postulates:

I. Assumptions A and B of Einstein, in accordance with which we write (1) in the form

$$ds^2 = V^2 dt^2 - ds_0{}^2, \tag{4}$$

where

$$ds_0{}^2 = \sum_{ik}^{1,2,3} a_{ik} dx_i dx_k, \tag{5}$$

the functions V and a_{ik} being independent of t.

II. The function V is a solution of

$$\Delta_2 V = 0, \tag{6}$$

where $\Delta_2\theta$ is the Beltrami differential parameter formed with respect to the form (5), and is defined by

$$\Delta_2\theta = \frac{1}{\sqrt{a}} \sum_{i,k}^{1,2,3} \frac{\partial}{\partial x_i} \left(\sum_{k}^{1,2,3} a^{(ik)} \sqrt{a} \frac{\partial \theta}{\partial x_k} \right), \tag{7}$$

where a is the determinant of the functions a_{ik} and $a^{(ik)}$ is the co-factor of a in this determinant divided by a. This assumption is equivalent to the equation $G_{44} = 0$.

III. The surfaces V = const. form part of a triply orthogonal system in the space, S_3, of coördinates x_1, x_2, x_3.

IV. The orthogonal trajectories of V = const. in S_3 are paths of the particle, in the sense that the coördinates x_1, x_2, x_3, of a world-line determine a path in S_3 of a particle in the gravitational field for which the world-line is the representation in terms of space and time t.

V. The form (5) is euclidean to a first approximation.

2. In order to simplify the equations, we take (5) in the form

$$ds_0{}^2 = \sum_{i}^{1,2,3} a_i dx_i{}^2. \tag{8}$$

If we choose s for the parameter along a world-line and apply the Euler equations of condition that the integral

$$\int \sqrt{V^2 dt^2 - ds_0{}^2}$$

be stationary along the line, we get

$$\frac{d^2x_i}{ds^2} + \sum_j \frac{\partial \log a_i}{\partial x_j}\frac{dx_i}{ds}\frac{dx_j}{ds} - \frac{1}{2a_i}\sum_j \frac{\partial a_j}{\partial x_i}\left(\frac{dx_j}{ds}\right)^2 + \frac{V}{a_i}\frac{\partial V}{\partial x_i}\left(\frac{dt}{ds}\right)^2 = 0, \tag{9}$$

$$\frac{dt}{ds} = \frac{k}{V^2}, \tag{10}$$

where k is a constant. In these equations and hereafter the symbol $\sum\limits_j$ means the sum for $j = 1, 2, 3$.

We inquire under what conditions the path of a particle in S_3 is a geodesic, that is a curve along which the integral $\int \sum\limits_i a_i dx_i{}^2$ is stationary. When s_0 is taken for the parameter along such a geodesic, we find that

$$\frac{d^2x_i}{ds_0{}^2} + \sum_j \frac{\partial \log a_i}{\partial x_j}\frac{dx_i}{ds_0}\frac{dx_j}{ds_0} - \frac{1}{2a_i}\sum_j \frac{\partial a_j}{\partial x_i}\left(\frac{dx_j}{ds_0}\right)^2 = 0. \tag{11}$$

From (4) and (10) it follows that the parameters s and s_0 along a world-line and the corresponding path in S_3 are in the relation

$$ds_0 = \sqrt{\frac{k^2}{V^2} - 1}\, ds. \tag{12}$$

With the aid of this relation we find that in order that equations (9) and (11) define the same curves in S_3, we must have

$$\frac{dV}{ds_0}\frac{dx_i}{ds_0} - \frac{1}{a_i}\frac{\partial V}{\partial x_i} = 0 \quad (i = 1, 2, 3). \tag{13}$$

These are the conditions that the path be an orthogonal trajectory of the surfaces V = const. When we require that equations (11) and (13) be consistent, we find that it is necessary and sufficient that ΔV be a function of V, say

$$\Delta V = \varphi(V), \tag{14}$$

where ΔV is the first differential parameter of V with respect to the form ds^2_0. For the form (5) its expression is

$$\Delta V = \sum_{ik} a^{(ik)} \frac{\partial V}{\partial x_i} \frac{\partial V}{\partial x_k}. \tag{15}$$

When (14) is satisfied by a function V, the surfaces V = const. are said to be *geodesically parallel.* Hence we have the theorem:

A necessary and sufficient condition that a path in a gravitational field be a geodesic in S_3 is that it be one of the orthogonal trajectories of the surfaces V = const., which must form a geodesically parallel family.

3. The function V is interpreted as the velocity of light in the field, and consequently along a world-line of a ray of light $ds = 0$, as follows from (4). In order to find the equations of these world-lines we apply the Fermat principle that $\int dt$ be stationary along such a line, that is the integral $\int \sqrt{\frac{1}{V^2} \Sigma a_i dx_i^2}$. This gives the equations

$$\frac{d^2x_i}{dt^2} + \frac{dx_i}{dt} \sum_j \frac{\partial}{\partial x_j} \log \frac{a_i}{V^2} \frac{dx_j}{dt} - \frac{V^2}{2a_i} \sum_j \frac{\partial}{\partial x_i} \left(\frac{a_j}{V^2}\right) \left(\frac{dx_j}{dt}\right)^2 = 0.$$

When we require that the path of a ray of light be a geodesic in S_3, we obtain equations (13) and (14) as formerly. Hence:

When the orthogonal trajectories of the surfaces V = const. are paths of a particle in a gravitational field, they are also the paths of a ray of light; and conversely.

4. In accordance with III we choose the coördinates x_1, x_2, x_3, so that V is a function of x_1 alone and we have (8). Then from IV, which is equivalent to (14), written $\Delta V = \varphi(x_1)$, and II we have

$$a_1 = \frac{(V')^2}{\varphi}, \quad a_2 a_3 = \frac{\psi^2}{\varphi}, \tag{16}$$

where the prime indicates differentiation with respect to x_1 and ψ is independent of x_1.

If $d\bar{s}_0{}^2 = \Sigma \bar{a}_i dx_i^2$ is the linear element of euclidean space, the functions $\bar{a}_i$ must satisfy the six equations of Lamé.[6] If a_1 is to be a function of x_1 alone and the second of (16) is to be satisfied, it can be shown that the coördinates x_1 can be chosen, so that

$$d\bar{s}_0{}^2 = dx_1^2 + x_1^2(\sigma^2 dx_2^2 + \tau^2 dx_3^2), \tag{17}$$

where σ and τ are independent of x_1 and satisfy

$$\frac{\partial}{\partial x_2}\left(\frac{1}{\sigma}\frac{\partial \tau}{\partial x_2}\right) + \frac{\partial}{\partial x_3}\left(\frac{1}{\tau}\frac{\partial \sigma}{\partial x_3}\right) + \sigma\tau = 0. \tag{18}$$

This is the condition that the expression in parenthesis in (17) is the linear element of the unit sphere.[7]

If we write

$$V^2 = c^2(1 + 2\varphi_1(x_1))$$

where c is the constant velocity of light, and in accordance with assumption V require that (16) be satisfied by expressions approximating the coefficients in (17), we obtain $\varphi_1 = -\frac{b}{2cx_1}, \frac{1}{a_1} = 1 + 2\varphi_1$, where b is a constant. Hence, if we take $\sigma = 1$, $\tau = \sin x_2$, which is a set of solutions of (18), we obtain the form (3). Another choice gives at once one of the forms found by Levi-Civita.[3]

[1] *Berlin Sitzungsberichte,* 1915 (831).

[2] *Ibid.,* 1916 (189).

[3] *Rendiconti dei Lincei* (Ser. 5), **27,** 1918 (350).

[4] *Ann. Physik,* **54,** 1917 (117).

[5] *Ibid.,* **56,** 1918 (401).

[6] Eisenhart's *Differential Geometry,* (449).

[7] *Ibid.,* (157).

A SIMPLIFIED METHOD FOR THE STATISTICAL INTERPRETATION OF EXPERIMENTAL DATA

By George A. Linhart

Division of Soil Chemistry and Bacteriology, University of California

Communicated by W. J. V. Osterhout, June 1, 1920

One of the fundamental postulates of the law of probability of errors is that positive and negative errors are equally frequent and that, therefore, the arithmetical mean is the most probable mean. This is true when we are dealing with *small independent errors,* but in cases of *interdependent* values of natural *frequencies* (physical, biological, agricultural), it may or may not be true, depending upon the maximum deviation from the mean. Thus we can conceive of a molecule of oxygen gas with a momentary velocity of zero or of "infinity."[1] Yet the average velocity as determined from density and pressure measurements is but a few hundred yards per second, a value not very far from zero, but infinitely different from "infinity." When the frequencies of such data are plotted on rectangular coördinate paper, we obtain what are called skew curves. Within the last twenty-five years a great deal has been written concerning skew curves and their types, and, judging from the number which have thus far appeared in print, they promise to be of infinite variety. To these various types of frequency curves have been fitted mathematical formulas and with their aid statistical constants have been obtained, many of which we have reason to believe are theoretically inapplicable and practically misleading.

In a paper now in press and another in preparation, it is shown that all the types of frequency curves thus far published, excepting those having a zero class, conform to one mathematical expression. It is pointed out also that it is futile to try to fit discontinuous irregular frequencies as well as those having a zero class to mathematical formulas, since the statistical constants obtained from such formulas are too often misleading. For these types of natural or perhaps unnatural frequencies a graphical method is developed capable of yielding any desired degree of accuracy; that is, within experimental error. The general equation referred to above is,

$$\frac{y}{y_0} = e^{-k^2\left(\log \frac{m}{m_0}\right)^2} \tag{1}$$

where m denotes the numerical value of any measurement, m_0 the value of the mean, e the base of natural logarithms, y any frequency, and y_0 a frequency of the probability of a deviation zero.

Obviously, when in equation (1), m is greater than zero and less than twice m_0, we may express the exponent of e in series, thus,

$$\frac{y}{y_0} = e^{-k^2\left[\left(\frac{m}{m_0}-1\right)-\frac{1}{2}\left(\frac{m}{m_0}-1\right)^2 + 1/3\left(\frac{m}{m_0}-1\right)^3 \ldots\right]^2}. \tag{2}$$

Neglecting all terms but the first, we obtain,

$$\frac{y}{y_0} = e^{-k^2\left(\frac{m-m_0}{m_0}\right)^2} = e^{-h^2(m-m_0)^2} = e^{-h^2x^2}, \tag{3}$$

which is identical with the usual form of the equation for the probability of errors, and which is but an approximation of equation (1).

In order to minimize the labor and time required to perform a large set of calculations, often running into the thousands, equation (3) was transformed into a rectilinear one, thus,

$$\text{Log}\,(\text{Log}\,y_0/y) = 2\,\text{Log}\,x + 2\,\text{Log}\,h - 0.3623 = 2\,\text{Log}\,x + K \tag{4}$$

With a few determinative values for x read off on the frequency polygon constructed from the experimental data, we may obtain the value for K, which is the distance on the Log (Log y_0/y) axis from the origin to the point of intersection. We can then calculate the several statistical constants from the value of K; for example, the index of precision, h; the probable error, x; the probable error of the mean, x_0; the standard deviation; the coefficient of variability, etc.; or,

$$h = (10)^{\frac{k+0.3623}{2}},$$

$$x = 0.4769\,(10)^{-\frac{k+0.3623}{2}},$$

$$x_0 = \frac{x}{\sqrt{n}} = \frac{0.4769}{\sqrt{n}}\,(10)^{-\frac{k+0.3623}{2}}, \text{ etc.}$$

Details of the method, as well as numerous examples from biology, genetics, physics and agriculture will be given in two subsequent papers.

---

[1] Actually it was found that the chances are 20000 : 1 that a molecule of oxygen gas would have a resultant velocity, at 0° C., of less than 50 meters per second, or greater than 2500 meters per second, the mean being 353.6 meters per second. Here the mean is not the arithmetical mean of the variables, but the *n*th root of the products of the *n* (dependent) variables. The theoretical basis for this will be given in another paper. For a mathematical discussion see Galton and McAlister, *Proc. Roy. Soc. Lond.*, **29**, 1879 (365).

A CRITICAL STUDY OF FERTILIZER EXPERIMENTS

BY C. B. LIPMAN AND G. A. LINHART

UNIVERSITY OF CALIFORNIA

Communicated by W. J. V. Osterhout, June 1, 1920

Among the most expensive and time-consuming experiments in agricultural investigations have been those which attempt to ascertain the proper chemical amendments or fertilizers for soils. This applies in particular to the long-term fertilizer experiments like those at Rothamsted in England, at State College in Pennsylvania, and at Wooster and Strongsville in Ohio. Conceived and inaugurated, as they were, at times when little or no real authentic information was available relative to the nature of soils and plants, it is but natural that fallacious and short-sighted planning should have dominated them. It occurred to one of us that as critical thinking in this field has become more general, and facts more plentiful, it is high time that fertilizer experiments in general and the "long-term" experiments in particular be subjected to critical scrutiny. It seemed desirable to employ the statistical method for this purpose, particularly in view of the striking results obtained in this laboratory by our former associate, D. D. Waynick. It seemed clear to one of us that if, as Waynick, and Waynick and Sharp had demonstrated, the variability of soils and of plants is very large even within selected and presumably uniform material, one could not expect previous fertilizer experiments to be of much value, since the factor of variability has been entirely ignored in their arrangement and study, and the probable error to which they were subject was not determined. Accordingly, Lipman and Waynick began in January, 1919, a systematic study by statistical methods of the results of fertilizer experiments at the Ohio and at the Pennsylvania Agricultural Experiment Stations. Mr. Waynick left this laboratory the following July, but with a number of interruptions, the work has been continued, and we hope to issue the voluminous data, together with a critical discussion of them, in due course. Since the work may be a long time in the press, we deem it wise to present here a few of the salient and most important findings of our studies, so that our colleagues may be apprised of what we consider to be an investigation of a fundamental nature, not merely on account of the importance of the subject itself, but because of its important bearings on agricultural research.

It seems that the originators of both the Ohio and the Pennsylvania experiments saw the value of replicating check plots, while not recognizing the value of such replication for the treated plots. Only in the case of two complete fertilizer plots in the Ohio experiments, was the fertilizer treatment the same. In all the others every treatment was different from that of every other. This situation renders it difficult to subject their data to statistical treatment in the most desirable way. Fortunately, however, the long series of years during which the plots were studied gives, in a certain sense, a replication of plots of each kind. Our procedure has been, therefore, to group the plots in different ways including the following:

1. All the check plots for each year.
2. One check plot for all the years.
3. All the plots treated with "one-element" fertilizers in one year.
4. Every "one-element" fertilizer plot for all the years.
5. The same double study for the "two-element fertilizer plots."
6. The same for the "three-element" fertilizer plots.

In addition to these groupings, others were made in which a given plot was studied with each crop in the rotation, so as to allow of only three to eight "yield" data for each plot. It may be said in passing that the rotation study included in these experiments only serves to complicate a situation which was already sufficiently difficult. We shall show in detail in the complete study which is to be published, that these experiments are utterly inadequate for solution of the questions involved and that all such fertilizer experiments are so fallacious as that they do not justify the use of the large amounts of money that are constantly being lavished upon them.

For the sake of brevity, only a few of our findings, from the study of the Ohio data alone, are summarized here.

1. The "one-element" fertilizer treatments show no significant increases in yield over the yields obtained on the untreated check plots. All statements to the contrary which have heretofore been made are erroneous and the data on which they are based were obtained by a misleading method of procedure and evaluation.

2. The "two-element" fertilizer plots give definite increases in yield over the untreated control plots. Whether or not the increases are profit-yielding, however, is a question which will be discussed later.

3. The "three-element" fertilizer plots give definite increases in yield over both the untreated control plots and the "two-element" fertilizer plots. Here again the question whether the increases are profit-yielding still remains to be determined.

4. The kind of fertilizer used seems to have been without significance, one being just as good as another, subject to the limitations above noted.

5. The amount of fertilizer used seems likewise to have been without significance (subject to the same limitations).

6. Even when fertilizer experiments are properly planned and the results adequately studied by statistical methods, our present knowledge of the enormous variability of all soils and plants renders the data from any given fertilizer plot of value only on that plot, no matter how near the experimental one. This important consideration renders it highly probable that no fertilizer experiment as ordinarily conducted is possessed of sufficient practical value to justify the large expenditure of money, time, and energy involved.

We wish to emphasize that we are not desirous of making a fetish of the application of statistical study to fertilizer experiments. Nevertheless, we must say that if statistical methods, inadequate as they may be, should not prove applicable to an evaluation of the results of fertilizer experiments, the latter must be accounted of even less value and significance than we have here accorded to them.

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CONTENTS

Page

GENETICS.—HOMOLOGOUS GENES AND LINEAR LINKAGE IN DROSOPHILA VIRILIS. *By Alexander Weinstein* 625

PHYSICS.—THE INTENSITIES OF X-RAYS OF THE L-SERIES, III. CRITICAL POTENTIALS OF THE PLATINUM AND TUNGSTEN LINES . . . . *By Frank C. Hoyt* 639

MATHEMATICS.—SEMIVARIANTS OF A GENERAL SYSTEM OF LINEAR HOMOGENEOUS DIFFERENTIAL EQUATIONS . . . . *By E. B. Stouffer* 645

PHYSICS.—SOME NEW METHODS IN INTERIOR BALLISTICS . . . . *By Arthur Gordon Webster* 648

GENETICS.—THE MUTANT CROSSVEINLESS IN DROSOPHILA MELANOGASTER . . *By Calvin B. Bridges* 660

——— IS CROSSING OVER A FUNCTION OF DISTANCE? . . . . *By J. A. Detlefsen* 663

——— INTERSPECIFIC HYBRIDS IN CREPIS . . . . *By E. B. Babcock and J. L. Collins* 670

ASTRONOMY.—NEW OBSERVATIONS ON THE VARIABILITY OF THE SUN . . . . *By C. G. Abbot* 674

MATHEMATICS.—THE PERMANENT GRAVITATIONAL FIELD IN THE EINSTEIN THEORY . . . . *By L. P. Eisenhart* 678

STATISTICS.—A SIMPLIFIED METHOD FOR THE STATISTICAL INTERPRETATION OF EXPERIMENTAL DATA . . . . *By George A. Linhart* 682

AGRICULTURE.—A CRITICAL STUDY OF FERTILIZER EXPERIMENTS . . . . . *By C. B. Lipman and G. A. Linhart* 684

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PRELIMINARY REPORT ON PTERERGATES IN POGONOMYRMEX CALIFORNICUS

By Harlow Shapley

Mount Wilson Observatory, Pasadena, California

Communicated by William Morton Wheeler, November 6, 1920

The phylogeny of castes among social insects has a recognized importance in the question of development of organic forms through continuous or saltatory variation. For the Formicidae the castes are already well established in the early Tertiary,[1] among the first fossil records of these insects, although in the primitive ancestral wasps neither the social habit nor the polymorphism of the female appears. Differentiation into soldier, worker, and fertile forms is also a long-standing condition with the termites, probably dating from the Cretaceous.

In discussing the origin of castes among ants, Wheeler has considered the bearing of the subapterous and apterous females (particularly in the genus *Monomorium*) on the problem of variation through mutation.[1] He concludes that although the sterile worker-females are now almost universally wingless, we should not believe that this micronotal wingless condition arose through mutants. Rather "this view of the castes, at least so far as their origin is concerned, cannot be maintained, because all the available evidence points to their being merely the surviving extremes of graduated and continuous series of forms, the annectant members of which have suffered phylogenetic suppression or extinction." For the termites Thompson and Snyder find few intermediate types, but trace a gradation of characteristics throughout the series of five castes (workers, soldiers, and three fertile forms), and find a further argument against mutational origins in the fact that the different castes are essentially constant both in occurrence and in structure.[2]

It is of interest, in connection with these views on the phylogenetic development of polymorphism, to find in typical worker ants some ontological evidence of early stages in the origin of castes—to find, in particular, that the embryonic vestiges of wings, discovered in workers of *Formica* by Dewitz,[3] do not invariably disappear with the passing of larval and pupal stages, but occasionally persist to the adult state. Wheeler described four adult workers of this kind in 1905, naming them pterergates,[4] but such forms are apparently very uncommon. As far as I know, only six were on record before 1919. Since then, in addition to the ants discussed in the present note, Professor Wheeler has kindly sent me one pterergate taken in Nova Scotia, I have found one, taken by Blaisdell, in the collection of the California Academy of Sciences, and one in a nest of *Pogonomyrmex* in Pasadena.

The record of species and localities for these nine pterergates is as follows:[5]

Pogonomyrmex californicus subsp., taken by Blaisdell, 1885, Powai, California.
Cryptocerus aztecus, taken by Wheeler, 1900, Cuernavaca, Mexico.
Myrmica scabrinodis var. (3), taken by Wheeler, 1904, Bronxville, New York.
Lasius flavus, taken by Bondroit, 1910, Landelies, Belgium.
Leptothorax curvispinosus, 1911, Weymouth, Nova Scotia.
Myrmica scabrinodis, taken by Keys, 1913, Yelverton, England.
Pogonomyrmex californicus, taken by Shapley, 1920, Pasadena, California.

All of the above, except *Lasius*, are Myrmicine ants.

From a local nest of the red Californian Harvester, *Pogonomyrmex californicus* Buckley, I have taken during the last two years more than 1700 workers, nearly one-half of which have vestigial wings in various stages of development. A summary of observations on this unique nest is given in the following paragraphs. An analysis of the development of the wing venation is reserved for later publication.

TABLE I

NUMBERS AND RELATIVE FREQUENCY OF PTERERGATES

GROUP	DATE	PTERERGATES	NORMAL WORKERS	PERCENTAGE PTERERGATES
a	Prior to Nov. 11, 1919	13	11	54
b	Nov. 11, 1919, to June 1, 1920	89	87	51
c	June 6, 1920	266	324	45
d	June 8, 1920	56	100	36
e	Oct. 15, 1920	49	51	49
f	Oct. 19, 1920	87	159	35
g	Oct. 19, 1920	8	16	33
h	Oct. 24, 1920	172	249	41
	TOTAL	740	997	42.6

1. The occurrence during 1919 and the early months of 1920 of equal numbers of pterergates and normal workers suggested that Mendelian factors might be involved in the appearance of wing vestiges. The pterergates became relatively much less frequent, however, in June, 1920 (table 1), suggesting that the tendency to develop vestiges was less pronounced or entirely absent for the 1920 brood; but on October 15 the per-

centage of affected forms appears temporarily re-established, notwithstanding that 320 of the normal ants had been returned to the nest after the June collections. Among the ants taken October 18 and 24 were a number of callows with well-developed vestigial wings; this also indicates that the tendency to produce pterergates still exists.

2. There is no obvious external reason why this particular nest should show such frequent reversion to a remotely ancestral condition of the worker ant. The intermittent war with Argentine ants (*Iridomyrmex humilis Mayr*), which is apparently destined to end in the elimination of most of the native ants in the California valleys, is no more severe for the nest of pterergates than for many other nests of *Pogonomyrmex.* This nest, however, has access to very little wild barley and similar grasses, a common food of the species; on the other hand, it is exceptionally well provisioned with mixed grains from a nearby feed store whenever the Argentine ants permit the nest to be opened for normal harvesting activities.

3. A small colony of the same species, less than ten feet distant from the nest containing pterergates, yields workers indistinguishable from the normal ants of the affected nest, but no pterergates; in fact, an examination of several thousand individuals from some fifty other colonies of this species within a radius of two miles has shown only the one pterergate listed above.

4. In all details of thoracic structure—size, sculpture, pilosity—the pterergates are identical with normal ergates of this and neighboring nests. In excavating and guarding the nest, and in harvesting, the pterergates and the normal workers participate equally. Hence these abnormal ants are certainly workers rather than modified fertile forms, such as pseudogynes, microgynes, and β-females.

5. The seventeen young queens and two males, taken from the nest in June, 1920, appear to be normal in every way when compared with queens and males from other nests of the same species.

6. Grouping the pterergates roughly in order of the development of vestiges, we have the following enumeration:

With minute veinless wing-sacs, or with stubs of broken wings..........	385
With sacs from 0.5 to 1.0 mm. long and indistinct veining.............	219
With transparent, clearly veined winglets from 0.8 to 1.5 mm. long.....	132

The gradation of wing vestiges is, however, perfectly continuous, from small protuberances (without appendages) on the mesothoracic segment to the most developed winglets, with venation approaching that of the wing (6 mm. in length) of the mature queen. This continuity in structure may be significant for the problem of variation and the origin of castes.

7. Four ants from the affected nest have vestiges of both posterior and anterior wings—a phenomenon not heretofore recorded, as only anterior wings are represented in all other pterergates.

[1] Wheeler, W. M., These PROCEEDINGS, **3,** 1917 (109–117).

[2] Thompson, C. B., and Snyder, T. E., *Biol. Bull., Woods Hole, Mass.*, **36,** 1919 (115–132).

[3] Dewitz, *Zs. wiss. Zoöl., Leipzig*, **30,** 1878 (78–105).

[4] Wheeler, W. M., *Bull. Amer. Mus. Nat. Hist., New York*, **21,** 1905 (405–408).

[5] Shapley, H., *Psyche, Boston*, **27,** 1920 (72–74).

HYDROGEN ION CONCENTRATION OF THE CONTENTS OF THE SMALL INTESTINE

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Communicated by W. A. Noyes, October 8, 1920

The contents of the small intestine were removed by swallowing a tube 7 feet long so far down that the outer end was at the teeth and there were no loops or coiled portions in the stomach. The tube was of smooth rubber, 1.5 mm. internal diameter and 3.5 mm. external diameter, and hence stiff enough to keep from collapsing. A 6-gram iron sinker was attached to the lower end to assist in the introduction of the tube. The swallowing movements of the oesophagus acting on this sinker pulled the tube down. When the tube reached the pylorus, the subject reclined on his right side with hips elevated so that the sinker would pass through the pyloric sphincter. This passage was accomplished more easily on an empty stomach, in fact, in one subject who swallowed the tube after a meal, passage of the pylorus was not accomplished until about 4 hours had elapsed and repeated trials did not lessen the time required. On an empty stomach, passage was accomplished immediately in two subjects, as demonstrated by the fact that a darkly bile-stained fluid flowed out of the tube. Bile may be regurgitated into the stomach, but is then diluted with gastric juice and saliva. The tubes were kept down in two subjects for 5 days and 4 nights. The subjects ate their meals regularly, went to lectures, went swimming, automobile riding, slept soundly, and, in fact, the only inconvenience experienced was consciousness of the presence of the tube in the throat owing to its stiffness. To alleviate this annoyance they ate ice cream and bananas during the afternoon. The withdrawal of the tube with sinker attached caused no inconvenience. It required the tube about 5 days to descend to its full length and it was examined each day with the fluoroscope by Dr. F. S. Bissell, the antimony in the rubber casting a distinct shadow.

The tube was kept clean by forcing distilled water down it when not in use. Samples of intestinal contents could be obtained only when digested food was coming down from the stomach. At other times hardly a drop of fluid could be obtained. The H ion concentration of the individual samples are without apparent significance, but the average figures for each day show the contents to become less acid as the tube descended farther. The average figure for the 1st day was $10^{-4.7}$, 2nd day 10^{-5}, 4th day $10^{-5.1}$, and 5th day $10^{-6.2}$. The intestinal contents, when incubated at body temperature with boiled egg-white, showed hardly any proteolytic action. Probably egg-white requires to be swollen by the acid in the stomach before rapid proteolysis can take place. Egg-white will swell also in alkali, but the intestinal contents were near the isoelectric point of egg albumin, at which least swelling occurs.

On taking a census of laboratory workers, I found they all considered the intestinal contents as being alkaline, and, in order to account for this erroneous impression, reviewed the literature since the days when physiologists, ignorant of the germ theory, tasted body fluids to see whether they were acid or alkaline. Von Helmont, in 1648, found that the taste changed from acid to salt on passing from the stomach to the intestine. After the discovery of indicators, Tiedemann and Gemlin, in 1826, found that the soda from the common bile duct unites with the acid chyme and makes it less acid. In 1891, Macfadyen, Nencki and Sieber, with the aid of a grant from the Elizabeth Thompson Science Fund, studied the outflow from a fistula in the ileum of a patient continuously for 6 months and found it always acid. It seems strange, therefore, that the popular notion is that the intestine is alkaline and I cannot account for this unless it is due to the fact that the secretions of the pancreas and the glands of Brunner and Lieberkuhn in the intestine are alkaline as shown by titration and with the hydrogen electrode. They are not sufficiently alkaline to neutralize the acid chyme.

My determinations were made with the hydrogen electrode and after 5 years' experience with biological fluids, I have begun to feel confident of determinations without the precaution of triplicate and quadruplicate samples. Palladium electrodes are attacked by acid of the stomach and sometimes of the duodenum. I use gold electrodes plated bright with iridium. By using a very concentrated solution of iridium chloride the plating can be done in about 2 minutes, and the electrode may be used longer without replating than when plated with platinum, and is easier to clean because it is bright. In non-viscous fluids the electrode must be entirely immersed in the sample, and the latter kept saturated with hydrogen gas by shaking. The presence of oxygen gives a low voltage to the hydrogen-calomel cell. In such cases the voltage rises after the shaking has ceased. The acidity of the intestine aids in the preservation of the antiscorbutic vitamine while it is being absorbed. Feeding of lactose changes the intestines of guinea pigs from alkaline to acid.

BLOOD CONCENTRATION CHANGES IN INFLUENZA

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Communicated by L. B. Mendel, October 22, 1920

From the pathological viewpoint influenza presents a striking similarity to the respiratory complications induced by the lethal war gases, chlorine, phosgene and chlorpicrin. In both conditions there are two features that stand out prominently, namely, pulmonary edema and a pneumonic process. In both, edema may be regarded as the response to an intense inflammatory reaction resulting in a loss of the normal protective mechanism of the upper respiratory tract, thus allowing bacterial invasion of the lung.

It has been shown by Underhill[1] and his associates that in acute poisoning by the lethal war gases edema of the lungs is accompanied by marked changes in the concentration of the blood, which, according to Underhill, is the chief factor contributing to death. Blood concentration means a failing circulation, an inefficient oxygen carrier, oxygen starvation of the tissues, fall of temperature, and finally suspension of vital activities.

During the recent epidemic an opportunity was afforded to study a series of cases of influenza in the New Haven Hospital through the cooperation of Dr. George Blumer and his staff. The investigation with these subjects enables us to state definitely *that in the fulminating cases of influenza there is a marked increase in the concentration of the blood resembling closely that observed in the acute stages of gas poisoning. Death invariably followed a marked increase in blood concentration. Again, of the cases that recovered in not a single instance was there evidence of a significant increase in blood concentration.*

Therefore it is quite apparent that both physiologically and pathologically there is a marked resemblance between influenza and war gas poisoning. Blood concentration in both conditions is an important factor in the cause of death. Blood concentration is incompatible with continued existence.

Under proper conditions, blood concentration changes in influenza may be followed by accurate hemoglobin estimations.

A method of treatment evolved for acute gas poisoning has been applied with success in a few cases of influenza. The method consists in the maintenance of blood concentration as near the normal level as possible by venesection and fluid introduction.

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[1] Underhill, *The Lethal War Gases: Physiology and Experimental Treatment*, Yale University, 1920.

LUMINESCENCE AT HIGH TEMPERATURES

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Read before the Academy, November 20, 1920

It is the purpose of this note to announce the discovery of luminescence at temperatures above those at which ordinary phosphorescence disappears and fluorescence, excited by light, reaches extinction: in a word, roughly speaking, above the beginnings of a visible red heat.

Luminescence at these higher temperatures has probably remained unnoticed[1] because the usual source of excitation has been light and photo-excitation, as was observed by Lenard and Klatt[2] in their studies of the phosphorescent sulphides, generally ceases below 400° C. The experiments of Wiedemann and Schmidt[3] on the effect of temperature on the kathodo-luminescence of certain substances reached their upper limit at about the same temperature, not because the materials ceased to glow but on account of the softening of the glass of their vacuum tubes.

The main facts about this luminescence at high temperatures, the details concerning which will be given in a series of forthcoming papers, may be briefly stated as follows:

1. *The glow is produced by a new type of excitation*, i. e., by contact with the outer zone of the hydrogen flame; also in some instances by kathode bombardment and the action of X-rays.

2. *The effect is quite distinct from photo-luminescence,* from which it differs in the following respects:

(*a*) It is observable in many substances incapable of excitation by light; such as the oxides of calcium, zinc, magnesium, aluminum, silicon and zirconium; and in sulphides of calcium and zinc of such purity as to be non-phosphorescent.

(*b*) It is absent in some strongly fluorescent materials such as willemite and calcium tungstate.

(*c*) The addition of a trace of some activating substance, as in the production of the phosphorescent sulphides, is not necessary. Such an admixture sometimes modifies the color of the glow but more frequently prevents fluorescence altogether. Thus many of the Lenard and Klatt sulphides and the well-known, red glowing, cadmium phosphate of Andrews are inert under the excitation of the flame.

(*d*) Where photo-fluorescent substances are active, as in Sidot blende, Balmain's paint and several of the phosphorescent sulphides, the luminescence has an upper limit of temperature far above that at which photo-excitation ceases.

3. *Distinguishing features in relation to temperature radiation.*—The effect often occurs at temperatures far below the red heat. When superimposed upon the red heat it is readily distinguishable:

(*a*) *By its color,* which is more often green, blue or white than ruddy.

(*b*) *By its law of decay,* which is that of vanishing phosphorescence instead of the law of cooling.

(*c*) *By its spectrum,* which is a characteristic broad banded fluorescence spectrum often lacking altogether in the red. The bands of these spectra are made up of over-lapping components which form series with the usual uniform frequency intervals.

4. *Limits of Activity.*—The upper limiting temperature (or temperature of extinction) differs with various substances. It is, for example, about:

690° C. for calcium oxide,
740° C. for calcium sulphide,
940° C. for zinc oxide and pure zinc sulphide.

The lower limit also varies through a very wide range; e. g., from
52° C. for aluminum oxide to
568° C. for the blue-green band of zinc oxide.

These limiting temperatures are in some cases very sharply defined.

5. *Individual excitation of bands.*—The over-lapping bands of the spectrum are separately excited and each has its particular active range of temperatures. As these differ, color changes with temperature result. In some cases the temperature ranges do not over-lap, so that two or even three fluorescences follow one another as the temperature rises. Thus zirconium oxide shows a bluish white fluorescence between 76° C. and 372°, followed by deep red between 440° and 720°.

6. *Kathodo-luminescence at high temperatures.*—Many of the substances which exhibit flame excitation are also luminescent in the kathode tube, and sometimes we find the same bands brought out and the same upper temperature of extinction.

Thus calcium oxide becomes extinct at about 690°, whether excited by the hydrogen flame or by kathode bombardment.

Sometimes the two types of excitation give strikingly different appearances, as in the case of the synthetic ruby, which shows the same pale green band as other forms of aluminum oxide in the flame but the well known brilliant red in the vacuum tube. Our observations indicate that under kathodic action also the bands are independently excited and that bands of shorter wave-length have a higher temperature range: Thus the color shift towards violet with heating recorded by Crookes and later systematically studied by Wiedemann and Schmidt (*l. c.*) is confirmed and explained.

7. Excitation by means of X-rays at these temperatures has thus far been tried only with a few substances. It was found, however, that calcium oxide, calcium carbonate, sidot blende and some of the phosphorescent sulphides which are subject to flame excitation respond also

to X-rays at temperatures up to a dull red heat. There is usually color change as mentioned under (6) and a distinct maximum of brightness at some intermediate temperature.

[1] In this statement reference is to *established* cases of luminescence. Exception is made of the rather numerous instances in which radiation of an unusual character is *ascribed* to luminescence without actual demonstration of the fact. The present author more than once has made such assumptions and believes them likely to be verified.

[2] Lenard and Klatt, *Ann. Physik, Leipzig,* **15,** 1904 (425).

[3] Wiedemann and Schmidt, *Ibid.*, **56,** 1895 (218).

FUNCTIONAL REGULATIONS IN ANIMALS WITH COMPOSITE SPINAL CORDS

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Communicated by R. G. Harrison, October 7, 1920

In several recent series of experiments,[1] involving the transplantation of the forelimb in Amblystoma embryos, a number of points were brought out by the author bearing upon the question of the functional readjustment of the peripheral nervous system in response to the altered conditions. The cardinal points may be summarized as follows:

(1) When the limb is transplanted to an abnormal (heterotopic) position, there is a striking tendency for its innervation to be derived from the original limb level of the cord, and only when this is totally or in part accomplished, is the limb capable of movements which are well coördinated with those of the opposite intact appendage.

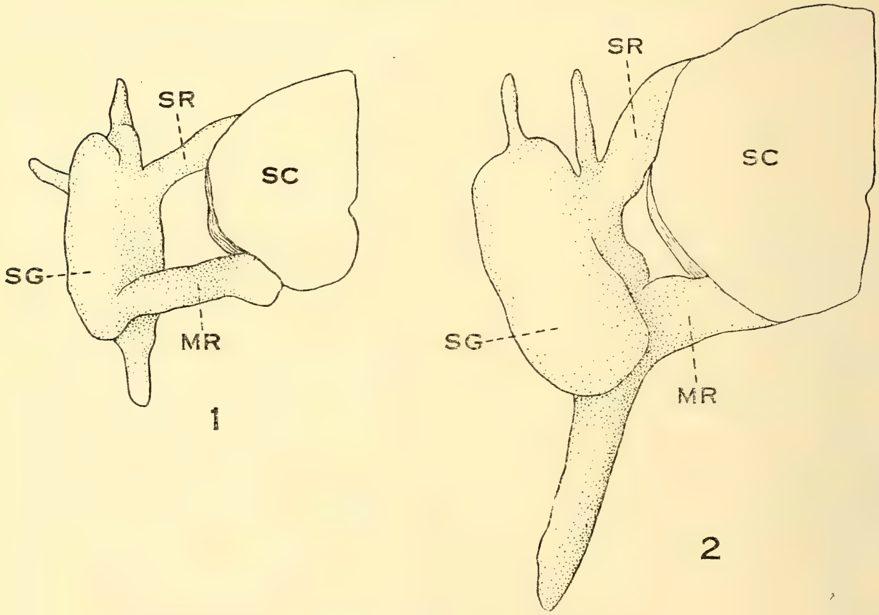
(2) Transplanted limbs which are not supplied by the normal limb nerves exhibit a degree of function which is directly correlated with the region of the spinal cord from which the nerves are derived. The motility of the appendage is more perfect when its nerves come from segments of the cord adjoining those which contribute the normal limb nerves.

(3) The gradual loss of function as limbs are transplanted further and further away from the normal situation is attributed to increased defective connections within the central nervous system and not to a corresponding decrease in effective peripheral efferent innervation nor to deficiencies in in the skeleto-muscular mechanism of the limb and the shoulder girdle.

(4) Peripheral nerves supplying a transplanted limb are larger than their counterparts which have no connection with a limb. Critical examination has shown this enlargement to be due to a hyperplasia of the afferent neurones with no evident over-production of cells on the motor side.

The fact that structurally complete transplanted limbs with an adequate peripheral nervous mechanism, derived from the extra-limb level of the cord, are capable of adaptive movements led to the inference above given, namely, that the defective motility is due to a corresponding inadequacy of central connections and not to any rigid specific relation between certain nerves and a given group of muscles. Further, the general failure of the motor centres to undergo hyperplastic regulation such as occurred in the sensory centres under the conditions of increased functional demands at the periphery, led to the view that factors other than the peripheral requirements must be sought for in the solution of motor readjustment.

In the light of these considerations, the experiments which are herewith briefly reported were carried out. They consisted in removing that portion of the spinal cord from which the normal brachial plexus is derived (third, fourth and fifth segmental nerves) and transplanting into the excised area a more posterior portion of the spinal cord of another embryo that normally gives origin to the seventh, eighth and ninth segmental nerves. These latter nerves, in their normal position, are capable of producing but very limited movements when innervating transplanted limbs.[2] The limb rudiments were left intact.



Explanation of Figures

(1) Reconstruction model showing the sensory (sr) and motor (mr) roots of the right eighth spinal nerve, the spinal ganglion (sg), and the corresponding level of the spinal cord (sc) × 180. Nerve not connected with limb. Case TrSc 137.

(2) Reconstruction model showing the enlarged senso-motor apparatus of the right fourth spinal nerve (originally eighth of another animal and transplanted into the limb level of the host) × 80. Nerve connected with limb. Case TrSc 137.

In the present experiments the grafted unit of spinal cord, carrying with it the neuroblastic forerunners of the seventh, eighth and ninth peripheral nerves, is subjected to all the factors which normally produce the increased proliferation of nerve cells in the limb region, and the resultant degree of development of its neurone components under these conditions can be compared directly with the undisturbed seventh, eighth and ninth segments of the host.

The experiments were carried out on Amblystoma embryos ranging in age from those with completely closed medullary folds (stage 21) up to those with a prominent tail bud (stage 33), a stage just preceding that in which primary reactions first appear. Although the survival of the embryo appears to have no correlation with its age at the time of the operation, the percentage of normal results, both structural and functional, is clearly a function of the age of the embryo and the most normal results were obtained from embryos in the stage of closed neutral folds (table 1). The operated animals were reared in individual aquaria and the majority were kept under daily observations for from 60 to 75 days.

The effects of the transplantation upon the development of the embryo, the initial swimming reactions, and the functional behavior of the limbs are given in table 1. The bearing of these experiments upon the question of healing and regeneration of the spinal cord,[3] and upon the development of the primary swimming reflexes in the light of the work of Herrick and Coghill,[4] will be considered later.

The observations made on the living operated animals showed that the limbs in fifty per cent of the cases performed normal adaptive and coördinated movements (table 1). The movements of the remainder were partially impaired. The defects in these consisted mainly of (1) imperfect flexion of both arm and forearm, and (2) extensor paralysis of the wrist. Only two animals of the series have been sectioned thus far (cases TrSc 90 and TrSc 137), in both of which the limbs functioned normally. A microscopic study of sections from these animals showed the presence of a perfectly developed brachial plexus with normal intrinsic nerve distribution, derived from the grafted portion of the spinal cord (seventh, eighth and ninth segments). Not only were the transplanted segmental nerves found to be larger than the undisturbed seventh, eighth and ninth nerves of the host, with which they can be directly compared, but also the cord itself and the spinal ganglia showed distinct hyperplasia of the nerve cells (table 3, A2 and B2), thus indicating that the hypertropic development was due to excessive proliferation of the neuroblasts rather than to a compensating increase in the volume of a specific number of cell bodies and their axones.

The increased development of the motor and sensory contributions to the fourth nerve (originally the eighth) is shown in figure 2 and also in table 2. In the latter are given the weights and the weight ratios of unas-

TABLE 1

Showing the Effects of Removal of the Forelimb Level of the Spinal Cord (3, 4 and 5 Spinal Segments) and Transplanting into the Excavated Area, a More Posterior Portion of the Cord (7, 8 and 9 Spinal Segments) from Another Embryo

STAGE OF DEV. WHEN OPERATION WAS PERFORMED	NO. OF OPERATIONS	POSITIVE EXPERIMENTS		NORMAL RESULTS		ABNORMAL RESULTS				AV. NO. OF DAYS AFTER OPERATION WHEN ABNORMALTIES WERE FIRST OBSERVED	AV. NO. OF DAYS AFTER OPERATION WHEN REACTION BEGAN	INITIAL REACTION				LIMBS WITH PERFECT FUNCTION		LIMBS WITH IMPERFECT FUNCTION		LIMBS WITHOUT FUNCTION		AV. NO. OF DAYS AFTER OPERATION WHEN LIMB REACTIONS BEGIN
						KYPHOSIS		LORDOSIS				PERFECT		IMPERFECT								
		CASES	%	CASES	%	CASES	%	CASES	%	DAYS	DAYS	CASES	%	CASES	%	CASES	%	CASES	%	CASES	%	DAYS
21–23	40	6	15	4	67	2	33	0	0.0	11	6	9	100	0	0.0							17
25–27	100	14	14	7	50	5	36	2	14	7–8	4	11	73	4	27	10	50	10	50	0	0.0	22
29–33	60	9	15	2	23	7	77	0	0.0	5–6	2–3	6	47	7	53							22–23
		29*																				

* Nine cases of the positive experiments were preserved before the period when limb reactions normally begin.

TABLE 2

SHOWING THE RELATIVE WEIGHTS OF THE SENSORY AND MOTOR COMPONENTS OF THE 4TH AND 8TH SEGMENTAL NERVES

(Weight of Model in Grams)

	SENSORY ROOT	MOTOR ROOT	SPINAL GANGLION	SPINAL CORD (140 SECTIONS)	
				TOTAL	CELL AREA
Fourth nerve* (connected with limb)...	1.3	4.3	14.5	209.6	97.3
Eighth nerve (not connected with limb....	0.3	1.1	5.1	121.5	50.9
Weight ratios........	3.5	3.7	2.8	1.7	1.9

* The fourth nerve was developed from a transplanted portion of the spinal cord which normally gives rise to the eighth nerve.

TABLE 3

SHOWING THE NUMBER OF CELLS COUNTED IN (*a*) 10 CONSECUTIVE TRANSVERSE SECTIONS IN THE RIGHT HALF OF THE SPINAL CORD ANTERIOR TO THE EXIT OF THE 4TH AND 8TH NERVES, AND (*b*) IN THE 4TH AND THE 8TH RIGHT SPINAL GANGLIA

	CONDITIONS	LEVEL OF 4TH NERVE (CONNECTED WITH LIMB)	LEVEL OF 8TH NERVE (NOT CONNECTED WITH LIMB)	RATIOS
A	(1) Normal.............	1750	930	1.881
	(2) 7, 8 and 9th spinal segments substituted for the 3, 4 and 5th..	1300	824	1.577
	(3) Limb excised (spinal cord intact).........	1580	890	1.775
		4TH RT. SPINAL GANGLION	8TH RIGHT SPINAL GANGLION	
B	(1) Normal.............	1430	685	2.087
	(2) As in A2............	1860	945	1.968
	(3) As in A3...........	685	720	0.951

sembled blotting paper models of the component parts of both fourth and eighth nerves, the former of which is in connection with the limb. Both motor and sensory roots of the fourth nerve (transplanted eighth) have undergone striking enlargement under the conditions which normally produce the increased proliferation of those centres in the limb region.

The hyperplastic development of the sensory centres, secondarily brought into connection with a transplanted limb, has been shown in recent experiments,[5] although no over-production of motor cells could be found. Under the conditions of the present experiments the nerve cells in the transplanted portion of the cord, which has been subjected to the factors normally producing the brachial enlargement, have undergone a degree of development, as indicated by a cell count, which almost equals that of the normal (table 3A, 1 and 2). The hyperplasia of the motor cells, however, under these conditions is evidently not the result of the functional activity of the limb with which the nerve is connected, for in the absence of the limb the production of cells in the limb level of the spinal cord is almost as great as under normal conditions (table 3A, 1 and 3).

The possibility that the ingrowth of the axones of a given number of peripheral afferent neurones might determine the extent to which the motor centres will develop, is negated by the facts obtained from previous limb experiments,[6] which show that a certain degree of both hypoplastic and hyperplastic development of the sensory neurones in a given reflex pathway can be induced experimentally without effecting a corresponding measure of development on the motor side.

The evidence thus far obtained indicates, therefore, that the factor which is involved in the over-production of the motor cells is the stimulus afforded by the connection with the central neurones (bulbo-spinal fibers), a large number of which very likely run only as far as the limb level where they discharge into the appendicular motor centres.

This preliminary report will be followed by a detailed account of the experiments, as well as a discussion of the results bearing more particularly upon the factors involved in the growth phase of nervous development.

[1] Detwiler, S. R., (*a*) These PROCEEDINGS, **5**, 1919 (324–331); (*b*) *Ibid.*, **6**, 1920 (96–101); (*c*) *J. Exp. Zoöl.*, **31**, 1920 (117–169).

[2] See 1 (*c*) pp. 142–147, series AS5 and AS6.

[3] An account of healing and regeneration of the spinal cord in frog embryos is given, by Hooker, D., in the following papers: (*a*) *J. Comp. Neur.*, **25**, 1915 (469–495); (*b*) **27**, 1917 (421–449).

[4] Herrick, C. J., and Coghill, G. E., "The Development of Reflex Mechanisms in Amblystoma," *J. Comp. Neur.*, **25**, 1915 (65–85).

[5] See 1 (*b*).

[6] See 1 (*b*), pp. 96–98.

EXPERIMENTS WITH THE TUBE RESISTANCE FURNACE ON THE EFFECT OF POTENTIAL DIFFERENCE

By Arthur S. King

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Communicated by George E. Hale, October 12, 1920

The form of electric furnace which has been found most effective in spectroscopic work is the tube resistance type, in which the substance to be vaporized is placed in a graphite tube heated by a current, the electrodes being applied at the ends of the tube. A column of vapor of almost uniform temperature is thus obtained in a considerable length of the tube. The temperature can be closely controlled and raised until the vaporization of the tube material becomes very violent, in the neighborhood of 3000° C. Regulated in this way, the furnace has uniformly emitted a definite spectrum of the enclosed vapor at a given temperature, and much evidence has been gathered which indicates a high degree of independence of chemical action arising from the use of various compounds or the presence of different gases in the furnace chamber, such actions appearing to affect the general intensity of a spectrum without materially modifying its character.

Since an important feature of the furnace is the excitation of the enclosed vapor by the high temperature of the tube rather than by the passage of a current as in the case of the arc, any approach to arc conditions which may result from the mass of ionized vapors being exposed to the potential difference of the ends of the tube must be carefully taken into account. It is obvious that a certain amount of such conduction may take place, in spite of the low potentials employed and the very small resistance of the graphite tube. Experiments have therefore been carried out to see whether, at least for the temperatures which usually bring out the important features of furnace spectra, a reduction or elimination of the potential difference by special means would cause any deviation from the spectrum observed at the same temperature with the regular operation of the furnace. For this purpose the tube was charged with iron, and certain lines very sensitive to change of temperature and especially strong in the arc were taken as a test spectrum. The bands given by the vaporization of the carbon tube and the lines of titanium and vanadium resulting from impurities were noted for the same purpose.

A reduction of the potential difference was obtained by two methods: first, by the use of direct current, which avoided the high momentary values of the alternating voltage usually employed; and, second, by using an insulated tube to contain the luminous vapor, inside the tube carrying the current, this inner tube furnishing an extra low-resistance path for any current not carried by the furnace tube. In each case, the test spectrum was the same at a given temperature, showing no effect of the reduced possibility of conduction by the vapor. With the first method, an extra protection of the tube from loss of heat permitted the observation of the test spectrum with a potential difference of less than 0.6 volt per centimeter length of the tube.

Two experiments in which there was no potential difference on the tube were next tried. In one, the tube was heated, the current broken, and the exposure made during an interval while the tube cooled with no current. The spectrum was found to be the same as when the furnace was operated in the regular way at the mean temperature of the no-current experiment. In the other test, a tube containing iron was heated by a high-current arc between horizontal electrodes supported beneath the tube. The vapor in the tube used as a crucible in this way emitted a spectrum in which the test lines appeared as in the resistance tube carrying a current.

The conclusion from this series of experiments is that for the temperatures required in the line classification which the writer has carried out for a number of metallic spectra, the potential difference acting on the tube is not effective in modifying the spectrum, the lines sensitive to arc conditions appearing with equal ease whether a voltage is acting on the tube or not. Higher temperatures, accompanied by increase of both ionization and potential drop, should be checked when possible as to the effect of conduction by the vapor. Lines brought out only at these higher temperatures are, however, usually so faint that they are of little importance in the furnace spectrum. For a study involving these lines, the arc would usually be employed.

AN APPLICATION OF THE PORISM OF FOUR TANGENTS OF A TWISTED CUBIC

By J. H. McDonald

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Communicated by E. H. Moore, October 25, 1920

The problem of determining the involutions of degree n of a given discriminant has not been solved except when $n = 2,3,4$.

Special forms of discriminant occur in Jacobi's transformation of elliptic integrals and in researches on reducible integrals. Geometrically the problem is to construct a rational curve in n-space of degree n to touch $2(n - 1)$ hyperplanes of a pencil. For $n = 3$ the solution depends on a certain porism;[1] that of four tangents of a twisted cubic. There is in fact the theorem: No proper twisted cubic can be drawn to touch four lines unless they satisfy a condition in which case an infinity of cubics can be drawn. Let *abcd* be four tangents to a twisted cubic meeting their transversals l and l' in $ABCD$, $A'B'C'D'$ and let AA', CC' touch the cubic at points of parameters o, ∞ and BB', DD' at points of parameters θ, φ. It is found that the equations of AA' are $x = y = 0$, of CC' $z = w = 0$, of BB' $x - 2\theta y + \theta^2 z = y - 2\theta z + \theta^2 w = 0$, of DD' $x - 2\varphi y + \varphi^2 z = y - 2\varphi z + \varphi^2 w = 0$. The equations of l are $x - \kappa y = 3z - \lambda w = 0$, of l' $x - \lambda y = 3z - kw = 0$, where k, λ are connected with θ, φ by the equations $\theta + \varphi = \kappa + \lambda/2$, $\theta\varphi = {}^1/_3\, \kappa/\lambda$. Putting $m = \{ABCD\}$, $n = \{A'B'C'D'\}$ the cross-ratios are connected by an equation. The values of m and n are

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[1] Dixon, A. C., on twisted cubics which fulfil certain conditions, *Quar. J. Math.*, **23,** 1889 (343–357).

$$\frac{\varphi}{\theta} \cdot \frac{3\theta - 2\lambda}{3y - 2\lambda}, \quad \frac{\theta}{\varphi} \cdot \frac{3\theta - 2\kappa}{3\varphi - 2\kappa}\,;$$

if $\theta/\varphi = t$ it is found that $mn = t^4$, $m + n = t^4 + 1 - (t - 1)^4$.

Eliminating t; $m^{\frac{1}{2}} - n^{\frac{1}{2}} = 2m^{\frac{1}{8}}n^{\frac{1}{8}}(1 - m^{\frac{1}{4}}n^{\frac{1}{4}})$. This is the poristic condition or if the ranges on the transversals of four lines satisfy this condition an infinity of cubics can be drawn to touch them. The modular equation for the cubic transformation appears here.

To determine the involutions of cubic forms of a given discriminant, let $\alpha\beta\gamma\delta$ be four planes of a pencil of axis l; in $\alpha\beta\gamma$ take lines *abc* arbitrarily (distinct from l) meeting l in distinct points ABC. The cubics tangent to *abc* and the plane δ are of multiplicity ∞^2 being subject to 10 conditions. Let d be the line in δ tangent to a cubic and D its intersection with l and l' the second transversal of *abcd*. If m n have the same meaning as before n is given being the cross-ratio of the pencil $\alpha\beta\gamma\delta$ and m has four values. D has four positions and the lines d form four pencils.

To see that to a value of n correspond only four values of m it suffices to notice that the equation between m and n results from the elimination of t between $mn = t^4$, $m + n = t^4 + 1 - (t - 1)^4$. Considering n as given and eliminating m there results an equation of degree four for t and so four values of m are found.

The final result may be stated in this way. Let $ABCD$ be any four points on the axis of a pencil of planes $\alpha\beta\gamma\delta$ and suppose $\{ABCD\}$ $\{\alpha\beta\gamma\delta\}$ connected by the modular equation; then any four lines, one through each point to lie in the corresponding plane, are tangent to an infinity of twisted cubics.

There are four inequivalent involutions determined on these cubics by the pencil of planes, and n is the invariant of their discriminant. Their construction is as follows: Given n, determine m by the modular equation. From $\theta + \varphi = {}^1/_2(\kappa + \lambda)$, $\theta\varphi = {}^1/_3\kappa\lambda$ and

$$m = \frac{\theta}{\varphi} \cdot \frac{3\theta - 2\lambda}{3\varphi - 2\lambda}$$

$\kappa\lambda$ are determined uniquely. For $mn = t^4$, $m + n = t^4 + 1 - (t - 1)^4$

regarded as equations in t have a unique common solution, φ may be arbitrarily taken and $\theta = t\varphi$. The equations of l are $x - \kappa y = 3z - \lambda w = 0$ and the cubic is $x = \sigma^3$, $y = \sigma^2$, $z = \sigma$, $w = 1$; or finally the involution is $\sigma^3 - \kappa\sigma^2 + \mu(3\sigma - \lambda)$ a single one determined by each value of m.

Putting $X = x - \kappa y$, $Y = x - \lambda y$, $Z = 3z - \lambda w$, $W = 3z - \kappa w$ the planes $X = 0$, $Y = 0$, $Z = 0$, $W = 0$ are the faces of the tetrahedron whose edges are a, c, l, l'. The collineations ($X' = \rho_1 X$, $Y' = \rho_2 Y$, $Z' = \rho_1 Z$, $W' = \rho_2 W$) leave invariant the lines $abcdll'$ evidently but transform any cubic tangent to $abcd$ into an infinity of others.

The coincidence of the equation between m and n with the modular equation leads to this geometrical theorem. If four planes of a pencil of axis l touch a cubic and the tangents meet l in $ABCD$ and if the planes meet the cubic in simple intersections $A'B'C'D'$ then $\{A'B'C'D'\} = \{ABCD\}$.

STUDIES ON THE TRANSFORMATION OF THE INTESTINAL FLORA, WITH SPECIAL REFERENCE TO THE IMPLANTATION OF BACILLUS ACIDOPHILUS, II. FEEDING EXPERIMENTS ON MAN

By Harry A. Cheplin and Leo F. Rettger

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Communicated by L. B. Mendel, October 20, 1920

In a recent communication in these Proceedings 6, 1920, pp. 423–426 the results of feeding experiments with albino rats were presented. Similar attempts to implant *Bacillus acidophilus* in man have led to the following conclusions.

Thorough implantation may be effected by the daily administration of 300–400 grams of lactose, 300 cubic centimeters of a whey broth culture of *B. acidophilus* (McFarland nephelometer turbidity scale of 5), or by a combination of 150 grams of lactose and 150 cc. of the whey broth culture. Complete transformation of bacterial types may be brought about also by the daily use of 500–1000 cc. of acidophilus milk culture.

The same results are obtained with dextrin as with lactose, but dextrose, sucrose and maltose exert no flora-transforming influence.

The time required for changing the flora varies with the nature and the amounts of the material administered. The most rapid implantation was obtained with the *Bacillus adophilus* milk. In two of the subjects 500 cc. of the milk brought about marked alteration within two to three days. Some of the subjects that were but slightly affected by 500 cc. of the milk culture showed very pronounced transformation when 100 grams of lactose were added daily to this amount.

No change could be detected in the hydrogen ion concentration of the feces during the experimental periods. The transformation of the intestinal flora from the usual mixed to the simple aciduric type cannot be ascribed, therefore, to increased acidity within the lumen of the intestine.

The presence of a reducing substance in the feces of subjects receiving transforming amounts of lactose is further evidence in support of the view that the lactose is at least in part carried into the large intestine and helps to create in it an optimum environment for the development of *B. acidophilus.*

Attempts to implant *Bacillus bulgaricus* in the intestine of man failed in every instance. The conclusions arrived at in the rat feeding experiments are borne out here.

Seventeen human subjects were employed in a total of 45 experiments. All but two of these subjects were apparently normal individuals. No restrictions were made in the daily diets, which continued essentially the same as before the investigation.

B. acidophilus milk possesses several advantages over ordinary sour and *B. bulgaricus* milk, aside from its property of transforming the intestinal flora. It is pleasing to the taste and smell. It does not become very acid or in any other way materially change its character even on long standing. The curd is soft and of a creamy consistency. The creamy appearance of the milk persists without appreciable wheying off. The product is prepared easily when proper precautions are taken. Pure strains of *B. acidophilus* must be employed which have been grown in milk sufficiently long to produce acidity and a soft curd within 24 hours when kept at a temperature of 35–37° C.

The same methods were employed in the routine examination of the feces as in the earlier experiments with rats, namely, the preparation and use of whey agar plates, Veillon tubes and Gram-stained slides. The plates and the Veillon tubes were particularly valuable and are strongly commended for work of this kind. The Veillon tubes aided not only in the detection of *B. acidophilus* colonies, but served admirably as an index of the disappearance of *B. coli* and other gas-producing bacteria from the intestine. It soon became evident that as the aciduric type of organisms materially increased in numbers the volume of gas in the tubes correspondingly decreased. A so-called complete transformation of flora was evidenced by the crowding of the whey agar plates with small fluffy colonies, the absence of gas from the Veillon tubes, as well as complete preponderance of the characteristic sea-urchin-like colonies throughout the tubes, and by the large numbers of Gram-positive rod-shaped organisms of the *B. acidophilus* type on the stained slides.

A full account of the authors' studies on the transformation of intestinal flora and the implantation of *Bacillus acidophilus* is now in the Yale University Press, and will appear in book form at an early date.

THE KILAUEA VOLCANO OBSERVATORY

BY A COMMITTEE OF THE NATIONAL ACADEMY OF SCIENCES

PREPARED AT THE REQUEST OF THE SECRETARY OF AGRICULTURE

Communicated by C. D. Walcott, October 8, 1920

February 11, 1920.

DR. CHARLES D. WALCOTT, *President*,
The National Academy of Sciences.

DEAR SIR:

The Committee appointed by you to respond to a request from the Secretary of Agriculture for the advice of the National Academy of Sciences in regard to the conduct, by the Weather Bureau, of the Kilauea Volcano Observatory on the island of Hawaii, submits the following report. This Committee consists of Whitman Cross, Chairman, William Bowie, Arthur L. Day, Herbert E. Gregory, and Harry Fielding Reid. The letter of the Secretary of Agriculture indicating the scope of the subject on which the assistance of the Academy is desired is as follows:

DEPARTMENT OF AGRICULTURE, WASHINGTON,
January 18, 1919.

DR. CHARLES D. WALCOTT, *President*,
National Academy of Sciences,
Washington, D. C.

DEAR DOCTOR WALCOTT:

May I not request the National Academy of Sciences to give the Department the benefit of its advice with reference to scientific questions involved in the conduct of investigations in seismology and volcanology, to be carried on principally at the Kilauea Volcano, Hawaii, and elsewhere as the circumstances may warrant? The Congress, at its last session, appropriated $10,000 for this purpose, and it apparently had in mind the transfer to Government support and direction of the investigations now under way at this volcano, under the supervision of Dr. T. A. Jaggar, which are supported by the Hawaiian Volcano Research Association. The Department, through the Weather Bureau, desires to inaugurate the work promptly and efficiently and to secure the best contributions to the science of volcanic and seismic actions that may be possible within the limited appropriation available. If the representatives of the Academy of Sciences designated to consider this problem desire further details relative to the questions involved, the Chief of the Weather Bureau will be very glad to coöperate with them in every feasible way. I shall appreciate it if you will give the matter your early and favorable consideration.

Very truly yours,
D. F. HOUSTON, *Secretary*.

Soon after the appointment of the Committee it secured from Prof. C. F. Marvin, Chief of the Weather Bureau, a full statement in regard to the work of the Kilauea Observatory and the circumstances of its transfer from the Hawaiian Volcano Research Association. The Committee is much indebted to Professor Marvin for his cordial coöperation at all times.

The request of the Secretary of Agriculture pertains on the one hand to the immediate conduct of the Kilauea Observatory under existing appropriations, and on the other to the general subject of investigations in seismology and volcanology under the auspices of the Weather Bureau which may be conducted elsewhere than at Kilauea if circumstances warrant. As prompt action was desirable regarding the observatory, while the broader subject required further consideration, the Committee submitted to you under date of February 12, 1919, a preliminary report with recommendation as follows:

> The Committee recognizes the importance of the work hitherto accomplished at the Kilauea Observatory under the auspices of the Hawaiian Volcano Research Association and the direction of Dr. T. A. Jaggar, Jr. It also recognizes the great importance of maintaining continuity in the observations and records of the Observatory. This continuity appears to be endangered and the Committee earnestly recommends that the Department of Agriculture, through the Weather Bureau, should immediately take control of the Hawaiian Volcano Research Association equipment and provide for the uninterrupted continuance of the present program of observations and records until opportunity has been given for a thorough study of the opportunities for research in volcanism and seismology in this and adjacent regions, and the possible agencies through which a concordant and effective program can be carried out. The agreement already reached between the Department and the Hawaiian Volcano Research Association appears to furnish an appropriate basis upon which the Weather Bureau can assume control of the Kilauea Observatory.
>
> The broad request of the Secretary of Agriculture for advice as to the scientific problems involved in the conduct of the work in question, in order to secure the best results possible under the available appropriation by Congress, requires more mature consideration on the part of the Committee. It will give careful attention to this matter and submit its full report as soon as possible.

This report was transmitted by you to the Secretary of Agriculture, and in accordance with its recommendation the control of the Kilauea Observatory was at once assumed by the Weather Bureau and has been maintained under the general plan of its earlier administration. The Committee now presents its complete report and recommendation on the subjects outlined in the Secretary's letter to you, giving particular attention to the broader aspects of researches in seismology and volcanology and the larger agencies devoted to promoting them.

I. *Scientific Questions Involved in the Conduct of Investigations in Seismology and Volcanology.*—Seismology is the science of the causes and effects of earthquakes and the phenomena of their occurrence. Volcanology deals with the causes and phenomena of volcanic action and the character of the volcano itself. Both sciences treat phenomena arising from disturbances of more or less deep-seated origin in the lithosphere of the earth and are fundamentally branches of physical geology. Both belong to the group of the earth sciences, all of which are more or less intimately connected.

In seismology the immediate subject of investigation is the shock, heavy or light, which is transmitted from its sources through rocks to the place of observation on the earth's surface. The propagation of this wave-like movement is a matter for physical and mathematical discussion but the shock must also be studied in regard to its origin, the geological conditions under which it arises, the character of the material through which it is transmitted, and the conditions under which divers catastrophic phenomena may be exhibited at the surface.

Nearly all earthquake shocks are known to be due to relief or strain in the rocks of the earth at various depths below the surface. Such relief sometimes occurs in fault zones in the rocks where the geologist has recognized that movements have also taken place in past epochs. Other earthquakes are associated with volcanic disturbances. The source of many earth tremors cannot at present be determined.

Volcanology deals most directly with the physical and chemical phenomena of activity at the vent. However, a volcano represents but one local, though important, phase of eruptive activity. Its lavas are often but a small part of the igneous matter involved in the movement, other portions consolidating beneath the surface or pouring out from fissures as lava floods which may not be demonstrably connected with a typical volcanic centre. Volcanoes are of many types of activity dependent upon the chemical character of the magma involved, including the associated gases, and on differing physical conditions. A volcano is a natural laboratory where the geologists, physicists, chemists, mineralogists, petrographers, seismologists, and other specialists have a most important opportunity to study a great many processes in operation which have been active during the entire history of the earth from the time of its oldest known rocks. The geologist is vitally interested in volcanic phenomena as he is in all current processes whose effects are visible, though perhaps not clearly comprehensible, in the older rocks. The study of extinct and more or less deeply dissected volcanoes of past ages and the theoretical discussions of igneous earth history point to various phases of volcanic activity as of particular importance. This information has an important bearing on the plan of investigation to be carried out at a volcano observatory.

The consideration of physical processes in special relation to the development of the earth is now commonly called geophysics. By analogy we may also recognize geochemistry. Volcanology is sometimes referred to as a branch of geophysics but it clearly bears an equally intimate relation to geochemistry. The prolonged physical and chemical processes of volcanism result, on the material side, in volcanic rocks, minerals, and the volcano itself. No scheme of volcanological research is complete which does not provide for supplementing the field observations by studies in physical, chemical, and petrological laboratories.

This brief and imperfect summary of the scope and relations of seismology and volcanology is designed to show that the "scientific problems involved in the conduct of investigations" in these fields are many and complex. The coöperation of geologists, physicists, chemists, geodesists, and other specialists with volcanologists and seismologists is necessary both in constructing a plan for such research and in carrying it out.

In the abstract it is plain, we believe, that a general scheme of investigation in either seismology or volcanology is distinctly beyond the proper scope of the Weather Bureau's activities. This Committee cannot recommend that the Weather Bureau should organize special departments of research in these sciences. But there are conditions at the present time which make it both appropriate and fortunate that the Weather Bureau should assume the conduct of observation at the Kilauea Observatory. These modifying conditions will now be reviewed.

From the beginning of human history earthquakes and volcanic eruptions with their associated tidal waves have been perhaps the most awe-inspiring and terribly devastating manifestations of natural forces. They have occurred at irregular intervals, unexpectedly as a rule, and in widely separated parts of the world. Owing to long prevailing ignorance or crude ideas as to the causes of these phenomena, and to the difficulties in the way of making extended scientific observations concerning them, the sciences of seismology and volcanology are still in a poorly developed state. Science is as yet unable to predict where or when the next great catastrophic exhibition of either earthquake or volcanic activity may be experienced. Our present knowledge is sufficient, however, to show that, as our understanding of the source, the cause, and the nature of these phenomena increases, we shall gain in some measure a power to anticipate violent earthquakes and volcanic eruptions and to guard more and more successfully against their destructive and terrifying results.

It is essential to the future advance of seismology and volcanology that provision should be made to secure extensive and accurate observations and record of the phenomena involved. Studies of this kind must be carried on at the most favorable centres for such work, and according to plans of investigation representing the best information of the time. The extent and complexity of the work requires that it should be under the direction of an organization planned for the purpose. As the earthquakes or volcanic eruptions of to-day may be in the United States, Italy and Japan, those of to-morrow in New Zealand, Africa, or Iceland, and equally scattered at other times, it is plain that the researches in various countries, whatever their auspices, should be conducted on the same general plan, and that a world-wide correlation of data and coöperation in effort is of the greatest importance. In the closing paragraphs of this report is a statement of the reorganization now in progress, of international agencies intended to promote such coöperation.

The development of delicate automatic registering instruments for measuring the intensity and periodicity of earth tremors and analyzing these complex motions into several components, has now progressed so far that a number of recording seismologic stations have been established in the United States by various government bureaus, in educational institutions and in private laboratories. In order that the records of such stations should be of much real value it is necessary, however, that certain conditions should be met. The sites must be selected with good judgment, the instruments should be of the best type, their installation must be satisfactory, and they must be in charge of skilled observers. Further, the efficiency of the seismologic stations in this country would be increased if they were under the supervision of a central coördinating organization. Such an organization does not exist, as yet. It would require, moreover, large financial support to establish the desirable number of special seismologic stations, with observers and adequate equipment, at all desirable points. The stations of the Weather Bureau are widely distributed, at many of them are trained observers capable of caring for seismometers and allied instruments, and it is, therefore, fortunate that Congress has for several years past appropriated a small sum of money which the Weather Bureau is authorized to spend in seismologic work.

It is the opinion of this Committee that, until a permanent special organization is created, the seismologic investigations of the Weather Bureau should be extended by the equipment of additional selected meteorologic stations, with the proper seismometers and other necessary instruments, to be operated by the staff of the Weather Bureau. The Committee, however, believes that the seismologic research of the Weather Bureau, if thus expanded, should be placed under the immediate direction of a trained seismologist, belonging to its corps. If this is inexpedient, it is suggested that the desirable supervision and advice might be secured by coöperation with the American Geophysical Union, connected with the National Research Council. It is to be noted that the Chief of the Weather Bureau and several of its staff are members of this Union, which includes also several of the leading seismologists of the country.

II. *Investigations in Volcanology and Seismology by the Weather Bureau in the Hawaiian Islands.*—Bearing in mind the general considerations as to researches in seismology and volcanology presented in the earlier part of this report, the Committee has reviewed the exceptionally favorable opportunity for obtaining valuable contributions to these sciences in Hawaii through the administration of the Kilauea Volcano Observatory and other studies of volcanic action by the Weather Bureau. The broader relations of such investigations have been considered. It is evident that the supervision of this work should be directed with due regard to general, provincial, and local relations which may now be discussed.

Observations and researches in volcanology and seismology carried out at the Kilauea or any corresponding observatory cannot have their maximum value unless conducted with due appreciation of the more pressing problems requiring solution at the present time and of the work being done in other parts of the world to advance these earth sciences. Upon this basis the work in Hawaii can be most effectively directed, undesirable duplication avoided, and possibilities of fruitful coöperation recognized.

The Hawaiian Islands are one of many elongated groups or chains of volcanic islands scattered through the great Pacific Ocean basin. These islands have all been built up from considerable, and in some cases from great, depths, and some rise to notable heights above the sea. The Hawaiian chain really extends from the island of Hawaii for 1,500 miles to the west northwest far beyond the largest and best-known members of the group. The only active vents of the present time are the volcanoes of Mauna Loa and Kilauea on the island of Hawaii, but minor eruptions have occurred in geologically recent time on all of the principal islands, while submarine disturbances have been noted on the southeast of Hawaii which are supposed to indicate the existence of other vents where new volcanic mountains are being built up.

Kilauea and Mauna Loa are typical volcanoes of the sort formed by repeated eruptions of basaltic lavas, similar to those which have been predominant in the principal eruptions throughout the group and in most other islands of the Pacific Ocean basin. Kilauea, being one of the most frequently active volcanoes in the world, readily accessible, and presenting unusually favorable conditions for study, was naturally chosen as the site of an observatory of volcanic phenomena by the Hawaiian Volcano Research Association, formed by residents of the islands and assisted for a time by research funds donated by the Massachusetts Institute of Technology.

The work of the observatory, organized and directed to the present time by Dr. T. A. Jaggar, Jr., has already resulted in a most important contribution of fundamental data on the phenomena of volcanism, greater, perhaps, than has been obtained by the study of any other volcano. The value of systematic and continuous observation and permanent record has been clearly established. For example, it is only by such sustained and complete records that the facts of periodicity can be determined and an ultimate knowledge of the influences controlling such periodicity may be gained.

The Committee believes that the Weather Bureau can most effectively contribute to the advancement of the sciences of volcanology and seismology, under the present small appropriation, by maintaining the Kilauea Observatory as the major undertaking, with such extension of the work in lines to be suggested, as may be found practicable. The observatory should be regarded, however, as the chief base for researches and observations on all active phases of Hawaiian volcanism, and not for Kilauea alone.

The work at Kilauea involves on the one hand the observation and record of phenomena and on the other certain investigations by the observatory staff or by specialists whose assistance it is practicable to secure. The researches as to gases associated with the magma at eruption and the development of heat through chemical reactions occurring under decrease of pressure and other conditions of approach to the surface are of greatest importance. The results of coöperation in these studies with investigators from the Geophysical Laboratory of the Carnegie Institution of Washington, already secured, illustrate the desirability of continuing such collaboration and promoting similar work in other lines. We understand this to be the commendable policy of the Director of the Observatory.

It is known that there have been marked differences in the chemical composition of the lavas erupted at various times in several of the Hawaiian volcanoes. It is very desirable to know the chemical composition of the magma exhibiting the observed characteristics of eruption and to note the relation of this composition to a great variety of textural and mineral features of the rocks developed on consolidation of the lava. It will be fortunate if the assistance of specialists capable of conducting such researches can be secured by coöperation or otherwise.

Mauna Loa, the giant volcano adjacent to Kilauea, presents many important problems for investigation, both individual and in relation to Kilauea. It represents a different stage in the development of a basaltic volcano. Its eruptions are on a larger scale than those of Kilauea and under somewhat different conditions. It is desirable to compare its phenomena with those of Kilauea, and to determine the genetic and existing connections of these two neighboring vents of similar magmas.

The difficulties of access to the summit crater of Mauna Loa and to the main rift or fault zones on which modern eruptions are most frequent make the investigations highly difficult or impossible at this time. The Committee recommends that special attention be given to the development of trails, the establishment of shelters and camps, and the provision of proper equipment, which will facilitate access to the most important points for observation. It is desirable to establish a systematic investigation of Mauna Loa as soon as possible. It will be impracticable, naturally, to subject this enormous volcano to such constant observation as is feasible at the comparatively diminutive Kilauea, but adequate preparation will permit the securing of very important supplementary or confirmatory data from study of the short-lived but often extensive eruptions of Mauna Loa.

It is to be hoped that this work may be promoted through coöperation of the National Parks Bureau, the Hawaiian Volcano Research Association and other agencies interested in the study of Mauna Loa.

Besides the study of the active volcanoes it is recommended that the Weather Bureau should include in its field of observation all minor phenomena, such as fumaroles, steam vents, hot springs, local seismic shocks, etc., which indicate either the dying out or renewal of volcanic action at other places. A complete record of such activities in all islands of the Hawaiian group should be kept and observations made from time to time. Thus it is reported that fumaroles exist on the southern slopes of Haleakala, the great volcano of eastern Maui, at points not readily accessible. The evidences of relatively very recent eruptions within the crater of Haleakala and the existence of these fumaroles show that the volcanic fires cannot be assumed to be quite extinct, and may possibly flare up at any time. Facilities should be provided for visiting all places where traces of subterranean heat are found.

Seismologic work at Kilauea, or in the immediate vicinity of any active volcano, is mainly an adjunct to the study of that volcano, and it is a part of the work deserving of careful attention. It will also throw light on the nature of so-called "volcanic earthquakes." By adequate instrumental record of shocks originating in the volcanic field it is certain that valuable information of increasing or declining activity may be obtained which will be an important factor in forecasting future eruptions. This would be of special importance as regards eruptions of Mauna Loa. It will be necessary, however, to place seismographs adapted to this work at several places about the mountain in order to locate approximately the point at which an outbreak may be anticipated. If a relatively cheap instrument which can be attended to by a volunteer observer becomes available, this observation of local shocks originating beneath Mauna Loa should be provided for.

The seismographs hitherto in use at the Kilauea Observatory are not of the best models obtainable at the present time. Some of them have been loaned by private institutions or individuals. It is, of course, highly desirable that the observatory should be equipped with its own instruments, of types best suited to the purpose.

The Kilauea Observatory is, manifestly, not the most favorable site for seismometers designed to record earth tremors originating in other parts of the Hawaiian Islands or at still more distant sources. For such instruments an installation at the island of Oahu would be much more suitable. If a study of the seismic disturbances of the Hawaiian group as a whole is to be carried out, it would be necessary to establish secondary stations with appropriate instruments, at selected points on other islands of the group.

III. *General Conditions Affecting Future Direction of Work by the Weather Bureau.*—It was pointed out in Part I of this report that the advancement of volcanology and seismology, especially in the fundamentally important matter of accurate observation of phenomena, would be greatly promoted by systematic coöperation of scientific men of all parts of the world, working at the presumably widely scattered centres of either volcanic or earthquake activity which may be of special importance at any given time. Efforts to establish such coöperative researches in various earth sciences are now being made. The field to be covered is so great that in some places laboratories or observatories are likely to be established where work in several directions can be carried on. As an instance may be cited the observatory at Apia, in Samoa, founded by the Göttingen Academy of Sciences, where data of much value in the fields of seismology, meteorology, terrestrial magnetism, atmospheric electricity and tidal movements have been obtained.

While this committee believes that the Weather Bureau should not undertake to organize an extensive scheme of investigation in volcanology, it is felt that it may well conduct the work of the Kilauea Observatory for the present. It may be pointed out in this connection that meteorological observations of much value to the Weather Bureau are made under the auspices of many private institutions and individuals, as a contribution to the work of the Bureau.

In the course of the development of scientific research it may well be that some large organization will be formed, devoted to the prosecution of researches in volcanology in the United States or in the Pacific basin, which could take over the Kilauea Observatory, to the benefit of science. The desirability of such a transfer may be considered if and when a permanent organization of this kind is formed.

Under existing conditions it appears evident that hearty coöperation between all Governmental Bureaus conducting scientific work in Hawaii will be of great mutual benefit. The Committee ventures to refer directly to the mutual interest of the Weather Bureau and the National Parks Administration in making both Mauna Loa and Haleakala accessible, by constructing trails and shelter or camp accommodations, and in securing good topographic maps by the U. S. Geological Survey, of Kilauea and the two volcanoes just mentioned.

A geological survey of the whole Hawaiian group under either national or territorial auspices would be of great value to the volcanological work of the Weather Bureau. A well-considered coördination of the work of the Kilauea Observatory and of a geological survey should, of course, be made.

The Hawaiian Volcanic Research Association includes a number of generous, broad-minded men who retain an active interest in volcanological studies and it is in a position to take a helpful place in any coöperative scheme. Two other organizations in Honolulu, while not primarily interested in seismological and volcanological research, are devoting their resources largely to the promotion of science in the central Pacific region. One of these, the Bishop Museum, has expressed a desire, through its

Acting Director, to take part in any scientific undertaking which comes within the scope of its deed of trust, and the other, the University of Hawaii, possesses good laboratories and an increasingly strong scientific faculty.

The importance of world-wide correlation of plans for the advancement of the earth sciences has been referred to repeatedly in the course of this discussion. In closing it seems appropriate to call the attention of the Secretary of Agriculture to the developments now in progress aiming to secure the coöperation of agencies in many countries in promoting researches in meteorology, seismology, and volcanology, as well as in various sciences.

During the late war the National Academy of Sciences, at the request of President Wilson, organized the National Research Council to assist in coördinating and developing the research facilities of the United States, first as a measure of national defense and later as an aid in prosecuting the war. The success of this advisory and coöperative body has led to its perpetuation by the Academy, as directed by the President in an executive order of May 11, 1918.

The benefits of similar organization in other countries and of coöperation between the research agencies of different countries have been so fully recognized that an International Research Council has been formed by representatives of central bodies corresponding to the National Research Council, in several of the allied and associated countries.

One of the principal fields of effort for this International Research Council is to secure the formation under its auspices and with desirable unity and coördination of a number of international associations devoted to particular fields of science. These new organizations replace, in some instances, older ones whose operations were suspended during the war and cannot be revived to advantage under existing conditions.

Among these new bodies of international scope is the International Geophysical Union, devoted to a harmonious development of the earth sciences of physical aspect, dealing with many over-lapping problems. This Union has sections of (*a*) Geodesy; (*b*) Seismology; (*c*) Meteorology; (*d*) Terrestrial Magnetism and Electricity; (*e*) Physical Oceanography; (*f*) Volcanology. (A section of Geochemistry has been added since the submission of this Report.) As a part of the plan there will be organized in each country participating in the International Research Council a branch of the Union with sections corresponding to those named. Thus, in the United States, the American Geophysical Union has already been formed, in connection with the National Research Council. Professor Marvin is the Chairman of the Section of Meteorology. The membership of the various sections embraces a considerable number of leading investigators of the country in the special fields involved.

It is therefore possible at the present time to bring projects for the development of seismologic and volcanologic research directly before both national and international bodies qualified to furnish advice and promote a broad coöperation of great value.

Yours respectfully,
Whitman Cross, *Chairman,*
William Bowie,
Arthur L. Day,
H. E. Gregory,
H. Fielding Reid.

ERRATA

P. 215. For $v = \frac{9.\ \epsilon.\ E.\ D}{4\pi.\ \eta.\ l}$ read $v = \frac{q.\ \epsilon.\ E.\ D}{4\pi.\ \eta.\ l}$.

P. 268. In theorem II for "but $B_{\nu\lambda} \equiv 0$" read "but $B_{\nu\lambda}$ not $\equiv 0$"; and in theorem IV for "or $0 \begin{matrix}+\\+\\+\end{matrix}$" read "or not $\equiv 0$".

P. 416. For the denominator of (1) read $2^a p^b$ where $a = \frac{1}{2} p^{n-1}(p-1)^2$, $b = \frac{1}{2} np^{n-2}(p-1)^2 - 1$, and on p. 417 read p^b for the denominator of the second fraction and 2^a for the left side of the next formula.

INDEX

Abbot, C. G. A New Method of Determining the Solar Constant of Radiation, 4.
——. The Larger Opportunities for Research in the Relations of Solar and Terrestrial Radiation, 82.
——. New Observations on the Variability of the Sun, 674.
Absorption spectra (King), 63.
Acid-combining capacity (Cohn), 256.
Adsorption in charcoals (Harkins and Ewing), 49.
Age distribution of a population (Pearl), 427.
Altitude, effect on hemato-respiratory functions (Henderson), 62.
Amblystoma embryos (Detwiler), 695.
American Astronomical Union (Campbell and Stebbins), 349.
Ancestral influence (Laughlin), 235.
Anderson, J. A. Spectra of Explosions, 42.
Anesthetics, effect on cells (Medes and McClendon), 243.
Anthropometry of Siouans, 131.
Antipyretic drugs (Barbour and Herrmann), 136.
Ants, thermokinetics of (Shapley), 204.
Ants, (Shapley), 687.
Armsby, H. P., Fries, J. A., and Braman, W. W. The Carbon Dioxide: Heat Ratio in Cattle, 263.
Atlantis (Matthew), 17.

Babcock, E. B., and Collins, J. F. Interspecific Hybrids in Crepis, 670.
Bacillus Acidophilus (Cheplin and Rettger), 423, 704.
Bacteria, reducing power of (Glaser), 272.
Bailey, I. W. The Formation of the Cell Plate in the Cambium of the Higher Plants, 197.
Baitsell, G. A. The Development of Connective Tissue in the Amphibian Embryo, 77.
Ballistics of rifle (Webster), 289.
Ballistics, interior (Webster), 648.
Ballistic measurements (Thompson *et al.*), 169.
Barbour, H. G., and Hermann, J. B. On the Mechanism of Fever Reduction by Drugs, 136.
Barnett, J. A. Functionals Invariant under One-Parameter Continuous Groups of Transformations in the Space of Continuous Functions, 200.
Bartell, F. E. Anomalous Osmose, 306.
Barus, C. An Example of Torsional Viscous Retrogression, 1.
——. The Torsional Magnetic Energy Absorption of an Iron Conductor, 59.
——. Note on a Pneumatic Method of Measuring Variations of the Acceleration of Gravity, 495.
——. Note on Torsional Measurement of Variations of the Acceleration of Gravity by Interference Methods, 498.
Bateman, H. On a Differential Equation Occurring in Page's Theory of Electromagnetism, 528.
Bauer, L. A. Some of the Chief Problems in Terrestrial Magnetism and Electricity, 572.
Behavior studies on Hermit Crabs (Cowles), 40.
Benedict, F. G. The Basal Metabolism of Boys from 1 to 13 Years of age, 7.
Berry, E. W. Fossil Plants from the Late Cretaceous of Tennessee, 333.
Binaural location of sound (Stewart), 166.
Blackwood, O. The Existence of Homogeneous Groups of Large Ions, 253.
Blood concentration (Underhill *et al.*), 692
Boas, F. The Influence of Environment upon Development, 489.
Bogert, J., see Underhill, F. P., 79.
Botrychium (Campbell), 502.
Bowen, N. L. Differentiation by Deformation, 159.
Bowie, W. Present Status of Geodesy and Some of the Problems of This Branch of Geophysics, 545.
Bowie, W., see Cross, W., 706.
Boys, metabolism of (Benedict), 7.
Braman, W. W., see Armsby, H. P., 263.
Breeding of cattle (Pearl), 225.
Bridges, C. B. The Mutant Crossveinless in Drosophila Melanogaster, 660.
Bridges, C. B., see Morgan, T. H., 162.
Bridgman, P. W. Further Measurements of the Effects of Pressure on Resistance, 505.

γ-Rays (Kovarik), 105.
Calcium metabolism (Underhill *et al.*), 79.
Calendars of Central America (Spinden), 56.
Cambium, cell plates in (Bailey), 197.
Camarasaurus (Osborn and Mook), 15; (Gregory), 16.
Campbell, D. H. The Genus Botrychium and Its Relationships, 502.

CAMPBELL, W. W. Report of the Meetings of the International Research Council and of the Affiliated Unions held at Brussels, July18–28, 1919, 340.
CAMPBELL, W. W., and STEBBINS, J. Report on the Organization of the International Astronomical Union, 349.
Cancer in Mice (Murphy), 35.
Capella, orbit of (Michelson), 474.
Carbon dioxide: heat ratio (Armsby *et al.*), 263.
CASTLE, W. E. Model of the Linkage System of Eleven Second Chromosome Genes of Drosophila, 73.
Cattle, breeding of (Pearl), 225.
Census of U. S. (Pearl and Reed), 275.
Central American Calendars (Spinden), 56.
Cell plate formation (Bailey), 197.
Cervical ganglia (Meltzer), 532.
Charcoal activation (Sheldon), 178.
Charcoals, adsorption in (Harkins and Ewing), 49.
CHEPLIN, H. A., and RETTGER, L. F. Studies on the Transformation of the Intestinal Flora, with Special Reference to the Implantation of Bacillus Acidophilus, I. Feeding Experiments with Albino Rats, 423. II. Feeding Experiments with Man, 704.
Chondrules in Meteorites (Merrill), 449.
Chromosome genes, linkage of (Castle), 73.
Climate, affected by solar radiation (Abbot), 82.
COHN, E. J. The Relation between the Isoelectric Point of a Globulin and Its Solubility and Acid Combining Capacity in Salt Solution, 256.
Cold and plant growth (Coville), 434.
COLLINS, J. F., see Babcock, E. B., 670.
Color index of giant stars (Shapley), 293.
Conduction of electricity (Hall), 139; of heat (Hall), 613.
Conformal mapping (Gronwall), 300, 312.
Connective tissue, development of (Baitsell), 77.
COVILLE, F. V. The Influence of Cold in Stimulating the Growth of Plants, 434.
COWLES, R. P. The Transplanting of Sea Anemones by Hermit Crabs, 40.
Cows, temperature of (Kriss), 539.
CROSS, W., *Chairman.* The Kilauea Volcano Observatory, 706.
Crabs, behavior of (Cowles), 40.
Crossing-over (Detlefsen), 663.
Cubic curves (McDonald), 702.

Dairy cattle (Pearl), 225.
DALY, R. A. A General Sinking of Sea-Level in Recent Time, 246.
DAVIS, H. N., see Shapley, H., 486.
DAY, A. L., see Cross, W., 706.
Deformation, differentiation by (Bowen), 159.
DETLEFSEN, J. A. Is Crossing-Over a Function of Distance? 663.
DETWILER, S. R. On the Hyperplasia of Nerve Centers Resulting from Excessive Peripheral Loading, 96.
——. Functional Regulation in Animals with Composite Spinal Cords, 695.
Development, influence of environment (Boas), 489.
Differential equations, seminvariants of (Stouffer), 645.
Differentiation by deformation (Bowen), 159.
Distribution, geographical, and relationship (Metcalf), 432.
Dinosaur (Osborn and Mook), 15; (Gregory), 16.
Drosophila melanogaster, mutant cross-veinless (Bridges), 660.
Drosophila, linkage in (Castle), 73.
Drosophila, genes in (Metz), 164.
Drosophila virilis, sterility in mutants (Metz), 421.
Drosophila virilis, genes and linkage (Weinstein), 625.
DUANE, W., and PATTERSON, R. A. Characteristic Absorption of X-Rays: L Series, 509.
DUANE, W., and PATTERSON, R. A. On the Relative Positions and Intensities of Lines in X-Ray Spectra, 518.
DUANE, W., and STENSTRÖM, W. On the K Series of X-Rays, 477.
DUANE, W., FRICKE, H., and STENSTRÖM, W. The Absorption of X-Rays by Chemical Elements of High Atomic Numbers, 607.
DURAND, W. F. Shock or Water Ram in Pipe Lines with Imperfect Reflection at the Discharge End and Including the Effects of Friction and Non-Uniform Change of Valve Opening, 441.
DUTCHER, R. A. The Nature and Function of Antineuritic Vitamine, 10.
Dynamics of growth (Reed), 397.

L. P. EISENHART. The Permanent Gravitational Field in the Einstein Theory, 679.
Elastico-viscous flow (Michelson), 122.
Electrolytic solutions (Hitchcock), 186.
Electric conduction (Hall), 139.
Electromagnetism, kinematical interpretation (Page), 115.
Electromagnetic theory (Bateman), 528.
Embryo, connective tissue in (Baitsell), 77.
Encephalitis (Flexner), 103.
Environment and heredity (Wright), 320.
Environment, influence on development (Boas), 489.
EWING, D. T., see Harkins, W. D., 49.
Exostosis, multiple (Underhill *et al.*), 79.
Explosions, spectra of (Anderson), 42.

Eye-defects, transmission of (Guyer and Smith), 135.

Fermat's Theorem (Vandiver), 266, 416.
Fertilization, selective (Jones), 66.
Fertilizer experiments (Lipman and Linhart), 684.
Fever reduction (Barbour and Herrmann), 136.
FLEXNER, S. Encephalitis and Poliomyelitis, 103.
Fluorescence (Nichols and Wilbur), 693.
FOLEY, A. L. A Photographic Method of Finding the Instantaneous Velocity of Sound Waves at Points Near the Source, 310.
Fossil plants (Berry), 333.
FRICKE, H., see Duane, W., 607.
FRIES, J. A., see Armsby, H. P., 263.
Functionals, invariance of (Barnett), 200.
Furnace spectra (King), 63, 65.
Furnace, tube resistance (King), 701.

Ganglia, cervical (Meltzer), 532.
Gas, specific heat (Webster), 303.
Genes, order of (Morgan *et al.*), 162 (Metz) 164.
Geometrical Products (Moore and Phillips), 135.
GLASER, R. W. The Effect of the Concentration of Nitrates on the Reducing Powers of Bacteria, 273.
Globulin, isoelectric point (Cohn), 256.
GODDARD, R. H. The Possibilities of the Rocket in Weather Forecasting, 493.
Gravitational theory (Eisenhart), 679.
Gravity, measurement of (Barus), 495, 498.
GREGORY, H. E., see Cross, W., 706.
GREGORY, W. K. Restoration of Camarasaurus and Life Model, 16.
GRONWALL, T. H. On the Distortion in Conformal Mapping when the Second Coefficient in the Mapping Function has an Assigned Value, 300.
——. Conformal Mapping of a Family of Real Conics on Another, 312.
Groups of operators (Miller), 70.
Groups, structure of (Zeldin), 541.
Growth influenced by cold (Coville), 434.
Guinea-pigs, piebald pattern (Wright), 320.
GUYER, M. F., and SMITH, E. A. Transmission of Eye-Defects Induced in Rabbits by Means of Lens-Sensitized Fowl-Serum, 135.

HALL, E. H. Inferences from the Hypothesis of Dual Electric Conduction; the Thomson Effect, 139.
——. The Thomson Effect and Thermal Conduction in Metals, 613.
HARDY, A. C. A Study of the Persistence of Vision, 221.
HARKINS, W. D., and EWING, D. T. An Apparent High Pressure Due to Adsorption, the Heat of Adsorption and the Density of Gas-Mask Charcoals, 49.
HARKINS, W. D. The Ionization of Strong Electrolytes, 601.
Hawaiian Islands, Weather Bureau (Cross *et al.*), 706.
Heat production in platinum black (Loeb), 107.
HECHT, S. Human Retinal Adaptation, 112.
Hemato-respiratory functions (Henderson), 62.
HENDERSON, Y. The Adjustment to the Barometer of the Hemato-Respiratory Functions in Man, 62.
Heredity and environment (Wright), 320.
Heredity in man (Laughlin), 235.
HERRMANN, J. B., see Barbour, H. G., 136.
HICKMAN, C. N., see Thompson, L. T. E., 169.
High-tension D. C. (Webster), 269.
HILL, A. J., see Johnson, T. B., 290.
HITCHCOCK, F. L. A Thermodynamic Study of Electrolytic Solutions, 186.
HONEIJ, J. A., see Underhill, F. P., 79.
Hormones dependent on vitamines (Dutcher), 10.
Horns, experiments on (Webster), 316.
HOYT, F. C. The Intensities of X-Rays of the L-Series, III. Critical Potentials of the Platinum and Tungsten Lines, 639.
Hybrids in Crepis (Babcock and Collins), 670.
Hydrogen-ion concentration in the intestine (McClendon), 690.
Hydrogen, negative carriers in (Loeb), 435.
Hyperplasia of nerve centers (Detwiler), 96.

Influenza (Underhill *et al.*), 692.
Intensifier for telescope (Shapley), 127.
Interference methods in astronomy (Michelson), 474.
Interferometer, vertical (Michelson), 473.
Interferometric methods (Barus), 498.
International Astronomical Union (Campbell and Stebbins), 349.
International Research Council (Campbell), 340.
Intestinal flora (Cheplin and Rettger), 423, 704.
Intestine, hydrogen-ion concentration in (McClendon), 690.
Invariance of functionals (Barnett), 200.
Involutions on a cubic (McDonald), 702.
Ions, negative, in H_2 and N_2 (Loeb), 435.
Ionization of solutions (Noyes and MacInnes), 18.
Ions, mobility (Blackwood), 253.
Isothiocyanates (Johnson *et al.*), 290.

Jersey cattle (Pearl), 225.
JOHNSON, T. B., HILL, A. J., and KELSEY, E. B. Alkyl Amides of Isothiocyanacetic Acid, 290.

JONES, D. F. Selective Fertilization in Pollen Mixtures, 66.

KELSEY, E. B., see Johnson, T. B., 290.
Kilauea Volcano Observatory (Cross *et al.*), 706.
Kinematical interpretation of electromagnetism (Page), 115.
KING, A. S. A Study of Absorption Spectra with the Electric Furnace, 63.
——. A Study of the Effect of a Magnetic Field on Electric Furnace Spectra, 65.
——. Experiments with the Tube Resistance Furnace on the Effect of Potential Difference, 701.
KLINE, J. R. A New Proof of a Therorem Due to Schoenflies, 529.
KOVARIK, A. F. A Statistical Method for Studying the Radiations from Radioactive Substances and the X-Rays and Its Application to some γ-Ray Problems, 105.
KRISS, M. Observations of the Body Temperature of Dry Cows, 539.

LAUGHLIN, H. H. Calculating Ancestral Influence in Man, 235.
Lens-sensitized serum (Guyer and Smith), 135.
Leprosy (Underhill *et al.*), 79.
Leptospira icteroides (Noguchi), 110.
Light, velocity of (Michelson), 476.
LINHART, G. A. A Simplified Method for the Statistical Interpretation of Experimental Data, 682.
LINHART, G. A., see Lipman, C. B., 684.
Linkage in Drosophila virilis (Weinstein), 625.
LIPKA, J. Motion on a Surface for Any Positional Field of Force, 621.
LIPMAN, C. B., and LINHART, G. A. A Critical Study of Fertilizer Experiments, 684.
LITTLE, C. C. A Note on the Human Sex Ratio, 250.
LITTLEHALES, G. W. The Problems and Functions of the Section of Physical Oceanography of the American Geophysical Union, 580.
LOEB, L. B. On the Nature of the Heat Production in a System of Platinum Black, Alcohol, and Air, 107.
——. On the Nature of the Negative Carriers Produced in Pure Hydrogen and Nitrogen by Photo-Electrons, 435.
LOEB, J. The Influence of Ions on the Osmotic Pressure of Solutions, 211.
LOTKA, A. J. Analytical Note on Certain Rhythmic Relations in Organic Systems, 410.
Luminescence (Nichols and Wilbur), 693.
LUNN, A. C. The Commutativity of One-Parameter Transformation in Real Variables, 24.

MACINNES, D. A., see Noyes, A. A., 18.
Magnetism in iron (Barus), 59.
Magnitudes in clusters (Shapley), 293.
Magnitudes in star clusters (Shapley and Davis), 486.
Maize, fertilization experiments on (Jones) 66.
MARVIN, C. F. The Status and Problems of Meteorology, 561.
MATTHEW, W. D. Plato's Atlantis in Palaeogeography, 17.
MCDONALD, J. H. An Application of the Porism of Four Tangents of a Twisted Cubic, 702.
MCCLENDON, J. F. Hydrogen-Ion Concentration of the Contents of the Small Intestine, 690.
MCCLENDON, J. F., see Medes, G., 243.
MEDES, G., and MCCLENDON, J. F. The Effect of Anesthetics on Lving Cells, 243.
MELTZER, S. J. Are the Superior Cervical Ganglia Indispensable to the Maintenance of Life? 532.
MERRILL, G. P. On Chondrules and Chondritic Structure in Meteorites, 449.
Messier 3, magnitudes in (Shapley and Davis), 486.
Metabolism of boys (Benedict), 7.
Metabolism of cattle (Armsby *et al.*), 263.
METCALF, M. M. Upon an Important Method of Studying Problems of Relationship and of Geographical Distribution, 432.
Meteorites, chondrules in (Merrill), 449.
METZ, C. W. The Arrangement of Genes in Drosophila Virilis, 164.
——. Observations on the Sterility of Mutant Hybrids in Drosophila Virilis, 421.
MICHELSON, A. A. The Laws of Elastico-Viscous Flow, II, 122.
——. The Vertical Interferometer, 473.
——. On the Application of Interference Methods to Astronomical Measurements, 474.
——. A Modification of the Revolving Mirror Method for Measuring the Velocity of Light, 476.
Milk (Cheplin and Rettger), 423, 704.
MILLER, G. A. Groups Generated by Two Operators, s_1, s_2, Which Satisfy the Condition $s_1{}^m = s_2{}^n$, $(s_1s_2)^k = 1$, $s_1s_2 = s_2s_1$, 70.
Mobility of ions (Blackwood), 253.
MOOK, C. C., see Osborn, H. F., 15.
MOORE, C. L. E., and PHILLIPS, H. B. Note Geometrical Products, 135.
MORGAN, T. H., STURTEVANT, A. H., and BRIDGES, C. B. The Evidence for Linear Order of Genes, 162.
MURPHY, J. B. The Effect of Physical Agents on the Resistance of Mice to Cancer, 35.
Mutant hybrids (Metz), 421.

Mutant in Drosophila (Bridges), 660.
Myers, C. N., and Voegtlin, C. The Chemical Isolation of Vitamines, 3.

National Academy of Sciences. Report of the Autumn Meeting, 43.
——. Report of the Annual Meeting, 442.
——. Grants from Trust Funds, 445.
——. Committee Report on the Kilauea Volcano Observatory, 706.
National Research Council. Minutes of Executive Board, 335.
Nerve centers, hyperplasia of (Detwiler), 96.
Nervous system (Detwiler), 695.
Nichols, E. L., and Wilbur, D. T. Luminescence at High Temperatures, 693.
Nitrates, effect on reducing power (Glaser), 272.
Noguchi, H. Leptospira Icteroides and Yellow Fever, 110.
Noyes, A. A., and MacInnes, D. A. The Ionization and Acitivity of Largely Ionized Substances, 18.
Nuclear activity (Tennent), 217.

Organic systems, rhythm in (Lotka), 410.
Osborn, H. F., and Mook, C. C. Reconstruction of the Skeleton of the Sauropod Dinosaur Camarasaurus Cope (Morosaurus Marsh), 15.
Osmose, anomalous (Bartell), 306.
Osmosis in solutions (Loeb), 211.

Page, L. A Kinematical Interpretation of Electromagnetism, 115.
Patterson, R. A., see Duane, W., 509, 518.
Pearl, R. A Contribution of Genetics to the Practical Breeding of Dairy Cattle, 225.
——. On a Single Numerical Index of the Age Distribution of a Population, 427.
Pearl, R., and Reed, L. J. On the Rate of Growth of the Population of the United States since 1790 and Its Mathematical Representation, 275.
Persistence of vision (Hardy), 221.
Phillips, H. B., see Moore, C. L. E., 135.
Photoreception (Hecht), 112.
Platinum black, heat production in (Loeb), 107.
Platinum, critical potentials (Webster), 26.
Poliomyelitis (Flexner), 103.
Population growth (Pearl and Reed), 275.
Population, age distribution in (Pearl), 427.
Potentials, critical, of platinum and tungsten (Hoyt), 639.
Pressure, effect on resistance (Bridgman), 505.
Protein requirement for man (Sherman), 38.
Products in geometry (Moore and Phillips), 135.
Pterergates in Pogonomyrmex (Shapley), 687.

Radiation from Sun (Abbot), 4, 82.
Radioactive radiations (Kovarik), 105.
Reed, H. S. The Dynamics of a Fluctuating Growth Rate, 397.
Reed, L. J., see Pearl, R., 275.
Regulation, functional (Detwiler), 695.
Reid, H. F., see Cross, W., 706.
Relationship and geographical distribution (Metcalf), 432.
Relativity (Eisenhart), 679.
Report of the Autumn Meeting of the Academy, 43.
Report of the Annual Meeting of the Academy, 442.
Resistance, effect of pressure on (Bridgman), 505.
Retinal adaptation (Hecht), 112.
Rettger, L. F., see Cheplin, H. A., 423, 704.
Rhythm in organic systems (Lotka), 410.
Riffolt, N., see Thompson, L. T. E., 169.
Ringer, M., see Underhill, F. P., 692.
Rockets in weather forecasting (Goddard), 493.

Schoenflies' theorem (Kline), 529.
Sea-level sinking (Daly), 246.
Seismology (Cross *et al.*), 706.
Seminvariants of differential equations (Stouffer), 645.
Sex-ratio in man (Little), 250.
Shapley, H. Note on a Simple Device for Increasing the Photographic Power of Large Telescopes, 127.
——. Thermokinetics of Liometopum Apiculatum Mayr, 204.
——. Studies of Magnitudes in Star Clusters, XI. Frequency Curves of the Absolute Magnitude and Color Index for 1152 Giant Stars, 293.
——. Preliminary Report on Pterergates in Pogonomyrmex Californicus, 687.
Shapley, H., and Davis, H. N. Studies of Magnitude in Star Clusters, XII. Summary of a photometric investigation of the globular system, Messier 3, 486.
Sheldon, H. H. Charocal Activation, 178.
Sherman, H. C. The Protein Requirement of Maintenance in Man, 38.
Spectroscopy with tube resistance furnace (King), 701.
Spectra of explosions (Anderson), 42.
Ship, starting of (Whittemore), 182.
Siouans, anthropometry of, 131
Smith, E. A., see Guyer, M. F., 135.
Social insects (Shapley), 687.

Solar constant (Abbot), 4.
Solar radiation (Abbot), 82.
Solubility of globulin (Cohn), 256.
Solutions, osmotic pressure in (Loeb), 211.
Solutions, ionization in (Noyes and MacInnes), 18.
Solutions, osmotic pressure (Bartell), 306.
Solutions, thermodynamics of (Hitchcock), 186.
Sosman, R. B. An Outline of Geophysical-Chemical Problems, 592.
Sound, location of (Stewart), 166.
Sound, velocity of (Foley), 310.
Specific heat of gases (Webster), 303.
Spectra, absorption, in electric furnace (King), 63.
Spectrum in X-Rays from platinum (Webster), 26.
Spinden, H. J. Central American Calendars and the Gregorian Day, 56.
Star clusters (Shapley), 293.
Star clusters, magnitudes in (Shapley and Davis), 486.
Statistical methods (Linhart), 682.
Stebbins, J., see Campbell, W. W., 349.
Stenström, W., see Duane, W., 477, 607.
Stewart, G. W. The Functions of Intensity and Phase in the Binaural Location of Pure Tones, 166.
Stouffer, E. B. Seminvariants of a General System of Linear Homogeneous Differential Equations, 645.
Sturtevant, A. H., see Morgan, T. H., 162.
Sullivan, L. R. Anthropometry of the Siouan Tribes, 131.
Sun, variability of (Abbot), 674.

Telescopes, increasing photographic power (Shapley), 127.
Temperature of dry cows (Kriss), 539.
Tennent, D. H. Evidence of the Nature of Nuclear Activity, 217.
Tennessee, fossil plants from (Berry), 333.
Temperature effects on ants (Shapley), 204.
Thermodynamics of solutions (Hitchcock), 186.
Thompson, L. T. E., Hickman, C. N., and Riffolt, N. The Measurement of Small Time Intervals and Some Applications, Principally Ballistic, 169.
Thomson effect (Hall), 139, 613.
Time, measurement of (Thompson *et al.*), 169.
Torsional magnetism (Barus), 59.
Transformations, commutative (Lunn), 24.
Transformations, groups of (Zeldin), 541.
Transplating limbs in animals (Detwiler), 695.

Underhill, F. P., Honeij, J. A., and Bogert, J. Calcium and Magnesium Metabolism in Certain Diseases, 79.
Underhill, F. P., Ringer, M., and Wilkins, L. Blood Concentration Changes in Influenza, 692.

Vandiver, H. S. On Kummer's Memoir of 1857 Concerning Fermat's Last Theorem, 266.
——. On the Class Number of the Field $\Omega(e^{2i\pi/p^n})$ and the Second Case of Fermat's Last Theorem, 416.
Velocity of light (Michelson), 476.
Velocity of sound (Foley), 310.
Vibration of air in horns (Webster), 316.
Viscosity and elasticity (Michelson), 122.
Viscosity, torsional (Barus), 1.
Vision, persistence of (Hardy), 221.
Vitamine, antineuritic (Dutcher), 10.
Vitamines, isolation of (Myers and Voegtlin), 3.
Voegtlin, C., see Myers, C. N., 3.
Volcanology (Cross *et al.*), 706.

Washington, H. S. The Problems of Volcanology, 583.
Water ram in pipe lines (Durand), 441.
Weather Bureau, Hawaii (Cross *et al.*), 706.
Weather forecasting (Goddard), 493.
Webster, A. G. On the Springfield Rifle and the Leduc Formula, 289.
——. On the Connection of the Specific Heats with the Equation of State of a Gas, 303.
——. Experiments on the Vibration of Air in Conical Horns, 316.
——. On a Condition for Helmholtz's Equation Similar to Lamé's, 605.
——. Some New Methods in Interior Ballistics, 648.
Webster, D. L. The Intensities of X-Rays of the L Series, II. The Critical Potentials of the Platinum Series, 26.
——. An Improved Form of High Tension D. C. Apparatus, 269.
Weinstein, A. Homologous Genes and Linear Linkage in Drosophila Virilis, 625.
Whittemore, J. K. The Starting of a Ship, 182.
Wilbur, D. T., see Nichols, E. L., 693.
Wilkins, L., see Underhill, F. P., 692.
Wright, S. The Relative Importance of Heredity and Environment in Determining the Piebald Pattern of Guinea-Pigs, 320.

X-radiation (Kovarik), 105.
X-rays, characteristic absorption (Duane and Patterson), 509.
X-rays in cancer (Murphy), 35.
X-rays, K series (Duane and Stenström), 477.
X-rays, intensities of (Webster), 26.
X-rays, platinum and tungsten (Hoyt), 639.
X-ray spectra (Duane and Patterson), 518.

Yellow fever (Noguchi), 110.

Zeeman effect (King), 65.
Zeldin, S. D. On the Structure of Finite Continuous Groups with Exceptional Transformations, 541.

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CONTENTS

Page

ZOOLOGY.—PRELIMINARY REPORT ON PTERERGATES IN *Pogonomyrmex Californicus* *By H. Shapley* 687

CHEMISTRY.—HYDROGEN ION CONCENTRATION OF THE CONTENTS OF THE SMALL INTESTINE . . . . *By J. F. McClendon* 690

PATHOLOGY.—BLOOD CONCENTRATION CHANGES IN INFLUENZA. *By F. P. Underhill and Michael Ringer* 692

PHYSICS.—LUMINESCENCE AT HIGH TEMPERATURES . . . . *By E. L. Nichols and D. T. Wilbur* 693

ZOOLOGY.—FUNCTIONAL REGULATIONS IN ANIMALS WITH COMPOSITE SPINAL CORDS . . . . *By S. R. Detwiler* 695

PHYSICS.—EXPERIMENTS WITH THE TUBE RESISTANCE FURNACE ON THE EFFECT OF POTENTIAL DIFFERENCE . . . . *By A. S. King* 701

MATHEMATICS.—AN APPLICATION OF THE PORISM OF FOUR TANGENTS OF A TWISTED CUBIC . . . . *By J. H. McDonald* 702

ZOOLOGY.—STUDIES ON THE TRANSFORMATION OF THE INTESTINAL FLORA, WITH SPECIAL REFERENCE TO THE IMPLANTATION OF *Bacillus Acidophilus*. II. FEEDING EXPERIMENTS ON MAN . . *By H. A. Cheplin and L. F. Rettger* 704

GEOLOGY.—THE KILAUEA VOLCANO OBSERVATORY 706

INDEX 717

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